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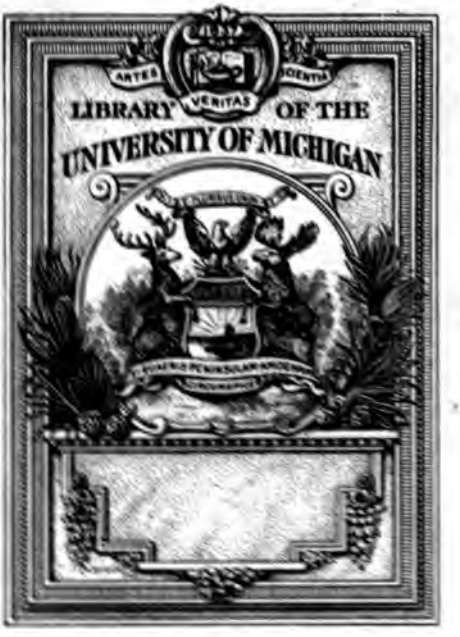
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THE

# RETROSPECT

OF

# PRACTICAL MEDICINE AND SURGERY,

BEING

## A Half-Yearly Journal,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M. D.,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

J. BRAITHWAITE, M. B.,

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In this Volume the reader will find a large amount of information which we have collected from various sources within the last six months. We have endeavoured to take the "cream off the bowl," and to throw the old milk away. At the same time we ought to say, that we have been obliged to reject numerous papers of the highest value, but which we did not consider sufficiently practical for our purpose. Passing over, as almost too numerous to notice in this introductory summary, the numerous and highly interesting papers in the body of the following pages on the various subjects in Medicine and Surgery, we have to point out here, many interesting facts and suggestions which have been published within the last six months, on "*Midwifery, and the Diseases of Women.*"

Few special branches of our Profession now engross more of the attention of Medical practitioners than does this subject, and when we include the Diseases peculiar to Children, this branch of Medicine, becomes still more comprehensive, and requires still more industry and reading to keep up with the literature of the day. For these reasons we have for some time reprinted, from each of our larger volumes, a pamphlet which the Medical practitioner might easily carry in his pocket, and read at the bed-side, whilst passing some of those wearisome hours which his irksome duties call upon him to spend.

In this little pamphlet the Obstetric practitioner will find most of the facts and ideas which have been published in various papers during the last six months, on the subject of Midwifery and the Diseases of Women. It will thus save him immense trouble and labour in referring to all the published literature of Great Britain, Ireland, and America, and enable him in an hour or two, to make himself master of things which have taken us many weeks, and even months, to collect and condense. Amongst other things, we will first direct the reader's attention to a new pair of Forceps, in great use in America, and especially by Dr. Knight, of Baltimore. We have long thought that the forceps required improvement—that they might be made smaller and fit the head better, thus running less risk of lacerating the perineum. The forceps of Dr. Knight, described in the first article on Midwifery, seem to us to deserve the attention of the British practitioner. They are smaller, more easily applied, fit the head better, have no fenestra, and possess other advantages which Dr. Knight describes.

It often happens that the child is low down in the pelvis and all ready for delivery, but the pains abate, the powers of the mother give way, and the practitioner feels convinced that if his fingers were a little longer and a little stronger, he could easily deliver. We want, then, a pair of very small forceps, so as to be useful at the outlet of the pelvis, to fit the head better, and to be able to introduce the upper handle without bringing the patient to the edge of the bed. This last can only be accomplished by making the handle of the upper blade with a *hinge* in it, so as to bend at any angle during its introduction, and keep the handle from the bed; or to make this upper blade in two parts, the lower part or handle to be fixed to the blade when this has been introduced. The parts to be well secured when the for-

ceps have fitted well. The simple introduction of such a pair of blades, with very little traction, would often save the patient and the practitioner hours of pain and suspense. These remarks will apply not only to Dr. Knight's forceps, but also to a good paper by Dr. Doig, of Louth, who has compared the uses of the Forceps and Perforater. Why should we sit days and nights by the bed-side doing nothing, when, by the judicious use of the forceps, we might end the business at once, with safety to both mother and child?

Dr. Braxton Hicks suggests that the old method of Turning the child in the womb may frequently be improved. He describes his method in the following pages: but we confess that we cannot agree with his practice, as there are few or no cases, in which turning is required, where it cannot be done with perfect ease in the usual way, except in cases of impaction low down in the pelvis; and in these last-mentioned cases it can be accomplished with comparative ease, even when the head of the child is very low down indeed, by getting hold of one leg and securing it by means of a bit of strong tape, or the end of a pocket handkerchief. By this contrivance we have great power of traction with the left hand, and can at the same time push up the head with the fingers of the right hand, so that there are no cases which require turning which we cannot treat with safety and ease. Why, then, have recourse to this external manipulation so well described by Dr. Hicks? Let the reader, however, peruse the paper, and judge for himself. There are some good ideas in it which may require to be adverted to hereafter.

It is becoming a more and more important opinion, first brought prominently before us by our eminent friend, Dr. Simpson, and mentioned at great length in one of our former volumes with several wood-cut illustrations, that there are numerous cases of Deformity of the Pelvis, or unusual size of the child's head (in which hitherto labour has been obliged to be accomplished by instrumental aid, either by the forceps or by craniotomy,) which can be terminated safely both to mother and child by means of turning. Dr. Robert Barnes's paper in this volume illustrates the truth of this opinion well, and the operation of turning in the early stages of labour, when the os uteri is well dilated or very dilatable, is really so easy, especially since we can dilate the part still more by the use of chloroform, that we shall not be sorry to see this practice still more carried out in cases of difficulty and tardiness, where either mother or child are in danger. Dr. Barnes truly says, that in certain cases the forceps may be used with great success, but in other cases "this instrument will not effect delivery, obstructed through disproportion, with less risk to the mother or with more safety to the child than can be attained by turning." By getting hold of the leg or legs and turning, we have in fact a degree of power given to us which we can exercise with great safety and with wonderful success.

Puerperal Convulsions are often of the most alarming description. We are often unable to administer any remedy by the mouth; a single drop of croton oil on the tongue moistened with a little water is, perhaps, the only *thing* we can do when the convulsions are incessant. Dr. Scanzoni, of Wurtzburg, suggests that we should inject subcutaneously a solution of meconate of morphia, in the manner lately recommended by Dr. Wood, of Edinburgh, in

other cases. Of all the preparations of opium we have found none so good as the *Meconate of Morphia*. Practitioners will find it to be followed by very few of the bad effects of opium. At the same time we may here say that, next to the delivery of the women, we have found no remedy to exceed in value the injection of an enema composed of half an ounce of spirit of turpentine, well mixed with the yolk of an egg and thin water gruel; this to be repeated, if necessary, every two, three, or four hours. We have repeatedly known this instantly to stop the most violent convulsions. We even venture to think that, in most convulsions dependent on some *eccentric* cause (either in the abdomen or pelvis), whether in women or children, this remedy is superior to all others, especially in children when the convulsions are owing to some intestinal cause. In this case a teaspoonful or small dessert-spoonful of turpentine will be enough.

Mr. Spencer Wells records some good cases of Ovariotomy, and warns us against the too common exhibition of opium in the after-treatment. He says that opium and not the operation itself, is sometimes the cause of death; and in other cases *stimulants*, too incautiously given have done as much harm as opium. At the same time starvation and purgatives may be equally objectionable. One great secret of success in the after-treatment of these cases is *perfect rest*, in a well-ventilated room.

An interesting question has arisen lately, respecting the Diagnosis of those cases which have been called Pregnancy, in which the fœtus has seemed to be absorbed, or in some way to disappear. Dr. James Jones, of the Metropolitan Free Hospital, gives us a case of this kind. The menses disappeared—the movements of the child were evident—the abdomen enlarged and the breast increased—and there were all the most important symptoms of pregnancy. In nine months labour seemed to commence, but disappeared; and in eighteen months the abdomen had lessened and was occupied by a firm mass, more like a fibrous tumour than a child. Such, in fact, would naturally be the opinion of most practitioners when the diagnosis could be thus apparently verified after so long a period had elapsed after parturition was due. But we do not take this view of such a case. We have ourselves lately had a case very similar to the above, and which has ended in the same fibrous enlargement of the womb. But in our case this additional symptom of pregnancy was present—the fœtal heart could be distinctly heard, both by the senior and junior editor of this work, and not once or twice, but repeatedly: this fœtal pulsation was too well-marked to be mistaken—and yet this patient had all the most important symptoms of pregnancy with the most energetic movements of the child. All this has now disappeared, the patient is much improved in health, the abdomen has coniderably subsided, leaving a hard fibrous enlargement of the womb, but no labour has come on, and no child has been expelled. One curious feature of this case was, that we could never find the os uteri, which seemed completely occluded. As far as the finger could reach in all directions, especially backwards, where we might have expected to find it, we carefully sought for this orifice, but never found it, and up to our latest examination we failed to do so.

With this case in our own practice we were the more interested with the case of Dr. E. J. Fountain, of Davenport, Iowa, which resembles our own in *many particulars*, except that he had the advantage, by the ultimate expulsion

of the fœtus itself, of proving the correctness of his own diagnosis. The case will be seen related at page 267 of this volume. The woman became pregnant about June, and continued to enlarge till December. From this time the enlargement ceased to increase, and gradually decreased. The movements of the child were evident, and the usual symptoms of pregnancy present—yet at the end of eleven months no labour had come on, and Dr. Fountain thought fit to bring it on artificially, when there was expelled "the remains of a fœtus, enveloped in its membranes unbroken, with the placenta attached," and not larger than a good-sized orange. It was greatly compressed and emaciated, the flesh had been almost entirely absorbed, leaving little more than the skeleton partially covered with a thin remnant of muscular tissue. From these cases, therefore, it seems possible for a woman to be pregnant, and eventually for the fœtus to be more or less, if not entirely absorbed, or to disappear so far as not to interfere with the functions of health.

Mr. Erichsen has given us some valuable hints about Tumours of the Breast, particularly those small cystic tumours, with a considerable degree of hardness, which are not unfrequently mistaken for cancer by the most experienced surgeons. The great thing to observe is to try to find out whether there is a degree of *elasticity* about them, or whether they are *obscurely elastic on deep pressure*. If so, introduce an exploring *trocar (not a grooved needle.)* Even in cancer this trocar can do no harm, and if it be a cystic tumour the fluid will ooze out and settle the diagnosis. The trocar must be pushed *directly* into the tumour and not sideways, else the skin will form a valve, and only blood will escape, thus confusing the diagnosis. The trocar alone will often cure the cyst; if not, you may inject it with iodine, or cut a bit of the cyst out, or introduce a seton. All these things are well known and often practised, but we are surprised not to find mentioned what we have before recommended in similar circumstances, viz.:—the introduction of a good large silver probe, well charged with nitrate of silver, which we have found most useful. The art consists in changing the probe, the principle of which may be carried out either on small probes for cysts in the eyelid, or on larger probes for cysts any where else. You may either melt a little nitrate of silver in a silver spoon and dip the probe in the fluid, coating the end with it *thick* or *thin;* or you may make the probe itself hot (if in a hurry) and it will adhere at once to the required size of the nitrate, which can be warmed and made to fit the probe better by being applied near a gas-light. In treating cysts, then, make a puncture with a bistoury (and this can be done directly or valvularly), and after evacuating it, immediately introduce the charged probe and *poke* it about the cavity. Cystic tumours of the breast are sometimes so mixed up or surrounded by condensed fibrous matter, that the diagnosis may be very obscure; they may even degenerate into cancer.

In the diseases of women, let us always remember that there is the greatest connection or sympathy between the symptoms of disease in the Rectum and disease in the Womb. You may at first think that certain symptoms are uterine when they are owing to disease in the rectum; this can easily be *shown by an examination. When*, therefore, you cannot find sufficient evi*dence of disease in the womb itself* to account for certain symptoms, examine

the rectum. Mr. I. Baker Brown, with his usual practical skill, has pointed this out clearly. In many cases there will be found a *fissure* just within the sphincter, (the same as in males), and *opposite* this fissure a *small pendulous polypoid body*, which will often drop into the fissure like a pea. In this case divide the fissure by Copeland's blunt-pointed straight bistoury, tie or strangulate the polypus, or twist it off, and *plug the rectum well with lint soaked in sweet oil.* Do not disturb the plug for forty-eight hours, after which time there need be no fear of union by the first intention. Let us advise our younger brethren to be attentive to such apparently *little* things, which are daily occurring in practice, while the more important operations happen only a few times in one's life.

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SYNOPSIS.

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# PRACTICAL MEDICINE.

&c., &c.

## DISEASES AFFECTING THE SYSTEM GENERALLY.

### ART. 1.—ON TYPHUS AND TYPHOID FEVER.

By Dr. HENRY KENNEDY, Dublin.

Dr. Henry Kennedy, of Dublin, in a paper published in the "Edinburgh Medical Journal," lays down the following propositions as the result of his consideration of the relationship between the various types of fever.

"1. Typhus and typhoid fevers exist in Paris, London, Sweden, parts of America, and Dublin; and relapsing fever in Great Britain and Ireland. 2. In Dublin, other types of fever exist, equally distinct from any of these; of which gastric, remarkable for its great duration, the congestive typhus of Armstrong, the *febris nervosa* of Huxham, and in summer, the inflammatory fevers may be adduced as examples. 3. These may exist in the same family, and at the same time. 4. When a whole family is attacked at once, some may exhibit spots, and others not. 5. Two crops of eruption, as observed long since by Grant, are not uncommon in the typhus of Dublin; and either may precede the other. 6. One of these may be a bright red, and the other of a much darker hue; and they often coexist. 7. Petechiæ may exist with typhoid fever; and bright lenticular spots without this fever. 8. Bright lenticular spots may be followed by petechiæ. 9. It would seem as if typhus and typhoid fever could exist in the same patient, and at the same time. 10. Whilst in London intestinal hemorrhage is common in typhoid fever, it is much rarer in Paris and Dublin. 11. In Dublin the same hemorrhage is not uncommon in typhus. 12. Cerebral complication is more common to typhoid fever than is usually taught."

Dr. Kennedy has been led to believe, contrary to the opinion of Dr. Jenner and other pathologists, that typhus and typhoid fevers are the result of a common poison. The variation in the types he is disposed to attribute especially to the influence of temperament, of habits, of constitution, and of the amount of the poison taken into the system. To refute the idea of the less contagious nature of typhoid than of typhus, he refers to the papers of Dr. William Budd, which tend to show the contagious nature of typhoid fever. As to their non-occurrence in the same locality, an answer is found in the work of Dr. Huss; who states that the two forms often arose under the same circumstances, and in the same house—an intermediate form being sometimes developed.

In speaking of the treatment of typhoid fever, Dr. Kennedy states that the practice in Dublin is to give stimulants, not as a matter of course, but because the particular case requires it. He believes that stimulants are used too indiscriminately.

"It seems," he says, "to be a very general impression, that if stimulants are to be used, it matters little of what kind they are. Hence brandy, wine, beef-tea, etc., are constantly spoken of as being given to the same patient. This is surely not the proper way of dealing with such a matter. With the knowledge we now have of the varied composition of these fluids—for the most of them are fluids—it appears to me to be injudicious to use them as is done at present; and I would particularly notice here the difference between wine and beef-tea. For a long period I have been convinced in my own mind that these two cannot be used indiscriminately; for it has been matter of observation to me that their effects are very different. Of the two, and

contrary to what is usually thought, wine may be given with much less risk of unpleasant or injurious effects following, than beef-tea; and when the latter is given indiscreetly, either as to time or quantity, it is very apt to be followed by secondary inflammations, which oftencause fever cases to be so precarious in their results. It appears to me that the differences spoken of as resulting from the use of the two fluids are well accounted for by a consideration of the difference of their composition; the amount of nitrogenous material—chiefly in the form of fibrine—being so very much greater in the beef-tea than in the wine. On the other hand, the latest, and, as I believe, the best analysis of wine, that by Mulder, shows us that it contains, but only in the very smallest quantities, several of those matters which are essential to our frames; such as albumen, iron, etc. And this analysis bears out, curiously enough, the remark of two of our ablest physicians on fever—Drs. Cheyne and Barker—made above forty years since; viz., that, after a trial of whiskey, which was used for the sake of lessening expense, they found that it would not supply the place of wine. The point brought under notice, and which I am not aware has been specially alluded to by any writer, would seem to be one of very considerable moment, as bearing directly on the treatment of not only fever, but of other acute diseases. Enough, however, has been advanced to call attention to the subject, and show the discrimination that is to be exercised. Cases, of course, will occur where both classes of stimulants may be used at the same time, and with the greatest advantage. But the point is, that they are not necessarily to be so used, and that mischief may result if a proper judgment be not exercised."

Of carbonate of ammonia, he says that he does not think it suitable for the treatment of fever; nor has he for many years used it in this way.

"I had observed that diarrhœa was exceedingly apt to follow its use. In cases of typhoid fever this would not have been so remarkable; for diarrhœa is there a symptom of the complaint. But it also occurred in cases where there was no irritation in the intestinal canal; as, for instance, erysipelas, pneumonia, and other chest affections. In this way my attention was drawn to the effect of the medicine; and I have now little or no doubt that very great caution should be used when ordering it in fevers, more particularly those of the typhoid type. My own conviction on this point is so strong, that I would not give it at all; and the effects of the medicine on the blood, as shown in Dr. Richardson's very able work, go far to confirm the opinion advanced."

Dr. Kennedy's treatment of typhoid fever is the following. If there be pain in the right iliac fossa, a few leeches are applied—care being taken that they do not bleed too long; if they be not required, a blister is applied, and after it a warm poultice. As internal medicine, he gives dilute sulphuric acid; prescribing from one to three drachms in an eight ounce mixture with from two to five drops of laudanum in each ounce; an ounce is given at intervals suited to the urgency of the case. In its administration, however, care must be taken that it do not *suddenly* check the diarrhœa. In cases where the diarrhœa suddenly ceases spontaneously, its cessation being attended by dangerous chest or head symptoms, Dr. Kennedy has adopted, he believes with benefit, the plan of keeping open a blistered surface for some days, generally on the chest.—*British Medical Journal, Sept.* 15, 1860, *p* 726.

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## 2.—ON THE DISTINCTIONS BETWEEN TYPHUS AND ENTERIC FEVER.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary of Edinburgh.

[The cases mentioned in this paper are four cases of enteric fever which occurred in the fever ward of the hospital a little before this lecture was *delivered by Dr. Gairdner.*]

*Observe, now, that judging* from our cases, the characteristic phenomena *of this fever are not to* be found either in its duration, or in its severity, or

in its so-called "typhoid" characters. The presence or absence of delirium, or of stupor, or of dry tongue, affords no ground of distinction. One of our cases had severe and protracted delirium, approaching coma; another had no delirium, but was very deaf and apathetic, with a foul and dry tongue for many days; the other two had hardly any "typhoid" symptoms at all. The heat of skin was equally variable; in one it was scarcely ever much elevated; in all the others it varied greatly at different periods, as also did the pulse. In not one of the cases was there a rapid and decided crisis; but one began to amend about the eleventh day, another about the twelfth day, while the two remaining cases were excessively protracted, and can hardly be said even now, after nearly seven weeks, to be out of danger; the fever having in one of the two degenerated into a kind of hectic type, exceedingly like that of subacute phthisis—a consequence, let me remark, which is very much more to be dreaded in enteric fever than in any other form of specific fever with which I am acquainted.

Nothing, then, can well be more variable or less characteristic than the general symptoms of this fever. I have seen it resolve itself in ten days, with the symptoms of febricula only, or of a mild remittent fever; I have seen it, on the other hand, last nearly as many weeks, and pass imperceptibly into organic disease. It mimics in turn not only all other fevers, but many other general or local diseases—phthisis, pneumonia, meningitis, perhaps more frequently than most others.

Next, as regards complications, we find that in two of our four cases there was intense deafness; in one, if not in both, with disorganization of the ears. In the same two there was pulmonary complication of considerable intensity, and doubtful issue; in one of them resembling broncho-pneumonia, in the other tubercular disease. The other two cases were almost perfectly free from pulmonary or other severe complication, and but for a little diarrhœa at the commencement might have been said to be free from complication altogether.

But *there was diarrhœa, more or less, in all the four cases.* This, I need hardly say, is the particular symptom from which the name "enteric," as applied to this fever, is derived. Diarrhœa, without known cause, with light-colored ochrey, or blood-tinged stools, and with a tympanitic state of the abdomen, gurgling on movement, or tenderness on pressure in the right iliac fossa, are symptoms as nearly characteristic of this fever as any that can be named. And yet I would not have you to trust to these symptoms too much, for some of them are absent in the majority of cases; and even diarrhœa may be very transitory, as it was in two, at least, of our cases (not lasting beyond a few days, or even hours); or it may appear to have been determined by a purgative. Further, there may be no diarrhœa; and even in fatal cases, with great ulceration of the intestines, diarrhœa may not appear till a very late period. Besides, in typhus fever, and, indeed, in all fevers whatever, diarrhœa, as an incidental symptom, is by no means uncommon.

Now observe, further, that *in all our four cases there was a peculiar eruption on the skin.* This, when present, and in sufficient amount to be identified, is the great criterion of enteric fever, as the eruption of typhus is also of that disease. I will not dwell on the characters of these eruptions here in the class-room. I will ask you to study them carefully in the wards whenever you have opportunities, and particularly to contrast them with one another. Of the maculated eruption of typhus, you have two excellent examples under observation at present. I will only recall to your attention now the fact, fully demonstrated to you before on many occasions, that in most of our cases of enteric fever the eruption has been very scanty, and that we have not unfrequently hesitated for a day or two to make a diagnosis. Indeed, the first crop of the eruption is rarely quite decisive; but as soon as successive crops, even of two or three spots each, appear, all doubt is removed. But in order to be sure, it is usually requisite in this disease, when the eruption is scanty, to mark each individual spot with ink in such a way as to distinguish accurately the period of its appearance; which pre-

caution against error, accordingly, you saw me take in all these four cases. This in typhus fever it is usually unnecessary to do, and from the number of the spots it would be impossible even if it were necessary.

As regards the characters of these eruptions, I may refer you for details to the excellent descriptions and illustrations of Dr. Jenner in the thirty-third volume of the 'Medico-Chirugical Transactions,' with which I entirely concur.

As regards the characteristic phenomena of typhus, and particularly the relation of the eruption to these phenomena as we have lately observed them in Edinburgh, I have no time to enlarge at present, and I will therefore take the liberty of referring you to a short paper of mine in the number for July, 1859, of the 'Edinburgh Medical Journal,' on the changes that have occurred in our fevers during the last ten or twelve years. You will there find that typhus, like enteric fever, (though not certainly to the same degree,) is a disease of exceedingly variable severity and duration; that its general febrile characters, though following on the whole a certain type, are by no means to be depended on in individual cases; that its complications are few, and follow no regular rule; and that therefore, still more decidedly than in enteric fever, we have usually to fall back on the eruption for its diagnosis.

You will observe, then, that in the diagnosis of enteric fever, as compared with our old and familiar (but, happily, not *of late* very familiar) typhus, I rely not so much on any of the characters of the fever itself, though these are often very peculiar and distinctive when taken all together; but chiefly, and in some cases exclusively, on the eruption. And the eruption I believe to be characteristic thus far, that when sufficiently abundant and well marked, it shows forth quite decidedly which of the two diseases is present. When scanty in amount, and present only for a short time, it sometimes leaves us in doubt, not only whether the fever present is typhus or enteric, but whether it is an eruptive or specific fever at all. Thus you saw that in one of our four cases, I remained in suspense for two or three days, but was afterwards quite decidedly enabled to pronounce that the eruption was that of enteric fever. And as regards typhus, some of you may have observed that, in the two cases now in the ward, I did not for a single instant admit of a doubt. The eruption, being a well-marked one, was, to my mind, a conclusive evidence of the nature of the disease, without a single inquiry as to the symptoms or antecedents of the cases. The same perfectly distinctive character of the eruption, at the first visit, has often occurred to me in the case of enteric fever.

Now I presume you will ask, and you certainly will do well to ask, what proof can be presented that the two eruptions alluded to (admitting them, as I think you will do, to be distinguishable from each other in the great majority of cases) indicate two diseases, and not mere accidental varieties of one disease? I have already fully confessed to you that, unless the eruption makes the distinction, there is no sufficient and constant distinction in the symptoms ascertainable during life. Of course there is the additional *fact* of the ulceration of the Peyerian patches—by far the most noteworthy single fact, pathologically speaking, in the natural history of enteric fever. But it will not do to found on a fact like this in the matter of diagnosis; and, besides, after you have settled that there are cases of fever with ulcerated intestines, you have still to settle whether these cases are different in nature from other cases, in many respects similar, in which the intestines are not ulcerated.

[In the summer of 1859, Dr. Gairdner made a very careful survey of the whole "fever field" of Edinburgh, but without any result giving rise to a suspicion that typhus had given rise to anything but typhus, or enteric fever to anything but enteric fever.]

I beg you to notice very particularly these facts; for not only are they *full of* instruction as to the danger of associating typhus cases with enteric *fever, or indeed with* any other fever, in the same ward, but they form the *most conclusive of* all possible proofs that the two diseases are distinct dis*eases. Typhus fever is* not subject to a relapse; and it is a curious and in-

disputable law with respect to it, that it almost never attacks again, at least within a period of years, those who have had it before. Yet here we have three members of one family, within a few weeks, seized with two distinct febrile attacks, with two distinct eruptions, the attacks being separated the one from the other by a perfectly distinct convalescence. To make these out to be mere varieties of typhus fever would require the whole laws of that fever as ascertained by innumerable observations here and elsewhere, to be set at nought in this particular instance.—*Lancet, July* 21, 1860, *p.* 53.

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## 3.—ON THE TREATMENT OF TYPHUS FEVER.

By Dr. Alexander Tweedie, F. R. S., Physician to the London Fever Hospital.

In the progress of typhus, generally in the second week, but sometimes earlier, symptoms of exhaustion become evident, and indicate the necessity for stimulants; hence wine, or some form of alcoholic fluid, becomes one of the most important remedies in the treatment. The precise period, however, when they should be given must be regulated, not so much by the duration or stage of the fever, as by individual circumstances. Symptoms of exhaustion or sudden collapse, for example, may supervene at any period, even at the very commencement, and require stimulants to be at once administered, and in quantities which can only be regulated by their effects. The state of the circulation, as indicated by the pulse at the wrist, but still more surely by the sounds of the heart, is the usual practical index to the determination of the question. In a former lecture I drew your attention to an excellent paper on this subject by Dr. Stokes, in which it is shown that the cardiac sounds form the only certain guide for the proper administration of wine in fever. When, therefore, we find the first or systolic sound so weak and abrupt as scarcely to be distinguished from the second, (in extreme cases it is nearly annihilated,) and the pulse at the wrist beating in sympathy with the heart, soft, compressible, and rapid, there must be no hesitation in at once administering powerful stimulants, the effect on the heart's action being the guide as to the amount of either wine or brandy, while medicinal stimuli—ammonia, chloric ether, or other remedies of this class—are given in the intervals. It is especially necessary in such cases to enjoin that in the night, when the depression is generally the most marked, nourishment, and even wine, be given at proper intervals, and not to admit the too common excuse offered by the nurse for withholding them—that the invalid was in such a sound sleep that it was not deemed prudent to interrupt it. Many a patient has thus been allowed to pass into hopeless collapse, when, in all probability, by constant judicious stimulation, recovery might have taken place.

It is always necessary to watch the effects of the first few doses of wine, and if the pulse abates in frequency, becomes soft and fuller, the tongue moist, and the heat of the skin not increased; and, when there has been delirium, if the patient becomes more calm, and has intervals of sleep, we may feel sure that the wine is doing good. On the other hand, if the pulse increase in frequency and strength, the skin become hotter, and the patient restless, flushed and excited, with throbbing of the temporal and carotid arteries, we may conclude either that wine is not suited to the case, or has been given too early, and should therefore be withdrawn. But, as a general rule, it is perhaps better to give wine a little too early than a little too late, since, if it appear to disagree, it is easy to suspend its use; but it may be very difficult to restore the vital powers if they have been allowed to remain too long unsupported.

Nor should the wine or brandy be discontinued until convalescence is fairly established; but as the symptoms for which the stimulants have been prescribed disappear, the quantity should be gradually abridged by giving smaller portions and at more distant intervals.

In regard to the amount of wine and alcoholic stimulants that may be ad-

ministered in typhus, no precise rules can be laid down, as the ever-varying circumstances presented by individual cases can alone determine this. It is prudent to begin with half an ounce or an ounce, and to repeat this amount at longer or shorter intervals, according to the effect produced. From six to twelve ounces may be considered to be an average daily allowance, but sometimes it is necessary to give two or three pints, or even more, in twenty-four hours, and, it is surprising to observe, without the slightest intoxicating effect, even when the patient has been previously unaccustomed to stimulants. Indeed, in low fevers, the exhausted state of the nervous system appears to be an antidote to the effects of stimulants—in short, to create a tolerance of wine and diffusible stimulants.

The wine should always be conjoined with nourishment, in order to assist its due assimilation, though in many cases the digestive powers are so feeble that they are unable to elaborate even the lightest articles of food, and therefore the wine or brandy may be given simply diluted with water.

I have just alluded to the daily quantity of wine that it may be necessary to prescribe in typhus, and stated that no precise rules can be laid down, as the circumstances of each case must determine it. You are doubtless aware that there is a great tendency in the present day to revive the Brownonian system, which flourished for a time in the latter part of the last century, in all acute diseases, including fevers, without regard to individual peculiarities. The doctrine inculcated by some teachers with respect to inflammation is, that this process being a deranged nutrition, involving supply and waste, and the waste being considerable while the inflammatory process lasts, there must be a compensating supply; that as the supplies for the formation of the abnormal products of pus and lymph must be drawn from the blood, or from the tissues, or from both, the vital powers become exhausted, in proportion to the organic disintegration that takes place. Hence it is concluded, that the more the inflammatory process draws upon the blood, the greater will be the exhaustion of vital force, and the consequent effect upon the whole frame.

Upon this physiological theory of the phenomena of inflammation is based the overthrow of established therapeutic principles, on which the treatment has been for ages conducted. But surely even the abettors of this theoretical view must admit that the object of treatment is to anticipate or prevent those so-called destructive processes; in other words, to promote resolution by all available means. Is this to be accomplished by extravagant doses of wine and brandy, regardless of the ever-varying condition of the sufferer or period of the disease?

Similar reasoning is adduced in regard to the phenomena of fevers, whatever be their type or special circumstances. It is against the indiscriminate employment of stimulants in fever that we protest, being convinced that their proper administration requires as much consideration as is generally bestowed on other measures employed as curative agents.

The enormous quantites of wine and brandy recommended in even the early stage of fevers, whatever be the form, the individual circumstances, or whether there be local affections present, have often surprised me, and inclined me to doubt the accuracy of the statements. I have certainly seen intercurrent inflammations materially aggravated by the injudicious stimulation adopted, and on more than one occasion all the ordinary characters of acute delirium tremens supervene when the unlimited administration of brandy had been left to the discretion of a nurse, who fancied that she was only obeying instructions when she poured down dose after dose of pure brandy. There is surely no practical philosophy in such indiscriminate abuse of a really valuable remedy when given on rational principles; and I deem it the duty of every physician who is convinced of the dangerous tendency of the Brownonian doctrine applied indiscriminately in the treatment of diseases, acute as well as chronic, to express his opinion boldly and decidedly, that *the young and inexperienced* practitioner may be warned of the dangerous *consequences of this recently*-revived doctrine.

*In regard to the class of* wines best suited for fevers, claret often answers

well, and may be employed, unless a more powerful stimulant be required. With most patients it suits well to commence with, and mixed with water is a grateful beverage. It has also the advantage of being less stimulating, and more likely to suit in cases in which the employment of wine is doubtful. The stronger wines,—port, sherry, or madeira,—properly diluted, are, however, generally preferred. Brandy is often very useful, especially when a more powerful stimulant is indicated, or when wine is either not relished or disagrees. In hospital practice, for obvious reasons, alcoholic fluids—gin, whiskey, British brandy (which is merely malt spirits disguised by colouring matter)—are generally prescribed.

In the Fever Hospital, Cape wine, of the best quality, and British brandy, are used.

Malt liquors are sometimes employed; but they are suited only to the stage of convalescence. When, however, the patient has a desire for them, from having been previously accustomed to their use as an article of diet, they may be allowed in addition to wine or spirits.

It is necessary, in many cases, to calm the nervous system; and this brings me to the consideration of the exhibition of narcotics, from the judicious employment of which so much benefit is occasionally derived.

Wakefulness, or want of sleep, so common in typhus, requires the employment of opiates; for nothing is more exhausting than two or three nights passed without sleep. Of the different remedies of this class, experience has led me to give the preference to opium, or its alkaloids; but I generally succeed with a much smaller dose than is usually prescribed,—with, perhaps, ten or twelve drops of Battley's laudanum, or of the liquor morphiæ,—repeating the same quantity every two or three hours until sleep is procured. But we ought to proceed on sure grounds, and if there be undue vascular excitement, indicated by headache, acute delirium, flushing and conjunctival injection, the narcotic must be postponed until the symptoms are subdued. In my last lecture I alluded to the beneficial effects of a combination of tartar emetic and laudanum in the cerebral affections of enteric fever, and need not do more, in the present instance than state, that it answers equally well under the same circumstances in typhus, at the same time administering, in the severer forms, which so closely resemble delirium tremens, wine or brandy in such quantities and at such intervals as the case requires.

Though opium generally acts better than any other of the narcotic class, it disagrees with some patients; and when such idiosyncrasy occurs, we must give a trial to some other remedy of the narcotic class, such as henbane, extract of poppy, or Hoffmann's anodyne liquor. Some speak highly in favour of the extract of belladonna as a narcotic when the pupil is so contracted as to be thought to contra-indicate the employment of opium; but I have no experience of it. I have often observed, however, that when patients, or those around them, have intimated that opium disagrees, if given by enema, it acts like a charm in soothing the nervous system and promoting sleep, concealing, of course, that opium is administered. Twenty or twenty-five drops of laudanum, added to three or four ounces of any bland fluid, is often sufficient, and is, in many cases, a preferable mode of administering an opiate.

As to the repetition of opiates, I would observe, that when the purpose for which they have been given has been accomplished, the remedy need not be repeated, unless the same circumstances recur. The remedy, however, should always be at hand to be administered if necessary.

In no disease are the beneficial effects of blisters more satisfactory than in these severe cerebral affections. But we must be careful not to apply them if there be subacute inflammation going on. When this is subdued, the blister may be applied to the forehead and the temples, which is preferable to blistering the nape of the neck, in which situation vesication is often attended with much discomfort from the constant friction to which the surface is exposed.

In the pulmonary affections to which I shortly adverted in a former lecture,—whether they assume the form of bronchitis, pneumonia, or, more rarely, of *pleurisy*,—special local treatment becomes necessary. But it

must be remembered that not only is the local malady itself
thenic character, but the general or constitutional powers are d
consequently, active remedies not only fail in their object,
further to lower the strength. Counter-irritation of the chest
stupes, or sinapisms, or the application of a blister, often giv
lief. The secretion of the pulmonary membrane should at the
promoted by antimony or ipecacuan, while the general powers
by wine and nourishment.

The management of the convalescent stage of typhus is not
though often neglected. Much benefit is derived from variou
the tonic class—quinine especially, or some bitter infusion, s
cascarilla, calumba, or gentian, to either of which the comp
ammonia may be usefully added. The diet should be light a
and suited to the digestive powers of the patient, and it is alw
make careful inquiry into the previous state of the assimilat
order that a suitable dietetic plan may be laid down. No ge
be given as each case must be regulated by its own peculiarit
care be observed in eliciting these, there will be seldom gr
adapting the details.—*Lancet, June* 16, 1860, *p.* 590.

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## 4.—CLINICAL REMARKS ON THE CURE OF TYPH

By Dr. Drinton, Physician to the Royal Free Hospital.

[The use of emetics in the earlier stages of many febrile disor
old date, but had somewhat gone out of use till revived by S
land, in his classical work 'Medical Notes and Reflections.']

In studying the act of vomiting some ten years ago, I mad
ments, (Lancet, 1853, 'On the Treatment of Fever;' and '
Anatomy,' article 'Stomach'), which justified the conclusi
vomiting of certain drugs from the stomach, there was inciden
determination of these drugs to the gastric and adjacent inte
membrane; by virtue of which afflux the vomiting they pro
object and effect of removing them from the system.

Extending this conclusion to the study of typhoid and other f
then and soon after I was treating numerous (about 100) case
epidemic character, I was led to recognize, in the earlier syn
malady, a specific determination of the feverous poison to the
tary tract of mucous membrane; and especially to its upper
sented by the stomach. And as a corollary to this view, it
while to try the effect of agents which, like emetics, seemed,
researches, to determine an increased afflux of blood to this
brane, as well as an increased secretion, and even excretion fr

In plain English, however dangerous the facility of such a n
theory in general, there seemed to be special physiological
validity in this instance. On the other hand, trained by th
large epidemics, I believed myself little likely to mistake for f
cula or slighter malady; and could see no possible harm in re
cially, an act which I had felt, in my own person, gave so
relief to the terrible symptoms which usher in a typhoid fever

Without troubling you with the details by which I had slowly t
self that this measure was really successful, I venture to sum up t

1. Emetics, early in fever, are most advantageous: with
tions, always harmless; in most instances, extremely benef
cases, positively cutting short the malady by a speedy cure.

2. They ought only to be given in an early stage, for whic
erally find an arbitrary limit in the first four days from that of
rigors, but which is practically better defined by the access a
the nausea or vomiting of this epoch.

3. Later than this period, they seem to have much less i

with it for about six minutes. During the last half of this period, the skin was white, hard, and insensible. When this congelation had ceased, a small quantity of pounded ice was placed across the ankles in order to prevent the smarting which would have otherwise accompanied the returning sensation, and the patient was desired to keep the ice applied for a quarter of an hour, or longer than this, if the smarting should return on its removal.

Circumstances prevented my seeing the patient until the third day afterwards, when she expressed in very energetic terms, her thankfulness for the relief which had been afforded. There had been no return of pain after the congelation, and consequently no interruption of her rest at night. All that she now felt was a sense of stiffness of the joints. There had been heat and burning of the skin, particularly on the second day after the frigorific had been used, but this she had quickly removed by sponging the part with iced water. As the congelation had not been kept up so long as it sometimes is, there had been no vesication produced, and consequently, none of that tenderness of the skin which follows vesication. The stiffness and weakness of the joints continued for some time, but not in such a degree as to prevent walking; and had the disease been of shorter duration, these effects of cold would not have existed. On the other hand, when the disease has continued for a very long period, and produced organic change in the joints, though all suffering may be removed, the stiffness will probably be permanent.

The congelation did not last four minutes; for being desirous to avoid injury of the skin, I should have preferred repeating the milder application, to prolonging this annoyance from one of greater duration. In determining the period during which the part should be kept congealed, it is necessary to take into the account the strength of the frigorific mixture employed. A large quantity of well-pounded ice and salt, applied when the materials are acting strongly on each other, will produce a more deeply penetrating and a more lasting cold than a smaller quantity not so well prepared. The circumstance which I have known of healing by the first intention being frustrated after an operation performed under congelation, proceeded from its application too long continued when produced by a very powerful frigorific; and under these circumstances there would probably have been no improvement had an appropriate mode of dressing the wound been adopted.

To the prevalence of two errors must the imperfection in the treatment of rheumatism be chiefly attributed. One of these is the undue influence which certain theories of the nature of the disease have been permitted to exercise; the other is the false view that has been taken of the disposition which rheumatic inflammation has to extend from one part of the fibrous system to another, or of what has been termed metastasis.

The theory of the nature of acute rheumatism at present in vogue is a plausible one, but it ought not to be relied upon to the degree that would render those lessons of experience which are apparently contradictory to it less impressive. That an acid exists abnormally in the blood, there can, reasoning from chemical analysis alone, be little doubt; but whether this acid be the cause of the disease, only one of its numerous effects, or what the importance of [illegible] may be, are points to be yet ascertained. What late Dr. Todd the eliminating mode [illegible] in the administration of large doses [illegible] this theory; but the well-attested [illegible] as the bark, opium, and lemon- [illegible] also, though the supposed [illegible] are in favour of the idea [illegible] influence, the immediate [illegible] ease, gout, after sprains, [illegible] fferent conclusion. One [illegible] rheumatic fever, immediately [illegible] severest attacks of gout [illegible] ure of stricture of the

## 6.—ON THE TREATMENT OF CHRONIC RHEUMATIC DISEASES BY CONGELATION.

By Dr. James Arnott.

Among the various purposes served by the new remedial agent, congelation, the most striking, if not the most important, is its use in the various forms of chronic rheumatism. I do not bring it forward as a remedy which may occasionally be of service, for this may be affirmed of numerous other plans of treatment, but as one which extensive experience of its use has shown to be capable, in the great majority of instances, of immediately and permanently relieving the pain and subduing the inflammatory condition of the part to which it is applied. Indeed so few have been the cases in which considerable advantage has not been obtained from its use, that I have attributed the apparent failure more to error in the diagnosis or to a complication of rheumatism with other affections, than to the inefficacy of the remedy. It is almost unnecessary to add that this agent can have no influence on any constitutional affection co-existing with that which is local; but the existence of such a general affection is only known by the local symptoms, and when these are permanently removed the disease may be said to be cured.

If this (or indeed any disease) can be cured by the application of local measures, instead of those whose agency pervades the system, the local measures should be preferred. Few internal medicines of an efficient character can be long persisted in without incurring some degree of danger; and, after all, though the whole constitution is pervaded by them, their beneficial action may be limited to the seat of disease. A coroner's inquest was lately held at Chatham to investigate the circumstances of a death caused by an overdose of opium, administered for the cure of rheumatism; and two days ago, I was requested to visit a gentleman dying from an attack of bronchitis which owed its fatal severity to the extreme debility caused by a protracted course of active medicine for sciatica. A vulgar and pernicious prejudice against local or external remedies is that they "drive the disease inwards;" but when internal remedies succeed in "driving it out," they too often cause, or (or as in the instance just mentioned) predispose to, a worse disease in its stead.

Of the *modus operandi* of congelation in chronic rheumatism it may be difficult to afford a satisfactory explanation. Some may deny that there is any new or peculiar action exerted by it, contending that it is only the thorough development of the antiphlogistic and narcotic virtues of cold applications; while others may regard it as only an appropriate mode of producing counter-irritation, or as a combination or succession of these two remedial agencies. A rational explanation of the mode in which a remedy operates is always very desirable, but experience is the only real test of its utility. The following case illustrates the mode of using the remedy, and its usual effects.

A woman, between fifty and sixty years of age, employed as a cook in a gentleman's family, was, after an exposure to cold and dampness, affected with pain, swelling, heat, and slight redness of both ancles. She walked with great difficulty, and her sleep was much disturbed by an increase of pain during the night. There was no fever, nor other symptoms of constitutional disturbance. I saw her nine weeks after the commencement of the disease, and learned that the colchicum, iodine, quinine, and other remedies, which she had taken, had proved of no avail. On the contrary, her sufferings had increased, and it was proposed to send her into the country for change of air. On the 26th of February last, recourse was had to congelation. About three-quarters of a pound of ice, enclosed in a small canvas bag, were, by means of a flat iron, broken into a fine powder, and rapidly mixed with about half their weight of common salt. The mixture was then poured into a piece of gauze, and applied to both sides, successively, of each ankle, while the *foot rested on the edge of* a basin. The gauze bag covered a circular space *of skin of between three and* four inches diameter, and was kept in contact

with it for about six minutes. During the last half of this period, the skin was white, hard, and insensible. When this congelation had ceased, a small quantity of pounded ice was placed across the ankles in order to prevent the smarting which would have otherwise accompanied the returning sensation, and the patient was desired to keep the ice applied for a quarter of an hour, or longer than this, if the smarting should return on its removal.

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The common opinion respecting the metastasis of these diseases is founded partly on imperfect observation and partly on imperfect theory. Inflammation of the heart sometimes precedes inflammation of the joints in rheumatism; and is so common an event during the continuance of the disease, that Dr. Watson has only known two cases of rheumatic fever occurring previously to puberty in which the heart was not affected. That rheumatic inflammation of the heart often takes place about the same time that inflammation ceases in a joint, is indisputable; but this coincidence no more shows the connection of cause and effect, than the occasional coincidence of dreams and events shows that dreams are prophetic. Rheumatic inflammation extends to various parts of the fibrous system, just as common inflammation attacks successively various parts of the respiratory mucous system; and, doubtless, the part last affected may act in some degree as a counter-irritant in removing the inflammation previously existing in other parts. If it be true (and the idea is, to a certain degree, supported by the pathological researches of Dr. Garrod) that the articular inflammation is produced by the deposition of a *materies morbi*, it must follow that, as inflammation so produced would impede this secretion or deposition, whatever is calculated to moderate or remove it, will, instead of repelling the poison into the blood, not only promote its deposition in the joints, but, by preventing a febrile disturbance of the system from the violence of the local affection, facilitate its excretion by the skin and kidneys. If we take it for granted that rheumatism is a blood disease, we must admit, with Dr. Graves, that the poison, in certain favourable conditions of the system, may pass off by the emunctories without exciting local disturbance—just as electricity may pass from the clouds to the earth without injury to buildings on its surface.

Before concluding, I may mention another circumstance calculated to oppose the general introduction of congelation in chronic rheumatism; I allude to the imperfect manner in which it has been too often used for antiphlogistic and other purposes. To this cause principally I would attribute the failures in its use mentioned in a work on rheumatism recently published by Dr. Fuller. He states that, out of five cases of lumbago in which he employed congelation, there were two failures. As this differs widely from my own experience, I can only explain the difference by the supposition that the remedy was applied by him in a different manner; and this opinion is confirmed by finding it stated in his book, that congelation is objectionable from the pain produced by it both during and after the application. When properly used, the smarting or tingling caused by it is too little to be complained of, and will certainly never be deemed by the patient a counterbalance to the immediate and permanent relief of the suffering from the disease. Dr. Fuller thinks that these failures of two out of five cases supported his theory, that rheumatism of every description arises from lactic acid in the blood, which must be neutralised or expelled by internal medicine. But surely, instead of the two failures, it would have been more reasonable to have adduced the three successful cases as a corroboration of this theory. What appears a metastasis or shifting of inflammation, probably arises from the poison (assuming the truth of the theory) being carried by the *vis medicatrix* to a new outlet or excreting texture, when the previously excreting texture or former outlet has been obstructed or disturbed by inflammation set up by the excreted matter; and, consequently, what would continue or restore the excretion in a joint already affected by removing this disturbing cause, or, in other words, by arresting the inflammation, would appear well fitted to prevent its extension to other parts. The truth is, however, that there is too great a dissimilarity between the various forms of acute and chronic rheumatism to authorize reliance on reasoning of this description, far less the substitution of such reasoning for the results of experience.—*Med. Times and Gazette, July* 14, 1860, *p.* 28.

## DISEASES OF THE NERVOUS SYSTEM.

### 7.—ON AFFECTIONS OF THE SPINAL CORD.

By Dr. C. E. Brown-Séquard, F.R.S., Physician to the National Hospital for the Paralysed and the Epileptic, &c.

[Dr. Brown-Séquard has published in the Lancet a series of most interesting lectures on this subject, from which we have endeavoured to collect some of the most practical remarks.]

*On the Diagnosis and Treatment of the Principal Forms of Reflex Paraplegia.*—[The present article treats of reflex paralysis from various causes. A number of cases are first related.]

1. *Cases of Paraplegia due to Disease of the Uterus.*—In 1855, a young lady consulted me for what she called an extreme weakness, which was really a paraplegia, almost complete at each menstrual period. No diminution of any kind of sensibility; no paralysis of the bladder or the rectum; no symptom of hysteria. Dysmenorrhœa, anteflexion of the uterus, which was extremely sensitive, very large and congested, bearing-down pain, &c. In a few days after the use of a bandage to support the womb, a great amelioration was evident; and, in less than two weeks, the paralysis had entirely ceased. It had lasted six months, and had been treated in vain by strychnia, galvanism, shower-baths, steel, and other tonics.

Lisfranc ('Clinique Chirurgicale de la Petié,' vol. ii., p. 199, 1842) mentions the case of a lady, completelely paralysed of the lower limbs, who was vainly treated, by the most active remedies, for a supposed affection of the spinal cord, and who became better only when treated for a chronic metritis. The progress towards a complete cure, which was obtained after a long period, took place gradually, the change in the paraplegia following that of the inflammation of the womb. Lisfranc mentions another case in which also the gradual amelioration of the paraplegia corresponded with that of the disease of the uterus, and a complete cure was obtained.

An excellent observer, Dr. Nonat, has seen seven or eight cases of paraplegia, depending upon an affection of the womb, in which the paralysis was quickly cured after the cure of the disease of the uterus. Those cases show that when the uterine affection is limited to one side the paralysis is also limited to the lower limb on the same side, The cases have been published by a pupil of Dr. Nonat, Mr. Esnault, ('Des Paralysies Symptomatiques de la Métrite et du Phlegmon Utérin,' Paris, 1857.)

Mr. Henry Hunt, of Dartmouth, Professor Romberg, Dr. Wolf, of Bonn, Leroy d'Etiolles, jun., and others, have also mentioned cases of paraplegia, following a disease of the womb, and more or less quickly cured after the cure of this disease.

What is the mode of production of paraplegia in those cases? We cannot admit that paralysis was due to a pressnre upon the nerves of the lower limbs,—at least in most of those instances,—as the enlargement of the organ was not sufficient to produce such an effect. Besides, in those cases sensibility was but slightly or not at all diminished, which excludes the idea that it was chiefly or solely a pressure on the nerves of the lower limbs that caused the paralysis. We must therefore admit that it is either through a peculiar influence upon the spinal cord, or in consequence of some alteration in the blood, that paraplegia takes place in cases of disease of the womb. This last explanation, of which I would not have spoken if it had not been represented to me as the right one, we must reject, because there is no reason whatever why an alteration in the blood should rather produce a paralysis of the lower limbs than of the other parts of the body. We must conclude, then, that it is through a peculiar agency upon the spinal cord that an affection of the womb produces a paraplegia. What this agency is we will try to show in another lecture.

2. *Cases of Paraplegia due to a Disease of the Urethra.*—Graves ('Clinical Lectures on the Practice of Medicine,' 2nd edit., edited by J. M. Neligan,

M.D., 1848, vol. i., p. 554) relates the case of a sailor who was treated by Dr. Hutton, for a stricture of the urethra, due to gonorrhœa. There were chills, dysuria, and loss of power in the lower limbs. "A very remarkable amendment took place in his back and lower extremities in a few days after the first introduction of the instrument: in fact, it was almost sudden." The paraplegia was soon cured, together with the disease of the urinary canal. Several cases, more or less similar to this one, are related by Leroy d'Etiolles, jun., and other writers.

3. *Cases of Paraplegia due to Inflammation of the Bladder.*—In a patient whom I saw in one of the wards of the Charité Hospital at Paris, under the care of Rayer, and whose case is published at length by Leroy d'Etiolles, jun., ('Des Paralysies des Membres Inférieurs,' &c., Paris, 1856, pp. 57–59,) the lower limbs became weak after an incontinence of urine, due to a gonorrhœa. Cystitis appeared, soon followed by a notable but incomplete paraplegia. Gradually the paralysis diminished, while the inflammation of the bladder was subsiding and a complete cure was obtained. Several other cases like this are recorded by Leroy d'Etiolles, Macario, and others.

4. *Cases of Paraplegia due to a Disease of the Prostate.*—A patient consulted Rayer, Magendie, and Leroy d'Etiolles, sen., for an enlargement of the prostate, which was followed by a paraplegia. The swelling proved to be due to an abscess, which opened, and continued for some time to give out pus. When the suppuration stopped, the paraplegia began to diminish, and was quickly cured (Leroy d'Etiolles, jun., *loc. cit.*, p. 83.) Other cases, nearly similar to this one, have been observed by Civiale and others.

5. *Cases of Paraplegia due to a Nephritis.*—Rayer has published several cases of this kind of paraplegia in his classical work on Diseases of the Kidney ('Traité des Maladies des Reins,' vol. iii.) In one case, weakness appeared in the lower limbs of a patient some time after he was attacked with nephritis. An amelioration soon took place in the nephritis and in the paralysis, under a treatment directed against the former. The patient left the hospital; but a short time after he was suddenly attacked again with acute symptoms of nephritis, and lost the power of standing on his feet. Cupping and diuretics quickly cured the inflammation of the kidney, and in less than a fortnight he was also cured of the paraplegia, It must be remarked how quickly in this case the symptoms of paraplegia appeared and disappeared twice after the production and the cessation of a nephritis.

6. *Cases of Paraplegia due to Enteritis.*—Two cases are recorded by Graves, (*loc. cit.*, vol. i., pp. 547–9,) in which the lower limbs were paralysed after an attack of inflammation of the bowels. There was no numbness, no pain, no formication. In one only of these cases the bladder and rectum were a little paralysed. In both patients there was a marked loss of the muscular sense. They both recovered. Zabriskie mentions a case of cure of paraplegia in three weeks, after the cure of an inflammanion of the colon, which seemed to have caused the paralysis ('Gaz. Méd. de Paris,' 1842, p, 296). Several cases of paraplegia, apparently due to dysentery, and cured after the cure of this affection, are also on record. (Macario, 'Gaz. Méd.,' 1858, p. 161.)

To the cases of inflammation of the bowels we may add those of irritation of these organs by worms. Such cases of paraplegia cured immediately, or in a short time, are indeed far from being rare. We will only say that instances of this kind have been observed by Bremser, by Mœnnich ('Biblioth. Méd.,' vol. lxi., p. 269) and by Dr. Calvert Holland ('Edin. Med. and Surg. Journal,' 1845, vol. lxiii. p. 325).

7. *Cases of Paraplegia due to an Affection of the Lungs or the Pleuræ.*—Three cases of this kind are recorded by Macario ('L'Union Méd.,' 1859, p. 276). I have seen a similar case (in 1849) at the Charité Hospital, in Paris. The patient I saw was cured in a few weeks. Two of those seen by Macario were cured, and one died.

8. *Cases of Paraplegia due to Diphtheria.*—Maingault ('De la Paralysie *Diphtérique,' Paris,* 1860, p. 45) relates the case of a young girl who, after *an attack of diphtheria,* became completely paraplegic. For a fortnight no

change, but from that time gradual recovery ; complete cure in three months. Trousseau has seen a similar fact in a child nine years old ; and Sellerier has seen paraplegia, following diphtheria, cured in a month, in a gentleman forty years old (Maingault, *loc. cit.*, p. 46.) I will try to show, hereafter, that paralysis due to diphtheria ought to be considered in the same light as the cases of paralysis more evidently of a reflex origin.

9. *Cases of Paraplegia due to Teething.*—I have seen a very interesting case of this kind in which the paraplegia, which had appeared at the very beginning of the second dentition, in a boy five years old, increased and decreased alternately at the time of, and after, the cutting of each of three molars. In that case there was a slight contraction in some of the paralysed muscles. Simple hygienic means, shampooing, galvanism, and a decoction of cinchona bark, were employed with success. The child was cured two months after the cutting of all his teeth (except the so-called "wisdom teeth"). Underwood has seen a case of paraplegia appearing after every cutting of a tooth. Fliess and others have already shown the relation between teething and paraplegia, and it would not be difficult to prove that those very able writers (especially Dr. Kennedy and Dr. West) who think that it is chiefly the condition of the bowels in teething that causes paralysis have been misled by the fact that very frequently enteritis precedes paralysis. Usually, enteritis in teething, as well shown by Dr. Cain and Dr. Fraser Campbell, is produced by a reflex action, and paralysis seems to be generated in the same way.

10. *Cases of Paraplegia from Irritation of Nerves of the Skin.*—Amongst these cases are the very numerous ones due to cold and wet. Graves (*loc. cit.* p. 563) relates the case of a gentleman, fond of shooting, who became paraplegic after having exposed his lower extremities to wet. There was no pain in the back ; no tenderness on pressure. He was but incompletely cured. This is the kind of reflex paraplegia which usually is most difficult to cure. However, it may be completely and very rapidly cured, as shown by two cases, one mentioned by Dr. T. Watson in his classical work "On the Principles and Practice of Physic," in which case the cure was obtained in a few days, and the other by Dr. W. Moore, of Dublin (The "Lancet," vol. ii. 1859, p. 282), in which case the patient, who owed his paraplegia to a long exposure to a heavy rain, was cured after six or seven days of treatment.

Graves (*loc. cit.*, p. 557) gives a case of hemi-paraplegia due to the irritation of cutaneous nerves by erysipelas, occupying the calf and inside of the right leg. The erysipelas yielded to treatment, but for several days the patient was altogether destitute of any power of motion in the affected limb. It is evident (says Graves) that this must have been a reflex paralysis, as the muscles which move the leg on the thigh lay far above the part in which the erysipelatous inflammation existed.

11. *Case of Paraplegia arising from Disease of the Knee-joint.*—I have had under my care a gentleman, who, according to all appearances, became paralysed of the two lower limbs in consequence of the irritation starting from an acute inflammation of the synovial membrane of the left knee-joint. The paralysis at first was limited to the left limb, but soon appeared in the other. The arthritis became chronic, and then it was observed, a great many times, that every increase in the pain, due to this inflammation, was followed by a corresponding increase of the paraplegia, and that the reverse occurred, as regards the paraplegia, when the pain diminished or ceased. There was no special symptom of any affection of the spine or its contents —except, of course, the paralysis, and a notable degree of muscular atrophy. The patient, after five months of treatment, was very much improved, and could walk pretty well, except at times, when there was a return of the pain in the knee.

12. *Cases of Paraplegia due to a Neuralgia.*—Two years ago I saw a case of this kind in consultation with Dr. Gordon, of Boston, U. S. As this is not a case of rapid cure, I will not speak of it now. Several cases of paraplegia, with or without atrophy of the paralysed muscles, are mentioned by

Notta ("Archives de Med.," Nov. 1854, p. 556). All the patients have been cured of the paralysis some time after the cure of the neuralgia.

The above cases, and many others which we could mention, show that the production of paraplegia may be associated with irritations starting from very different parts—such as most of the viscera, the skin, the mucous membranes, and the trunks of nerves. In all these cases we find that paraplegia has followed the outside irritation, which we consider as its cause, and that the cure of this paralysis has been more or less quickly obtained after the supposed outside cause had been suppressed. Besides, in many of these cases we find the supposed effect increasing or decreasing gradually, in correspondence with the outside irritation, and in several instances we find that even without treatment the paralysis disappeared after the cessation of its supposed cause; while, as a general rule, no treatment of the paraplegia seemed to have the least influence so long as the outside irritation was not alleviated, or had not entirely ceased. In some cases we find that paraplegia appears and disappears altogether, twice, or many more times, in correspondence with the renewed production and cessation of the ouside irritation. Can there be more decisive proofs that it is the outside irritation starting from some sensitive nerves in the various parts of the body which produce paraplegia?

Those persons who, perhaps, are not struck by the evidence in the foregoing facts and reasonings may argue against the view that paraplegia is the effect of an outside irritation in the cases we have mentioned: 1. That there is no need of admitting such a view, as other explanations of the various cases can be given; 2. That it is not possible to explain by a reflex action the production of a paralysis.

The first of these objections may in appearance be well grounded, as it may be said—

1. That, in cases of disease of the uterus, this organ produces paralysis by pressing upon the obturator nerve and the sacral plexus.

2. That, in cases of disease of the prostate, the urethra, or the bladder, the urine, not being freely expelled, becomes altered, and some of the altered principles are absorbed and act as poisons, producing paralysis.

3. That, in cases of disease of the kidney, paraplegia is due to uræmic poisoning.

4. That, in cases of teething, of diphtheria, and of disease of the bowels, the lungs, or the pleuræ, paraplegia is due to an alteration in the blood, caused by the disturbance of digestion or respiration.

5. That, in cases of paraplegia due to the action of cold and wet, we ought to place this affection in the category of rheumatismal paralysis.

6. That, in cases of pain in the skin, in joints, or in trunks of nerves, paraplegia has no other relation to this pain than that of being produced together with it by some affection of the spinal cord.

I am ready to admit that in some cases paraplegia is produced in the various ways just related; but in most of the cases in which a paralysis of the lower limbs follows an outside irritation from the womb, the bladder, or most of the other organs, when there is no evidence of an organic affection of the spinal cord or of the surrounding parts, it is not possible to explain the production of the paralysis by a pressure on nerves, an alteration of blood, etc. This I must show, as, lately, Romberg, one of the promoters of the view that paraplegia may be caused by a reflex action, has acknowledged that he was ready to abandon it on account of objections advanced by Hasse and Valentiner.

1. As regards paraplegia depending upon disease of the womb, we will remark that, except in cases of very great enlargement, as sometimes in pregnancy, pressure upon the nerves of the lower limbs cannot be the cause of the paralysis. In the case I have mentioned, of a young lady who was cured by using a bandage, the uterus was certainly not larger than in a woman at the end of the second month of pregnancy.

2. *It is possible that in* cases where urine accumulates in the bladder, and *remains there a long while* in an altered condition, some of its principles may

be absorbed and act as a poison; but, on the one hand, however possible this is, there is no proof that a paralysis has ever been produced in that way; and, on the other hand, we find that it is with the degree of pain that paraplegia has relations in cases of paralysis of the lower limbs following on affection of the prostate, the urethra, or the bladder, and not with retention of urine, as there is no change in this kind of paraplegia, whether the bladder is carefully voided or not, at every time there is a certain amount of urine in it, unless the irritation starting from either the bladder itself, or the prostate, or the urethra, be diminished.

3. Certainly, in cases of nephritis, there may be paralysis due to uræmic poisoning; but this kind of poisoning does not manifest itself only by a paralysis. On the contrary, it causes other and most striking symptoms, none of which has existed in the case I have cited, and in other more or less similar cases.

4. I am willing to admit that there is some alteration in the blood in diphtheria, or even in dysentery, in enteritis, etc. I am also willing to admit that this alteration may cause a degree of paralysis, but that a complete paraplegia, without any diminution of voluntary movement elsewhere, may be due to a cause which, as it is supposed to be in the blood, is *circulating everywhere in the body*, I think no one can admit.

5. It is true that cold and wet often cause rheumatism; but when, after an exposure to cold and wet, a man is suddenly seized with paralysis, without having the least symptom of rheumatism, we certainly have no right to place his affection in the vague group of rheumatismal paralysis.

6. No doubt that diseases of the spinal cord and its membranes very often manifest themselves by pains resembling neuralgic pains, and located either in a joint or along the course of a nerve; but besides the fact that, in such cases, paraplegia, when it appears, is accompanied by symptoms indicating a disease in the spinal cord or its membranes, which was not what occurred in the cases I have mentioned, there were real inflammations (of a joint, or of the skin) in two of those cases, instead of the *referred* pain due to a centric cause.

We think we may safely conclude, from this examination of the above explanations, that they can be considered as valuable only for a small number of those cases in which paraplegia has appeared after an irritation in some sensitive nerve of the skin, the mucous membranes and various organs. On the one hand, therefore, most of those cases cannot receive these explanations; and, on the other hand, I have already shown that the period of apparition, the variations of intensity, the progress, and the cure of paraplegia, in the cases I have mentioned, are in perfect correspondence with the view that paralysis depended upon an irritation from a sensitive nerve.

I have now to examine the objection that it is impossible to understand how a paraplegia can be caused by a reflex action. It is, indeed, very easy to show how a paralysis can take place by a reflex influence through an irritation starting from a sensitive nerve. There are two modes of reflex action by which such an irritation can produce paraplegia:—

1st. *Reflex Contraction of Bloodvessels.*—As it is now well established that bloodvessels contract with energy, and sometimes even are seized with a real and prolonged spasm, whether by a direct influence of their motor nerves, or through an excitation, which, from some centripetal or excito-motor nerve, has been reflected upon them by the cerebro-spinal axis, there is no need of showing here that bloodvessels are just like muscles of animal life as regards their relations with the nervous system. This being the case, it is extremely easy to understand how a paralysis of the lower extremities, as well as that of any other part of the body, may be paralyzed by a reflex action. In three different places a contraction of bloodvessels may cause paraplegia—1, in the spinal cord; 2, in the motor nerves; 3, in muscles. A contraction of bloodvessels in the spinal cord *I have seen* (in the vessels of the pia mater) taking place under my eyes, when a tightened ligature was applied on the hilus of the kidney, irritating the renal nerves, or when a similar operation was performed on the bloodvessels and nerves of the supra-

renal capsules. Generally in those cases the contraction is much more evident on the side of the cord corresponding with the side of the irritated nerves, which fact is in harmony with another and not rare one, observed first by Comhaire (as regards the kidney), and often seen by me after the extirpation of one kidney or one supra-renal capsule—i. e., a paralysis of the corresponding lower limb. It is probable that irritation starting from the urinary and other organs produce a paraplegia by a contraction rather of the bloodvessels of the spinal cord, than of those of the motor nerves and muscles. However in this form of paraplegia, it is not rare that a notable diminution of temperature of the paralyzed limbs shows that the bloodvessels of these parts are also contracted.

2nd. *Morbid Reflex Influence on Nutrition.*—This influence, proved by many experiments on animals and by pathological facts of daily occurrence, seems usually not to exist in reflex paraplegia, except in that form in which muscles become progressively and rapidly atrophied and altered (*wasting palsy*). But this morbid influence may take place at any moment in the course of a reflex paraplegia, so long as the irritating cause has not ceased to act, and a myelitis or some other affection may be generated by it.

We think that it will now be considered possible, if not probable, that the production of reflex paraplegia is due to a contraction of bloodvessels and to the insufficiency of nutrition that follows this condition of the vessels. In addition to the reasons we have given to show that the paraplegia caused by an outside irritation is due to a reflex action and probably to a reflex contraction of bloodvessels, we will say that very often in this peculiar kind of paralysis there are reflex convulsions in muscles of the trunk, the head, and the limbs, before the paraplegia appears (as in cases of teething, of worms, &c.) or when it has already existed some time, (as on the passage of a catheter, urinary paraplegia.) We might also have pointed out the influence of the urethra on the spinal cord, by mentioning the well-known and very frequent fact of the production of chills and tremor on or after the introduction of a catheter in persons who are or who are not paraplegic.

There is another series of facts of which we have now to speak, that show that reflex paraplegia is not due to an evident organic affection, which may be found in a post-mortem examination. We will very briefly mention some cases of autopsy made on persons who have died after having presented the symptoms of a reflex paraplegia.

Before concluding this lecture, I wish to say a few words on a most important point, to which we will again turn our attention in the succeeding lectures: I mean, the treatment of reflex paraplegia. I have already said that the various kinds of paraplegia may be classed into two general groups; one in which the amount of blood circulating in the spinal cord is too considerable; the other, in which the opposite condition exists. Of the various remedies, which, as I may perhaps be allowed to say, are too often blindly employed, there are some that diminish the quantity of blood in the spinal cord, such as mercury, ergot of rye, and belladonna; while others, on the contrary, such as strychnine and brucine, increase that quantity. If these last assertions be correct—and I will, by-and-by, try to prove that they are,—it is quite evident that to employ a remedy of either of these two kinds in both of the two groups of paraplegic cases is a most dangerous thing. Reflex paraplegia, as pointed out by its whole history given in this lecture, is accompanied and most likely produced by an insufficiency of the amount of blood in the spinal chord; it ought not, therefore, to be treated by those remedies which diminish the quantity of this fluid in the spinal nervous centre. How often, however, is not mercury employed in that affection? On the other hand, myelitis which is accompanied by an increase in the quantity of blood in the spinal cord, is often treated by the very remedy (strychnine) which has the most powerful influence in increasing the amount of blood in the spinal cord. How important, therefore, is it to know exactly to what group of affections (as to the quantity of blood in the spinal cord) *belongs a paraplegia,* before we give a remedy included in either of the two *categories above-mentioned?*

[Before speaking of the treatment of cases of reflex paraplegia, Dr. Brown-Sequard premises a few remarks on several forms of paraplegia presenting some analogies with reflex paraplegia.]

Amongst these forms or kinds of paraplegia, one of the most interesting is the paraplegia due to an obstacle in the circulation of blood in the aorta, or in its principal ramifications in the pelvis. This kind of paralysis has been well studied by veterinary surgeons, as it is frequent in horses. Several cases have been observed in man by Mr. Barth, Dr. W. W. Gull, and my friend Dr. Charcot. Besides the symptoms due to the cause preventing the circulation (generally an aneurism), this kind of paraplegia will be distinguished from the reflex paraplegia by the absence of the causes of this last form of paralysis, by the alterations of nutrition and pains in the paralyzed limbs, and especially by the rapid increase of the paralysis after every notable exertion of the lower limbs, and the return of some power after rest.

There is another kind of paraplegia in which in some degree, a pressure on the large bloodvessels (arteries and veins) of the pelvis has a share in the diminution or loss of power. This kind of paraplegia exists frequently, in a slight degree, at the end of pregnancy; it is chiefly due to the pressure on the nerves of the lumbo-sacral plexus. During pregnancy, or in cases of a tumour in the pelvis, a complete paraplegia may be caused by pressure upon those nerves. This kind of paralysis of the lower limbs will easily be distinguished from the reflex paraplegia by the existence of violent pains in the pelvis and in many parts of the lower limbs, by the production of cramps, &c.

The so-called *idiopathic paraplegia* is not to be taken into account in this history of the differential diagnosis of the reflex and other forms of paraplegia. We do not know any case of paralysis of the lower limbs that deserves the name of idiopathic. Most of the pretended cases of that kind, published by Macario and by R. Leroy d'Etiolles, are either cases of reflex paraplegia or a real affection of the spinal chord (especially congestion and serous effusion). We do not mean to say that it will always be easy to decide during the life of a patient, or after a post-mortem examination, what is the cause of a paralysis of the lower limbs. We think that sometimes this cause will not be found out, and we only mean to state that it is impossible to admit the existence of a special kind of paralytic affection, deserving to be called *essential* or *idiopathic* paraplegia—an affection not more characterized by special symptoms than by special anatomical lesions.

Of other forms of paraplegia, such as those which are connected with gout, rheumatism, or those which follow grave fevers, cholera, &c., we will only say that usually they depend upon a serous effusion in the spinal canal or a venous congestion, and that sometimes they belong to the group of reflex paralysis.

In concluding our remarks on the diagnosis of the reflex paraplegia we need hardly say, that for this affection, as well as for all other diseases of the nervous system, it is not necessary to find together all the characteristic symptoms to be convinced of the existence of a reflex paraplegia. This affection is almost sufficiently characterized by *the absence of the special symptoms of an organic disease of the spine or its contents, and the existence of an incomplete paralysis of the lower limbs that has appeared somewhat slowly after a disease of the urinary or genital organs, or of some other abdominal viscus; after an inflammation of the lungs or pleuræ, or after some other kind of irritation of a nerve in its trunk or cutaneous ramifications.* In a great many cases of reflex paraplegia we shall find nothing else upon which to ground our diagnosis. But usually, in a short time, a much greater probability of the accuracy of the diagnosis will spring from the correspondence between changes in the degree of the paralysis with changes in the visceral disease or external irritation that is supposed to have produced the paraplegia.

The prognosis of the reflex paraplegia depends, in a great measure, upon that of the external affection which has produced it. The whole history of the reflex paraplegia shows that it usually increases, diminishes, or disappears with the external morbid condition which has produced it. As a general rule, so long

as this morbid condition lasts, the paraplegia persists, and it ceases very quickly, and sometimes without any special treatment, after the cure of this morbid condition. In some cases, however, paraplegia persists long after its external cause has been completely cured. It is so especially when the paralysis has been produced by cold, or by a disease of the bowels. The reverse is more particularly observed after the evacuation of worms, or after a cure of a stricture of the urethra, or of an affection of the womb.

Of the two forms of reflex paraplegia, one of which depends upon an insufficiency of nutrition in the spinal cord, the other upon an alteration of nutrition in the muscles of the lower limbs—the first is almost always curable when the external irritation which has caused it has ceased; while the second is very frequently incurable, whether its external cause has ceased or not. It is, therefore, most important for the prognosis to ascertain if there has been, or not, a rapid atrophy of muscles in cases of reflex paraplegia.

*Treatment of the Reflex Paraplegia.*—It is evident from all that we have said concerning the reflex paraplegia that, for this affection more than for any other, we must employ every effort to cure or alleviate the disease or irritation that has produced it. At the same time the paraplegia itself must not be neglected, and the proper means must be employed against it. Therefore the treatment of a reflex paraplegia ought to consist of two distinct parts: 1st, the means to be employed against the external cause of this affection; 2nd, the treatment of the paralysis itself. As regards the first part, we will say nothing here, as we cannot enter into the details of the treatment of nephritis, cystitis, pneumonia, enteritis, and other morbid states that may cause reflex paraplegia. As regards the direct treatment of this affection, we will first lay down the general rules of the treatment, and then we will enter into the most important details concerning this treatment.

1. When it has been ascertained from what organ or from what nerve starts the nervous influence which causes a reflex paraplegia, besides the treatment that is appropriated to the nature of the local affection (of that organ or nerve), it is of the greatest importance to try to prevent or to diminish the transmission of any nervous influence from the diseased nerve or organ to the spinal cord. All the means usually employed to alleviate pain will be of service in such cases. If possible, we must try to paralyse for a time the sensitive nerves that convey the morbid influence to the spinal cord. Even a momentary suspension or diminution of the transmission of this influence may be very useful. Narcotics ought to be employed in injections,—in the bladder, if that organ be the place from which starts that morbid influence; in the vagina, if the uterus be the place; and in the rectum, if the large intestine be the place. Narcotics ought to be taken by the mouth if the stomach, the small intestines, or the kidneys are affected. In case of a pneumonia producing a reflex paralysis, inhalations of chloroform (which, by the way, have been successfully employed against the inflammation itself) may prove useful. We will say by-and-by what narcotics should be preferred.

2. The object of the means just proposed is to diminish the cause of the paraplegia; the object of the means we will now speak of is just the same, although it may seem to be quite different. Excitants or revulsives applied to the skin of the legs have been warmly recommended by Graves, who has obtained good results from their use. Probably the mode of action of these means consists in producing for a short time the same effect as the irritation which is the cause of the paralysis—i. e., a contraction of the bloodvessels of the spinal cord: but, according to a well-established law, if such a contraction becomes considerable, the muscular fibres are soon exhausted, and a relaxation of the contracted fibres takes place, and, as a consequence of this relaxation, a dilatation of the bloodvessels occurs. Of all the causes of irritation capable of producing a contraction of bloodvessels by a reflex action, none has more power than cold. In consequence of this fact, I *think some of* the modes of application of cold to the spine ought to be em*ployed in cases of reflex* paraplegia. But the excitation in those cases must *be very powerful, and* able to produce a very considerable degree of con-

traction, so that the consequent exhaustion and dilation may be obtained. With the same view, we may employ galvanism.

3. Another important principle, or rather another part of the same general principle of treatment, consists in making use of the following means to increase the quantity of blood in the spinal cord:—Every night, and often in the course of the day, the patient should lie down on his back, placing his head, his arms, and his legs on high pillows, so as to produce by gravitation a congestion in the spinal cord. This simple means, which is also applicable in cases of hysterical paraplegia, and in almost all the cases in which there is an insufficient amount of blood in the spinal cord, is just the reverse of what should be done in cases of inflammation or congestion of the spinal cord or its membranes, or of disease of the spine, &c., in which cases the patient ought to lie flat on the abdomen or on one side of the body, and have his feet and hands on a much lower level than that of the spine.

4. As regards the remedies to be taken by patients attacked with a reflex paraplegia, they must essentially be those which increase the amount of blood in the spinal cord, and augment the vital properties of this nervous centre, and also those remedies which render the blood richer in nutritive principles.

5. As regards food and the hygienic rules, patients attacked with a reflex paraplegia must have the most substantial aliments, so as to improve the deficient nutrition of the spinal cord. They must take a great deal of exercise in the open air, and especially make use, as much as possible, of the paralysed muscles.

Before giving the details of the rules of treatment, we will sum up in the following propositions what we have just said:—

1. Try to prevent, even for a short time, the continuation of the contraction of the bloodvessels of the spinal cord by the use of narcotics, to be applied, as much as possible, to the organs from which starts the external cause of the paralysis.

2. Try to produce by revulsives such a notable increase in the contraction of the bloodvessels of the spinal cord that an exhaustion of the contracted muscular fibres, and, as a consequence of this exhaustion, a dilatation of the bloodvessels, will soon take place.

3. Have the spine placed much lower than the head, the arms, and the legs, when the patient is lying in bed, so as to increase the amount of blood in the spinal cord.

4. Employ those remedies that have the power of augmenting the vital properties of the spinal cord in increasing the amount of blood in this nervous centre.

5. Give the kind of food and employ those hygienic means that are most fitted for increasing the quantity of blood and for improving nutrition in all parts of the body.

On the one hand, experience has proved all these rules of treatment to be the most effectual; and on the other hand, these rules are in perfect harmony with the theory of the disease that we have given: so that experience and theory furnish a confirmation to each other.

*On Inflammation of the Spinal Cord.*—To understand well the symptoms of inflammation of the spinal cord, it is important to bear in mind the following facts:—

1. That the gray matter of the spinal cord in its normal condition is not at all *excitable*, and that irritations upon it are followed by sensations or movements; while, on the contrary, when it is inflamed it is excitable and able to give all kinds of sensations, and to produce cramps or partial convulsions.

2. That the white matter of the spinal cord is not composed of motor and sensitive fibres coming from or going to the brain.

3. That a pressure upon the spinal nerves, or upon the spinal cord, able to produce a paralysis accompanied by cramps, may either produce anæsthesia or not, while it causes various sensations.

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2. That the white matter of the spinal cord is not composed of motor and sensitive fibres coming from or going to the brain.

3. That a pressure upon the spinal nerves, or upon the spinal cord, able to produce a paralysis accompanied by cramps, may either produce anæsthesia or not, while it causes various sensations.

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fever, as in almost all cases of that kind there is at the same time an acute inflammation of the spinal meninges, and also because we only intend to show what the symptoms are in those cases of paralysis of the lower limbs of long standing that depend upon a chronic inflammation of a part of the spinal cord in the middle or lower parts of the dorsal regions.

The characteristic symptoms of this local myelitis are :—

1. A constant pain at the part of the spine corresponding with the upper limit of the inflammation of the cord.

2. Whether a constant pain exists in a very marked degree or not, it is almost always found that pressure upon the spinous process of the vertebræ (sometimes even a slight one), when made at the upper limit of the inflammation, causes an acute pain.

3. The passage of a sponge, filled with warm water, along the spine, gives a normal sensation of heat in all the parts above the seat of the inflammation, but a burning sensation at its upper limit.

4. The passage of a small lump of ice along the spine gives the natural sensation of cold everywhere, except at the level of the inflammation, where the sensation is that of burning.

5. Most patients complain much of a sensation as if there were a cord, or some other ligature tied round the body, at the limit of the paralysis. In a few patients there is but a very slight sensation of that kind. This symptom seems to exist in all cases of myelitis, and to depend chiefly, but not entirely, upon a state of cramp of some part of the muscles of the abdomen or the chest.

6. Various sensations, resembling very much those which follow the pressure upon a nerve, such as formication, a pricking by pins and needles, and sometimes a feeling of burning or intense cold in the feet, legs, and thighs, less frequently in the abdominal walls. These sensations exist with as much, if not with greater violence, in parts deprived of sensibility, as in parts which are still sensitive. They originate from the irritation of the grey matter of the spinal cord, and are *referred* to the limbs and abdomen, just as the pressure upon a nerve produces sensations in its ramifications. They are important indications of myelitis.

7. Cramps in the feet or calf of the legs are very frequent. There are more or less of them in every patient. Frequently there are cramps also in the large abdominal muscles, besides the circular and linear cramp that gives the above-mentioned feeling of tightening. A cramp limited to a part of one or several abdominal muscles, may remain almost permanently for days and weeks, forming a kind of lump, which may be mistaken for a tumour.

8. Whether myelitis exists only in a small zone of the spinal cord, or occupies the whole of the dorso-lumbar enlargement, the paralysis of movement exists in all the parts of the body that receive their nerves from the portions of the spinal cord that are below the upper level of the inflammation. The degree of paralysis varies extremely in different patients, but it is nearly the same in the various muscles in the lower limbs in the same patient.

9. Paralysis of the bladder and of the sphincter ani is almost always present in inflammation of the lower part of the dorsal region of the spinal cord; but when the seat of the inflammation is higher up in the dorsal region, there is rather a spasm than a paralysis in the sphincters of the bladder and anus. Often then there is retention of urine, owing to the paralysis of the bladder while the sphincter vesicæ is more or less in a spasmodic state.

10. One of the most decisive symptoms of myelitis is the alkilinity of the urine. There is no patient attacked with myelitis in the dorsal region of the cord whose urine is not frequently alkaline. At times, especially after certain kinds of food, the urine is acid, but the alkalinity soon reappears.

*11. Anæsthesia*, or at least a diminution of sensibility, always exists in *myelitis, except when the* grey matter is not the seat of the disease, which is *very rare. Usually*, the inflammation begins in the central parts of the

grey matter, and then a diminution of sensibility is one of the first symptoms. That peculiar kind of sensibility of muscles which serves to direct our movements is especially impaired in the very beginning.

12. When the dorso-lumbar enlargement is inflamed, reflex movements can hardly be excited in the lower limbs, and frequently it is impossible to excite any. On the contrary, energetic reflex movements can always be excited when the disease is in the middle of the dorsal region, or higher up.

*On the Treatment of Chronic Myelitis.*—[Myelitis, in the acute form is almost always fatal. But the subacute and chronic forms are frequently stopped in their progress, when properly treated, and sometimes an almost complete cure may be obtained. Any actual injury, however, which the cord may have sustained, is almost irreparable. Chronic myelitis is an affection of very slow development, and still slower progress. In the treatment, the first and most important rule is, to diminish the congestion of the spinal cord. To this end the patient must avoid these positions of the body in which by gravitation the amount of blood in the cord will be increased. Hence the patient must never lie on the back. Dry cupping daily on the various parts of the spine will prove of service.]

Amongst the remedies to be employed internally, the most active are those which have the power of diminishing the congestion of the spinal cord. The two which seem to be most powerful in this respect are belladonna and ergot of rye. Experiments upon animals have shown to me, in the most positive manner, that these two remedies are powerful excitants of unstriped muscular fibres, in bloodvessels, in the uterus, in the bowels, in the iris, &c. Both of them dilate the pupil; both are employed with success to produce contractions of the uterus; but each of them has more power in certain parts than the other, so that we find belladonna acting more than ergot on the bloodvessels of the iris (which is the principal cause of the dilatation of the pupil)—on the bloodvessels of the breast (which is the principal cause of the cessation of the secretion of milk)—on the muscular fibres of the bowels (which is the mode of its action in cases of strangled hernia)—on the sphincter of the bladder (which is one of the causes of its success against nocturnal incontinence of urine), &c.; while, on the contrary, we find that ergot acts more than belladonna on the muscular fibres of the womb, on the bloodvessels of the spinal cord, &c. We cannot give here the proof of the exactitude of these assertions on the mode of action of these two remedies, but we must at least answer an objection which probably will arise in the mind of many persons. It will be asked—How is it that, of two remedies that are able to excite contractions in smooth muscular fibres, one produces them more in one place, and the other more in another place? The answer to this objection is indeed very simple. The excitability of smooth fibres, as well as that of striated muscles, varies exceedingly in different parts of the body. An exciting agent (whether galvanism, cold, heat, or belladonna and ergot of rye) will produce powerful contraction in some places, and hardly any in other places. The smooth fibres of the uterus contract more than those of the bowels or the bladder, and less than those of certain bloodvessels when stimulated by galvanism; the smooth fibres of certain bloodvessels contract more than those of the uterus under the excitation of cold; still more the bloodvessels of the cerebral lobes and of the face, which contract so much when their nerve (the cervical sympathetic) is irritated, contract but very little when excited by belladonna and ergot, while these two excitants produce powerful contractions in the bloodvessels of the spinal cord.

Not only have I seen the diminution in the calibre of bloodvessels of the pia mater of the spinal cord taking place in dogs after they had taken large doses of belladonna or ergot of rye, but I have also ascertained that the reflex power of the spinal cord (most likely as a consequence of the contraction of bloodvessels) becomes very much diminished under the influence of these two remedies, which in so doing act just in the opposite way to that of strychnine.

*Led by the knowledge* of the above facts, we have employed belladonna

and ergot of rye in cases of paraplegia due to a simple congestion or a chronic inflammation of the spinal cord and its meninges, and we have obtained a greater success than we had dared to hope for. Whatever be the value of our experiments on animals as regards the mode of action of these remedies, it is now certain that they have really a great power in diminishing the amount of blood in the spinal cord and its membranes. It is very well known that many French physicians, especially Bretonneau, Payan, Barbier, and Trousseau, have for many years employed with success belladonna and ergot of rye in cases of paraplegia. Of course we do not claim to have been the first to make use of these remedies in paraplegia, any more than we claim priority as regards employing strychnine in various forms of paralysis. But we claim to have pointed out, as clearly as we could, in what cases of paraplegia strychnine or belladonna and ergot of rye are to be employed or avoided. To indicate this distinction is the principal object of these lectures.

In the beginning of the treatment of chronic myelitis, we usually employ ergot of rye alone internally, and belladonna externally in a plaster applied to the spine, over the painful spot. The dose of ergot, when the powder is used, which is almost always the case, is at first two or three grains twice a day; gradually the dose is increased until it reaches five or six grains twice a day; and in a few cases we have given eight grains twice a day. We do not think it necessary to make use of the very large doses employed by M. Payan. The belladonna plaster applied to the spine must be a very large one, four inches wide, and six or seven inches long. If there is no amelioration in a few weeks, we give the extract of belladonna internally in doses of a quarter of a grain twice a day.

When we find that the patients, after six or eight weeks of treatment by ergot of rye and belladonna, do not get better, we give iodide of potassium in doses of five or six grains twice a day, in addition to the preceding remedies. When there is any reason to suspect that there is a degree of meningitis together with myelitis, we begin at once the treatment by the iodide of potassium with the ergot and belladonna.

*On Paralysis of the Lower Extremities.*—[One of the most frequent causes of this kind of paralysis is probably inflammation of the spinal cord; another cause is softening, caused by alterations in the bloodvessels preventing a proper interchange of matter between the nervous tissue and the blood.]

Usually patients attacked with the non-inflammatory softening of the spinal cord at first complain only of weakness. They gradually become weaker, although their general health may seem to be as good as ever. They feel the weakness especially at the knee and ankle-joints; they find it very difficult to go up or down stairs, to go in or out of a carriage, and their gait becomes tottering, especially when they cannot direct their movements with the help of the sight. When lying down, if they see the lower limbs, they can move them pretty freely, but without force. After a time the paralysis becomes much more marked, and then sensibility in the whole length of the lower limbs and the power of the will over the bladder and the rectum are also found much diminished. There is no pain, or hardly any, in the spine or paralysed parts. Very rarely is the urine altered. The temperature of the lower limbs is often higher than it is in health, especially when the paralysis is complete or nearly so.

*Hemorrhage in the Grey Matter of the Spinal Cord.*—Paraplegia due to this cause is characterized by the suddenness of its production, by a pain at the part of the spinal cord where the hemorrhage occurs, owing, probably, to the distension of the posterior roots of the nerves in the grey cornua, by a pain in the spine after pressure upon it, and also by a pain in the parts of the body where are distributed the nerves originating from the part of the spinal cord which is the seat of the disease. The sphincters of the bladder and rectum are almost completely paralysed from the very first. It is not *rare that an inflammation* supervenes in the parts of the cords surrounding *the clot, and then all the* symptoms of myelitis occur.

*According to the seat* and the extent of the hemorrhage the symptoms vary

extremely. If the amount of blood effused be very small, the symptoms may be very slight, and the patient may recover in a few months, as in a case recorded by Cruveilhier. If the amount of blood is considerable, it descends along the whole length of the central canal of the spinal cord, and also sometimes breaks through the grey cornua, especially the anterior ones. The loss of sensibility and of voluntary power is then complete, and the temperature of the paralysed limbs is increased. If the hemorrhage is limited to one lateral half of the grey matter,—as in a most interesting case recorded by Monod, and in another by Mr. Oré.—there is loss of movement in the side of the hemorrhage, and loss of sensibility in the opposite side.

It is a remarkable feature of paraplegia due to hemorrhage in the grey matter of the spinal cord. that a diminution of sensibility is always one of its first symptoms; and that when the whole of the grey matter is altered by the effused blood, the anæsthesia is complete.

*Diagnosis of the Hemorrhage in the Spinal Cord.*—The suddenness of the production of paraplegia is almost sufficient to show that it is due to effused blood, but it remains to decide whether the blood is in the tissue of the spinal cord, or in the vertebral canal, outside of this nervous centre. In this last case, there is more pain in a great extent of the spine then in the preceding; and also tetanic convulsions are not rare when the blood is effused outside of the spinal cord; while a hemorrhage in the cord has never produced convulsions, so far as I know. Besides, there is more marked anæsthesia in cases of effusion of blood in the tissue of the spinal cord thun outside of it.

It is usually very easy to distinguish a homorrhage in the spinal cord from a white softening of this organ. The suddenness of the production of the paralysis and the existence of the pains will sufficiently characterize the first of these affections; and in those cases of almost sudden paraplegia, owing to softening, the absence of pains (in the spine &c.) will serve to distinguish this form of paraplegia from that due to a hemorrhage. I need not say that these two affections—i. e., softening and hemorrhage—do not rarely co-exist, This co-existence will not be surprising to any one who knows that in the spinal cord, as well as in the brain, the same morbid condition of bloodvessels is usually the cause of both these affections.

*Prognosis of Paraplegia due to White Softening or Hemorrhage in the Spinal Cord.*—The great indication for the prognosis in cases of softening or hemorrhage in the spinal cord is the extent and degree of the paralysis. A softening limited to the lower part of the dorsal region of the spinal cord may admit of a very prolonged duration of life. A hemorrhage is a more grave affection than a softening, because there is always a great chance of its being reproduced, and also because the blood effused may cause very extensive alterations. We do not know a single case of complete cure of paralysis of the lower limbs caused by softening or hemorrhage in the spinal cord; but, as far as diagnosis not confirmed by autopsy may be relied on, we can say that we have seen many cases of non-inflammatory softening of the spinal cord arrested in their development, and still more, that in five or six cases, a very notable amelioration has taken place.

*Treatment of Paraplegia due to White Softening of the Spinal Cord.*—The treatment of this affection ought to be pretty much the same as that of hemiplegia caused by a non-inflammatory softening of the brain. We will not enter here into the discussion of the various modes of treatment which have been proposed or employed in this last case; we will only mention the general plan of treatment we have employed with some appearance of success in many cases of softening of the spinal cord.

1st. Iodide of potassium is the principal of the remedies that may be relied upon. We generally employ it in doses of five grains twice a day, mixed with nearly the same doses of sesqui-carbonate of ammonia, in a decoction of cinchona bark, or an infusion of calumba or rhubarb. We insist upon the importance of taking this mixture before food in the morning and an hour before dinner, to avoid the decomposition of the iodide by the *gastric juice and* the setting free of the iodine, which sometimes takes

place, and causes a gastric disturbance, erroneously attributed by some practitioners to the iodide itself.

2nd. Besides the preceding remedy, we prescribe various tonics, such as iron and cinchona-bark wine.

3rd. Strychnine may be employed with some profit in those cases where the paralysis is slight; but it must be remembered that, on account of the congestion produced by large doses of this alkaloid in the spinal cord, there would be danger in making use of it in cases where, the paralysis being complete, or nearly so, there is reason to think that the bloodvessels of the spinal cord are much altered, and that therefore a hemorrhage might be the consequence of the action of strychnine. I need hardly say that those remedies which diminish the amount of blood in the spinal cord (such as belladonna, ergot of rye, &c.) should not be employed in this form of paraplegia.

4th. Besides the use of some of the above remedies, I recommend, as an excellent means of improving the nutrition of the spinal cord, the daily use of the cold douche or the cold shower bath appplied to the spine. Sea-bathing may also be of service.

5th. The patient should lie down flat on his back at night; he should live upon the most nutritious food, drink wine and beer in a moderate quantity, and take as much exercise in the open air as possible, without however exhausting his diminished power of motion. Shampooing and galvanism may be applied with profit to the paralysed limbs.

*Treatment of Paraplegia due to Hemorrhage in the Spinal Cord.*—The same rules are to be followed in cases of this kind of paralysis as in cases of softening of the spinal cord, with these slight differences: that, 1st, three doses of iodide of potassium, instead of two (of five grains each), ought to be given every day; 2nd, strychnine ought not to be employed; 3rd, constipation, lying down on the back, and all other causes of congestion of the spinal cord, should be carefully avoided.

*Symptoms and Treatment of Paraplegia due to Hemorrhage in the Vertebral Canal.*—A hemorrhage between the spinal cord and its bony envelope is fortunately an event of rare occurrence. I have seen but one case of this kind of hemhorrhage, which proved fatal in less than two days. The blood is effused either between the pia mater and the arachnoid, or (more rarely) between the dura mater and the vertebræ. The first symptom is usually a pain extending along the spine from the seat of the hemorrhage to the lower extremity of the vertebral canal. Almost immediately a complete paralysis of the lower limbs and very often tetanic convulsions appear. In almost all cases this hemorrhage is rapidly fatal, either in consequence of its influence upon the circulation or respiration, or, indirectly, in consequence of an acute and extensive meningitis. Our object being here to study paraplegia in its chronic forms, we will not say more upon the symptoms of this affection.

As regards the treatment of the hemorrhage in the vertebral canal we will only say: 1st, that, at once, all the most active means usually employed in the various cases of visceral hemorrhage should be made use of; 2nd, that the patient should be placed in bed on one side, and not on his back; 3rd, that pounded ice should be applied, in bladders, all along the spine; 4th, that if the patient survives several days, the same treatment as is above prescribed for cases of hemorrhage in the grey matter should be employed.—*Lancet, April* 28, *May* 5, *June* 9, *July* 14, *Aug.* 18, *and Sep.* 8, 1860, *pp*, 415, 437, 563, 27, 155, 229.

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## 8.—ON TETANUS.

By Frederick C. Skey Esq., F.R.S., Surgeon to St. Bartholomew's Hospital.

[All the known facts of tetanus go to prove that the disease is centred in the nervous system, and that the circulatory system is but secondarily affected.]

*No sooner is the nervous centre sent to sleep than the disease appears as suddenly to cease.* Notwithstanding this improvement, so deceptive and

insidious, it is obvious that morbid actions predominate, and I doubt whether such temporary improvement, plausible and indeed marvellous as it is, can be said to indicate an arrest of the disease in however slight a degree.

A boy, aged fourteen, was brought into St. Bartholomew's Hospital, with a hand greatly mutilated by machinery. The house-surgeon decided on consideration,—and I thought wisely decided,—to make the attempt to restore the hand. For twelve days he progressed favourably. On the thirteenth, he complained of stiffness in the back of his neck. When I saw him, at the expiration of some hours, spasm of the muscles had extended to the chest and abdomen; but none of the muscles were severely or painfully contracted. Still the signs were distinct. The boy was the subject of tetanus; and as I had never had a succesful case, I was the more anxious to adopt some more promising agent than any I had hitherto employed. With this view I determined to test the efficacy of the woorara poison, which had been employed with some effect by Mr. S. Wells in three cases of tetanus. By the kindness of Mr. Savory, who had a small quantity of the fresh poison, I was enabled to obtain sufficient for the purpose. We dissolved two grains in an ounce of distilled water. The boy being placed under the influence of chloroform, I amputated the hand immediately above the wrist-joint. My intention was to have applied two drops of the solution to the wound, and, observing the consequences, to have increased the quantity applied according to the effect produced. On recovering his consciousness, it was obvious that what had been done for the boy, whether the removal of the hand or the administration of chloroform, had been in the direction of good, the pulse had fallen from 130 to 100, and the rigidity of the muscles was greatly diminished, and continued so. The pain was also reduced, and the countenance was more tranquil. He could open his mouth to an extent sufficient for the introduction of food. The rigidity of the muscles was by no means great, nor did he suffer the degree of pain usually attendant on severe cases of tetanus. It is very true that the sterno-mastoids, pectorals, abdominal muscles, and, indeed, the muscles of his trunk generally, were involved; but their contraction was comparatively slight and certainly was unproductive of considerable pain. The use of the woorara was postponed, and I ordered the boy tincture of opium, ten minims; compound spirit of ammonia, half a drachm; brandy, half an ounce, every two hours. On the following day I found he had had a tolerably good night; his pulse had slightly risen in frequency; the rigidity of the muscles had not increased; on the contrary, it appeared somewhat lessened. His countenance was more anxious, which appeared rather attributable to mental than bodily suffering, for he had only recently become cognizant of the removal of his hand. By two o'clock on that day, the boy had consumed four ounces of brandy, half an ounce of spirit of ammonia, and eighty drops of laudanum. Still I had no excuse for the trial of the woorara. Doubtless the boy was in a dangerous condition; but it was obvious that the condition was not attributable to the morbid contraction of the muscular system, but to something beyond it. His bowels were constipated. I ordered him calomel and jalap, castor oil, enemata of turpentine, croton oil in no insignificant doses; but I failed to obtain the desired result up to the time of his death. In other respects, the same treatment was continued. On the third and fourth days his condition was not materially changed. He took food without difficulty, as also his medicine at the required intervals. The ammonia was increased to drachm doses. On the fourth day, his pulse had increased to 135, and his countenance was more pallid. The rigidity of his muscles had not increased in any positive degree, and those of the extremities were entirely free from spasm. On the morning of the fifth day he had taken, without producing any marked effect on his system for good or for evil, about forty-eight ounces of brandy, one ounce of tincture of opium, and four ounces of spirit of ammonia. And we may form some judgment of the severity as well as the prostrating influence of the disease under which this poor boy was labouring when we consider that such an enormous power was inadequate to produce the smallest impression on his system. On the last visit I paid *except that he* appeared somewhat weaker, I cannot say that he was

positively worse. He could open his mouth sufficiently wide for the introduction of nourishment, and he took food freely throughout the greater part of his illness. His pulse was a shade weaker, but not materially so. His spasms were not severe nor considerable. and assuredly they were not a source of pain. Of this the boy gave me full assurance on many occasions. On the evening of the fifth day he died. His body was not examined.

If the nature of the disease we call tetanus could be ascertained by dissection, we should have acquired this knowledge long ago; but, so far as I know, we have obtained no information by such means, except negatively, on which to found a theory, or to obtain a guide to practice. The total amount of knowledge consists in this: that at varying intervals after the occurrence of more or less violent local injury, and occasionally—but in our latitude very rarely—independently of injury, we observe rigidity of the muscular system, commencing with the muscles of the jaw and extending to the neck, trunk, and finally to the extremities; that it is accompanied by great pain in the contracted muscles, a quick pulse and respiration, and constipated bowels; and that it terminates, in from two to six or eight days, in death. Such is nearly the sum total of our knowledge of tetanus, and it may be doubted whether we are even now occupied in the right path of inquiry. We fix our attention on the local spasm of the muscular frame, as though that condition of the system constituted the essence of the disease, of which it is only a symptom. We rather search for remedies that will abate spasm, than for such as will attack its cause. The above case exemplifies negatively this fact. The boy died of tetanus, but not of spasm. At no period of his case was the contraction of his muscles so rigid as to cause pain of a severe character. He complained of his arm; he suffered from a sense of illness; he desired to be let alone, and not to be disturbed; his expression betokened serious illness; but he did not die from pain nor from spasm: he gradually sank from utter prostration of nervous power, which every remedy employed was incompetent to contend against. And it is by no means uncommon to observe, towards the close of life, that the most prominent symptom of the disease—namely, muscular rigidity—gradually subsides, while the real disease advances towards its fatal crisis. In the whole range of disease, I do not recollect any evidence of the existence of what we term the nervous system more remarkably or more conclusive than is derived from the influence of anæsthetics in tetanus. How absolutely is the morbid influence of the nervous system on the heart and respiratory system suspended by chloroform! and how suddenly too! The pulse falls to its natural standard, the equilibrium of respiration is restored, all expression of suffering subsides under the reality of tranquil sleep. So complete is the external manifestation of health, that it would astonish a bystander, ignorant of the nature of the case, to be assured that in a few days, possibly a few hours, it would terminate in death. Suspend the remedy, remove the influence of the anæsthetic agent, and the entire train of fatal symptoms starts, as it were, into existence—the pulse rises, the respiration is quickened in proportion, and the muscular spasm, with its usually inseparable companion, pain, become again dominant. Nor do we observe that the disease, to whatever extent it may have reached, is retarded or even diminished by the treatment. On the contrary, its progress is unremitting; and both the violence of the spasm and the severity of the pain are regenerated with an increase of intensity proportioned to the duration of time during which they have been suspended by anæsthetic agency. In the above case, I was fully prepared to test the curative properties of the woorara poison, as recorded by Mr. Wells and recent French writers; but I really had no excuse for its employment. The boy did not die either from or with spasm and cannot persuade myself that we shall ever discover in the woorara poison a curative property capable of rousing the vital powers, already oppressedby mysterious morbid actions going on within the frame. Still I am quite prepared to acknowledge that all reasoning, more especially on a subject so obscure as *tetanus, sinks in comparison* with practical observation. Other authorities *may view the subject in* a different light; and it will prove no small accea-

sion to our knowledge should we discover in this or in any other untried agent the power to arrest so hitherto fatal a disease as that which forms the subject of the above brief remarks.—*Lancet, July 7, 1860, p. 1.*

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## 9.—ON THE TREATMENT OF ACUTE TRAUMATIC TETANUS BY TINCTURE OF ACONITE.

Of all the vegetable preparations, aconite is known to be one of the most powerful in its action upon the spinal nervous system; hence its value in tetanus ought not to be inferior to the many other anodyne remedies which are occasionally employed. In the pages of this journal (vol. i., 1846, p. 484) we have recorded an instance, under the care of Mr. Page, at the Cumberland Infirmary, Carlisle, in which a recovery took place under the use of Fleming's tincture of aconite. Another example also appears (ibid, p. 494) from Mr. Newton, of Newcastle, treated by the same remedy, which proved unavailing, for death ensued. We now give another, in which life was saved by the administration of aconite; but whilst we feel disposed to give due credit to the influence of the drug upon the disease, we can hardly attribute the entire cure to it, when the period at which its employment was commenced is remembered, for the tetanic symptoms had been existing twelve days before it was had recourse to.

We recollect a case of acute traumatic tetanus at Guy's Hospital in the summer of 1858, under the care of Mr. Cock and Dr. Wilks, treated by as many as 110 doses of cannabis indica; but the cure was attributed more to the quantity of wine and beef-tea taken than to the medicine. In Mr. Ferguson's second case, reported to-day, aconite was found ineffectual, and woorara was substituted for it, with, however, no better result.

The patient in the present case, a lad aged 15 years, had always enjoyed good health. On the 30th of August, 1858, while wearing thin boots, he trod on a rusty nail, which pierced the ball of his foot. He withdrew it directly; the wound bled but little, and healed in a few days. On Sept. 6th the boy felt stiffness about the neck and lower jaw, which increased daily, and on the 12th he was unable to open his mouth, and could not walk owing to the stiffness of his back. He was admitted into the hospital on the 16th. There was then well-marked rigidity of the muscles of the neck and jaws, and the risus sardonicus was characteristic. He was unable to turn his head or separate his jaws; abdominal muscles very tense; complains of much pain in the neck and back; has not slept for two or three nights; bowels open; perspiring; pulse 80, moderately full. A hard cicatrix in the sole of the foot was excised. He was at first put upon strychnine, one-tenth of a grain, every two hours; carefully watched; soon diminished to one-twentieth of a grain, but the symptoms continued to increase. Twitchings began in the thigh, and great difficulty of respiration, and finally severe general spasms, with well-marked opisthotonos.

On the 20th, the plan of treatment was changed, and tincture of aconite was given, at first five minims every two hours, then eight minims. This was continued till the 27th. The dose was then administered every four hours, and on the 28th every six hours, and on the 6th of October it was left off altogether. The diet was throughout the most nourishing that could be taken—strong beef-tea, brandy, &c., and the bowels kept open by turpentine injections. From the time the aconite began to take effect an improvement took place in the severity of the symptoms, and he has been steadily and gradually progressing, though very slowly. First the general spasms and opisthotonos ceased, then the convulsive twitchings of the extremities; these lasted till about the 1st of October, at which date he could sit up for his meals, could separate his teeth about half an inch; ate and slept well; face almost regained its natural expression.

Has been slowly convalescing since then. Can now (Oct. 22d) walk about, *and appears in good* health and spirits; only still feels a little stiffness

about the muscles of the back, and cannot yet quite separate the jaws as wide as natural.

He was discharged from the hospital well on Nov. 2nd. — *Lancet, Aug.* 18, 1860. *p.* 162.

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## 10. — ON THE TREATMENT OF HEMICRANIA BY INJECTION OF MORPHIA.

By John K. Barton, M. B., Surgeon to the Adelaide Hospital.

[The following case is interesting, as showing the successful results which in many cases follow the employment of the hypodermic administration of morphia. The patient was a pale anæmic-looking woman, debilitated from menorrhagia. A first attack of hemicrania was cured by quinine, but it returned worse than before, and the same treatment was again tried, and failed. Chloroform and opium relieved the pain, but only for a time.]

Upon the 2nd of March I dissolved a grain of the muriate morphia in a drachm of water, and injected exactly ten drops of this into the subcutaneous cellular tissue of the back of the neck, selecting this place as the patient pointed it out as that from which the pain seemed to proceed. The operation was performed without giving scarcely any pain to the patient, and with perfect accuracy as regards the exact quantity injected, by means of the graduated glass syringe, with a sharp-pointed perforated nozzle, as originally I believe used by Dr. Wood. In about an hour after this injection the pain had disappeared; it returned slightly towards evening, but she passed a good night, sleeping more soundly than she had done since the attack commenced.

Upon the 3rd the injection was repeated, the chloroform draughts being entirely omitted. The note taken upon the next day stated that the pain had entirely disappeared after the second injection, and she had remained free from it until evening, when there was a return of it for about an hour, but that she slept soundly and awoke free from pain — a feeling, as if the skin was sore along the side of the neck and head, remained for a day or two. Upon the 9th there was a return of the pain, whereupon ten drops of the same solution was again injected, with the effect of giving immediate relief, the pain returning at night for about half an hour, as upon the 5th, and disappearing entirely again.

Upon the 12th the patient at her own request left the hospital, being quite free from pain since the last injection, and I have not heard of her since.

The great severity of the pain in this case caused the effect of each remedy tried to be more distinctly seen than in other less marked cases, and no doubt the morphine injection was the first of the many means employed which gave decided, and more important still, permanent relief, for the pain did not return with the same intensity after this treatment was employed, and after the third injection it disappeared altogether. I confess that this was more than I anticipated, for I had tried the same treatment in two or three cases before this, and in most of them I had obtained temporary relief only; but some consideration of the nature of these cases may perhaps explain this. *First*, I employed it in a case of severe and long-continued sciatica in a strumous unhealthy countryman, who had been constantly exposed to wet and cold in cutting turf, and other country labour. Repeated blistering, &c., and cupping had been put in operation before he had come to town, without any effect in removing the pain. The injection was repeated several times, and was always followed by an abatement of the pain, which sometimes lasted for several days. I found it necessary, however, to resort to some other treatment, and gave him the corrosive sublimate in decoction of bark, and under this, with the continued injections, he recovered so far as to leave the hospital and return to the country nearly quite free from pain.

*Secondly. I employed* it in another case of sciatica occurring in a middle-*aged healthy woman, who* had got a very severe wetting. She had sat in

her wet clothes, and shivered before the attack of pain came on. In this case relief was obtained from the injection for two or three hours after it was employed, but no permanent benefit arose from it. Again, I employed it in a case where there was intense pain in the lumbar region, arising from caries of the lumbar vertebræ, hoping to relieve the pain until the issues which had been inserted should begin to have their effect; but without the least benefit, even temporary resulting.

From these cases it would appear that this treatment was of no use whatever in a case where there was active inflammatory disease (caries of bone) producing the pain; that it was of temporary benefit in a case of severe pain, originating in inflammation of the nerve itself; that it was of more decided benefit in a case of the same kind, but in which the inflammatory symptoms were less marked; and finally, that it was of very marked and decided benefit in a case where there was no inflammation whatever, and where the pain was of a purely neuralgic character, depending upon general anæmia.

In such cases as this latter I would therefore recommend its employment very decidedly, ten drops of a solution, such as I employed, with one grain of the muriate or acetate of morphia to a drachm of water, will be found quite enough. Women are more easily affected than men, so that ten drops is as much as ought, under any circumstances, to be injected when the patient is a female. Care should be taken that the needle has penetrated into the cellular tissue beneath the skin before the injection is made, otherwise the solution will flow out when the instrument is withdrawn.—*Dub. Hospital Gazette, Aug.* 1, 1860, *p.* 229.

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## 11.—ON THE HYPODERMIC ACTION OF MORPHIA.

By John K. Spender, Esq., Surgeon to the Eastern Dispensary, Bath.

[The following is an outline of four cases of neuralgia, in which the author administered a solution of the acetate of morphia by the hypodermic method:]

*Case* 1.—A female, aged about 35, of spare and delicate habit, came first under my care in 1857, for acute rheumatism; subsequently, for hemicrania and other quasi-rheumatic pains; and lately, for sharp and persistent pain in the inframaxillary nerve of the left side. I need not enumerate the various devices, local and constitutional, which were resorted to; suffice it to say, that they gave scarcely any relief, even for a few hours. At length, I determined to inject some morphia under the skin, and first gave, according to this plan, ten drops of Pharmacopœial solution of acetate of morphia. This caused sound sleep for two or three hours, which was followed by considerable remission of suffering. On the next occasion, I injected fifteen drops of the Pharmacopœial solution, and afterwards increased it, dose by dose, to twenty-two drops, which was the quantity introduced last Friday evening. The ultimate effect now resulting is complete relief from pain for about three days, and it then returns with less intensity than before. There are particular circumstances about this case which render it unlikely that the neuralgic disorder will ever be cured; but it is noticeable that whenever morphia was given by the mouth, no appreciable effect on the pain was ever produced.

*Case* 2.—The second case was that of a lady, aged about 50, who had for many years past suffered from paroxysmal attacks of neuralgia in the upper maxillary nerve, especially in the spring and autumn. All that I had been able to do, when consulted by this patient on several occasions, was to bring about a gradual removal of the pain by iron and quinine, and thus to alter the condition of blood on which the disease might be supposed to depend. She was always an anæmic subject; and, furthermore, often had her sleep disturbed by being obliged to wait on a sick husband. About a month ago, I was sent for to see her one evening during an intense neuralgic attack. I took my apparatus with me, and injected at once fifteen drops

of the officinal solution of the acetate of morphia. Next day, I found that the operation had been followed by sleep for six hours, with almost complete relief from pain; the only complaint now made being some amount of soreness over the course of the affected nerve. Complete recovery took place; and it is to be remarked that in this case the administration of narcotics in the ordinary way had only stupified the sensorium, without in any way reducing the local hyperæsthesia.

*Case* 3.—My third case occurred in Dispensary practice, and was a poor middle-aged woman, who is suffering from apparently malignant disease of the orbit. The eye-ball is pushed forward, the left side of the frontal bone is raised, and the malar bone of the same side is protruded downwards and outwards. The pain is often of an excruciating kind, and had scarcely at all been alleviated by enormous doses of opium. It was obvious that the utmost that therapeutics of any sort could do must be of a bare palliative kind; but I injected one day fifteen drops of solution of acetate of morphia, with considerable relief, though at the expense of such severe sickness that I was not allowed to repeat the process. The pain, of course, soon returned, and there does not seem anything further to be done.

*Case* 4.—A few days ago I injected ten drops of the solution of acetate of morphia into the areolar tissue of an elderly gentleman who has long suffered from subacute neuralgic disorder, attributable to suppressed gout. The result was an agreeable determination to the skin, and a generally soothed state of the nervous system.

The region which I have always selected for the injection of the solution is the areolar tissue of the upper arm; but the posterior part of the neck would probably answer equally well. Mr. Hunter has described the injecting instrument, and the mode of using it. It consists of a glass barrel, with a pipe made of silver, which screws on and off the barrel at pleasure: the end of the pipe being sharpened like a needle, and perforated on one side by an oblique opening, through which the drops of the solution are expelled. Having charged the syringe with the narcotic fluid, hold it in the right hand at the junction of the barrel with the pipe, and with the left hand take up, between the finger and thumb, a fold of the skin of the patient: having made it tense, let the point of the syringe be passed through it with a quick steady movement, and sufficiently deep that the point can be distinctly felt under the finger. The piston must now be pressed very slowly down with the thumb of the right hand, and the escape of the fluid into the areolar tissue will be indicated by a circumscribed elevation of the epidermis. After the instrument is withdrawn, a small strip of plaster should be placed over the aperture caused by the needle.

According to the best authorities, we should never use more than half the ordinary stomachic dose for males, nor more than one-third for females. The specific action of any medicine is developed with much greater certainty when it is introduced hypodermically, chiefly because the whole quantity of a substance given in this way is absorbed, without being submitted to the dynamic influences of the gastric and intestinal secretions.

I regret to say that *intense sickness* is a frequent and troublesome sequel of the hypodermic injection of morphia. In three of the cases above related this symptom was very severe, and in some measure obliterated the sense of relief otherwise obtained. In one instance, a temporary cutaneous eruption, like urticaria, was also noticed.

A due discrimination of cases ought to be made for this plan of treatment. To practise it in every example of neuralgia would be unwise and useless; for nerve-disorder often depends upon such trivial causes as an error of digestion or over fatigue, and vanishes spontaneously when the exciting agent is removed. The cases in which success can most probably be hoped for, are those in which the system has been long worn by pain, and in which a general spanæmic condition exists. The administration of morphia in the hypodermic method appears then to act as a narcotic shock, which powerfully and almost instantaneously soothes the whole nervous system, and *destroys the local hyperæsthesia*. Sometimes this result is permanent, some-

times not, according to the permanent or temporary agency of the causes of the disorder. But however short the interval of ease, it ought to be very welcome; for I believe that the great hindrance to our successful treatment of neuralgia has been our obstinate theorising about its etiology. While we are busy in classifying our supposed *materies morbi*(no classical phrase has ever exercised so complete a tyranny over us), and are applying our chemical and dynamical drugs accordingly, we are apt to forget the simple clinical fact that a human being lies in agony by our side, and supplicates relief for that one symptom—*pain*. Let us, if we can, first utterly overwhelm and abolish this pain; perhaps it will never return; but at all events, when our patient is in a grateful stupor, there will be time to philosophise about the origin of the malady which we are called upon to subdue.

It must be admitted that there are certain trifling practical difficulties in the way of the hypodermic administration of medicines. The introduction of a pointed instrument under the flesh causes a momentary pang, which cannot be evaded. We may liken this pang (as we can with perfect truth) to the prick of an ordinary needle; but the infliction of a punctured wound even of this simple kind may be a terror to children, and to nervous adults of both sexes.—*British Med. Journal, June* 9, 1860, *p.* 437.

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12.—*The Treatment of Neuralgia by Powerful Currents of Electricity.*—In a paper read before the Academy of Medicine, M. Becquerel strongly recommends the treatment of neuralgia by this method, which has proved highly successful in his practice, statistics being furnished which show that nineteen cases of sciatica, and twenty-three cases of lumbar, intercostal, crural, and trifacial neuralgia, many of which were of old standing and not amenable to other remedies, were all perfectly cured by the judicious use of powerful electric currents.

The principle upon which this mode of treatment depends is as follows:—If intense currents of electricity are applied in the course of a nerve, its sensory power is benumbed for a time; an analogous effect is produced if an electric shock be communicated to a nerve which is the seat of neuralgic pain, the morbid sensibility becoming diminished and annihilated under the influence of strong currents repeated at rapid intervals.

Sometimes the disease disappears entirely after a single operation, but it more generally returns after a variable period of from a few hours to a day or two. As the electrisation is repeated, the recurrence of the neuralgia is always at greater intervals, and the pain becomes less intense, until, finally, a cure is completed, for the perfection of which from three to fifteen applications, each of two to five minutes' duration, are necessary.

In employing this treatment in cases of trifacial neuralgia great precaution is requisite, as otherwise, either cerebral congestion, or obstinate headache, never in themselves dangerous, but yet alarming and distressing to the patient, may occur.—*Bulletin de l' Académie.*—*London Medical Journal, July*, 1860, *p.* 48.

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## 13.—ON CHRONIC ALCOHOLISM.

By Dr. Wiltshire.

[A good deal of discussion seems to be going on respecting the exhibition of alcohol in such cases as follow. Both sides are right. Practice, after all, and a long list of successful cases, induce the older physicians to bear two things in mind in these cases: 1. When a patient has an attack of delirium tremens, or is threatened with one, in consequence of drinking for a week or two to excess, it is best to withdraw all stimulants, and not to give opium—the case will right itself. 2. When a patient has for years almost lived on alcohol, and it is now from some circumstances withdrawn, it will most likely end in delirium tremens. The first case is caused by drinking alcohol, the second by suddenly withdrawing it. The two causes are quite

different; consequently in the second case we must carefully give small doses of alcohol and as much nutrition as the stomach will digest. We think that those who oppose this opinion will some day be obliged to alter their views. The following cases would seem to contradict us, but all we maintain is, that although you may do without stimulants in many cases, and all the better if you can, yet in many other cases you will lose the patient if you follow this out strictly.]

The notes of the following case were taken by Mr. C. Bennet, clinical clerk, Charing Cross Hospital.

Edward S., aged thirty-six, well educated, now an attorney's clerk, but formerly in India and China as a soldier for ten years. He there had dysentery twice, and what he supposes cholera. When abroad he drank very hard; indeed, the fleas and insects annoyed the soldiers so much that they were obliged to get drunk to enable them to go to sleep. At his period of greatest drinking he took, perhaps, one quart of spirits in the day, besides any other thing he might happen to get hold of; has seen some of his comrades drink a pint of spirits before going to bed; sometimes they were found dead in the morning; has seen some of his officers drink a pint of spirits in the afternoon. He had himself sleepless attacks, with delusions, when in India; but he maintains he never had delirium tremens. In 1849 he came to London; still drank pretty hard; was taken ill at Croydon, when his medical attendant stopped his drink. His friends had a physician from a well-known London hospital to see him, who said the medical man was killing him, and ordered him brandy and water. Two years ago he had typhus fever. He was then drinking hard, chiefly gin, and rum in his tea of a morning; at eleven he took some more. He next took stout at his dinner. In the afternoon he again took some gin or rum; and in the evening he had some porter, with, what he calls, a stiff tumbler of grog. He was then, he states, well for three months. He then began to feel very ill and wretched; his hand was unsteady; was obliged to resign his duties. Having no provision for his family, he got some law-reporting to do for the cheap journals. At last, however, he could not write; he could not eat; he had bile in the morning, and constant belching; the bowels were rather irritable; he got yet more shaky; required more stimulants; his face became bloated; began to waste about the legs; had involuntary seminal emissions without erections. At length a large patch of eczema impetiginodes broke out upon the right hand and forearm. He now came to the hospital (Oct. 18th, 1859) upon the advice of a medical man and a lawyer's clerk whom he knew. He trembled a good deal, had a coated tongue, congested eyes, and was (as he has since informed Dr. Willshire) scarcely able to get into the room from debility and nervousness; and that when he was told to sit down he was glad, as he could not have stood a minute longer. He had been drinking latterly just as much as his means would allow him to procure; he was firmly impressed with the idea that he must have spirits, and when told that he must not have a drop, he fancied he must die. Dr. Golding retained him for two or three days after he came into the hospital, and then transferred him to the care of Dr. Willshire. All fermented fluids were strictly forbidden, full diet given him, and a mixture of carbonate of ammonia for a few days. Poultices, lime-water, powdered calamine, and zinc ointment were used to the hand. All the skin came off like a glove from the hand and fingers, and, by the 10th of November, the hand was almost well; he had gained flesh, all trembling had left him, had plenty of confidence in himself, and felt altogether a different being. Latterly he has been taking quinine and sulphate of zinc, with tincture of valerian, purgatives, and as much as he likes to eat.

Nov. 17th.—Improves; complains of not sleeping, however, since he has left off the morphia pills; hand and arm comparatively quite well.—*Lancet, Sept.* 8, 1860, *p.* 237.

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*14.—Chronic Alcoholism, with impending Delirium Tremens, treated by Suspension of the Stimulus.*—(Under the care of Mr. CANTON. —For the

notes of this case we are indebted to Mr. J. Reeve Shorto, late house-surgeon to the hospital.

James W——, aged thirty-six, married, a general dealer, was admitted into the Charing-cross Hospital on Nov. 14th, 1859. With the exception of what is mentioned below, he has always enjoyed tolerable health. His habits, however, have been most intemperate, he being accustomed to take eight or ten glasses of gin daily, besides two or three pints of porter, and three-quarters of an ounce of tobacco for smoking. He states that he lives well, and takes meat daily, excepting when he more than usually exceeds in drinking, when for a week or so he is unable to take any solid food, but confines himself to slops, broth, &c. At these periods also he is exceedingly nervous. About two years ago he was admitted into King's College Hospital with idiopathic delirium tremens. He was there treated with brandy, beer, and opium. He remained in the hospital twelve days. Nine months afterward he had another attack, and six months ago a third, for both of which he remained at home, and treated himself by freely taking spirits and beer.

Nov. 14th. For the past week he has been, as he denominates it, "out on the drink." During the whole of to-day he has been indulging, and he has taken seven glasses of hot rum, besides gin and beer, and incessantly smoking. He has taken no solid food to-day. Toward evening he got into a row, and received a wound over the temple, which bled rather profusely. He applied at the Charing-cross Hospital for this, and, his wound being dressed, he was admitted an in-patient under Mr. Canton, who prescribed an opiate. In appearance he is the very picture of a man who is addicted to intemperance. His face is bloated; his eyes are expressionless, and have a peculiar unsettled stare; his hand trembles very much, as also his tongue (which is coated white), and his whole aspect betokens the enfeebled constitution of a confirmed drunkard.

15th. Very feeble. He has obtained no sleep. He spontaneously assures the house-surgeon that he apprehends another attack of delirium tremens if he is not allowed some spirits; and when told that it was intended that none should be given him, he was much astonished, and feared he would not recover. Ordered ammonia and bark, with chloric ether and opium, every four hours, and, as his stomach was irritable, some beef-tea; to have an opiate at bedtime.

16th. Better. He has had some sleep. He is not so nervous, and is more tranquil in his mind. To have full meat diet daily, but no beer or spirits. Ordered a grain and a half of quinine, twenty minims of chloric ether, and fifteen of tincture of opium in water every four hours.

17. Feels much better. He says he experiences the same sort of relief from taking this quinine mixture as he usually does from imbibing his dram of spirits.

21st. Still improving. The wound in the temple is almost well. The nervousness he complained of has almost departed.

A few days afterward he left the hospital cured.—*Lancet, Sept.* 8, 1860, *p.* 237.

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15.—*Three cases of Delirium Tremens, treated without Stimulants; Fatal Result in Two.* (Under the care of Dr. ALDERSON and Dr. SIBSON.)

In the annexed cases no alcohol stimulants were given after the patients were admitted into the hospital, as will be seen by the details, but some one of the preparations of opium, combined with ammonia, instead. In two a fatal result ensued, probably mainly brought about by the mechanical restraint employed. The prohibition of this terrible source of exhaustion cannot be too stringently enforced; for, as was recently observed in some clinical remarks on this disease made by Dr. Brinton at the Royal Free Hospital, "no matter to what degree reason may be degraded or dethroned, the delirious patient always seems sensible of the debasement and brutality implied by straps and bands." (Lancet, vol. i., 1860, p. 592.) If attendants cannot

be procured to gently restrain and watch the patient, it were better under such circumstances to administer chloroform, which we have known to do more good than all the strait waistcoats; and we have recorded cases which sleep having been induced by its employment in the London Hospital, terminated in recovery. (Lancet, vol. ii., 1858, p. 449.) The notes of the subjoined cases were kindly furnished by Dr. W. H. Broadbent, medical registrar to the hospital.

Robert S——, aged forty-five, butler, was admitted into St. Mary's hospital, under the care of Dr. Alderson, May 16th. He had been engaged in his occupation as butler up to the date of his admission, enjoying good health, but drinking largely. When brought to the hospital, his countenance had a wild expression, there was general tremor, and he was apprehensive of some intention to injure him on the part of his fellow-servants; the skin was perspiring; pulse quick; tongue white and creamy. He was ordered two grains of opium at once, and a grain every hour till sleep should be induced.

May 17th. In much the same condition; can answer a question rationally, but is unable to fix the attention on any subject. The following mixture was substituted for the crude opium: sesquicarbonate of ammonia, four grains; solution of morphia, twenty minims; tincture of lavender, fifteen minims; and acetate of ammonia mixture, one ounce, every four hours.—Eleven p. m. Very noisy; sweating profusely; in other respects about the same. As it was found that he and another patient suffering from phrenitis were mutually exciting each other, and disturbing the wards by responding in loud shouts, he was removed to a separate ward. During the night he became violent, and the attendant, without consulting the house-surgeon, put on the strait waistcoat. At four a.m. the house-surgeon was called. His struggles had suddenly ceased, and he was found unconscious; pupils fixed and dilated; surface cold and clammy; slight convulsive movements of the extremities, and twitching of the mouth; pulse imperceptible; respiration suspirious. He died in ten minutes.

At the post-mortem examination nothing was found beyond slight subarachnoid effusion.

Thomas C——, aged thirty-six, admitted, under the care of Dr. Alderson, June 4th, 1860. He is a pipe-maker, and, in carrying round his pipes to public-houses, has been in the habit of drinking much gin and beer. He has had several attacks of delirium tremens. When admitted, had not slept for two or three nights, but had imagined that people were jumping about outside his window, and deriding him. Hand and tongue unsteady; skin cool; pulse 88; tongue white and moist; no actual delirium. Ordered two ounces of compound senna mixture, to be followed by twenty minims of solution of muriate of morphia, with three grains of sesquicarbonate of ammonia in camphor mixture, every four hours. He had a pretty good night, continued to improve, and on the 12th was discharged cured.

Thomas M——, aged thirty-four, admitted, under the care of Dr. Sibson, March 16th, 1860. For the last six months has been suffering from cough, with pain in the side and expectoration, and he has been in the habit of taking half a pint at least of brandy daily. When admitted, he was agitated, tremulous, apprehensive, but not delirious. Skin moist; pulse 90; tongue white and moist. He was ordered ten grains of colocynth pill with calomel, and mixture of the acetate of ammonia, with excess of ammonia; at night he had forty minims of Battley's solution, and twenty minims every three hours till sleep was produced. Diet, broth, with a pint of porter.

March 17th. He had a little sleep in the night, and seemed about the same. The opium to be repeated at night.

18th. Did not sleep, and now insists on going home, imagining that some mischief to him was intended. He was easily persuaded to return to bed; but he required constant watching during the day, and toward evening became violent. Battley's solution to be given every two hours.

19. Very much excited, and with difficulty kept in bed; imagines that he *is to be punished for some* crime. Hands tremulous; skin perspiring freely; *pulse 98; respiration* 24. As he became more and more violent, and as he

was an exceedingly powerful man, and could not be restrained, he was removed to a separate ward, and the strait waistcoat was put on. His efforts to free himself were fearful, and though the opium was increased in quantity, and given at shorter intervals, he had no sleep.

At about six a.m. on the 20th he ceased to struggle, and seemed to sleep; but it was soon seen that this was not sleep, and before the house-surgeon could be called he was dead.

On post-mortem examination, both lungs were found universally adherent and greatly congested. There was slight subarachnoid effusion, and both lateral ventricles of the brain were greatly distended by fluid.—*Lancet, Sept.* 8, 1860, *p.* 238.

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## 16.—ON DELIRIUM TREMENS.

By Dr. JEFFERY A. MARSTON, Assistant Surgeon. Royal Artillery, Malta.

[The following paper by Dr. Marston is a most sensible one, and they who are now treating all cases of delirium tremens without alcohol will do well to consider it. Few are more strenuous advocates of total abstinence from all stimulants than ourselves in cases of health, but in certain diseases we have found it indispensable to carry the patient over certain periods of exhaustion. Dr. Marston says:]

First of all, let us see that our terms are precise and clear. I would say that there are three separate and distinct forms of the disease in question: that if any one plan of treatment be pursued in all, and if, without reference to their distinctive features, they be individually and severally heaped together under one head, we can obtain no reliable data. Not a little misconception, it seems to me, has arisen from this very source:

I. Delirium e potu, or Delirium Ebrietatis properly so called.

II. Delirium Ebriosorum.

III. Delirium complicated with { Renal, Hepatic, Gastro-Enteric, Cardiac, Central } Diseases.

Professor Todd, in one of his clinical lectures, has some admirable remarks upon the two first forms, and their distinctions. The first I would illustrate thus:—It happens in the younger and more acute drunkards (if I may so term them). The disease follows quickly after a debauch—within 24 or 48 hours. The symptoms are—the tongue very foul and tremulous; great headache; face rather flushed; tenderness often upon pressure of the epigastrium; nausea; sometimes vomiting; perfect anorexia; sleeplessness; tremors; hallucinations; illusions; restlessness; an excited manner; a quick, soft, and tolerably full pulse; and often there is present a smell of spirit. Now, these cases are by far the most frequent. The disease occurs in a man whose means prevent a regular steady soaking, but in one who drinks very hard whenever his pocket allows it. However frequently this may be, there is always a good and distinct interval—weeks or months. The subject of the disease goes in for a heavy night or so at a time—his money is exhausted—he does or does not go to prison, but at any rate he does not drink again for some period, for the best of all reasons—want of means. Here is the ordinary form of the disorder: An acute alcoholism—the drink being *in* the man. An emetic-purgative, with quiet and repose for two or three days, sets him all right again.

The second form is a delirium of drunkards, in contradistinction to a delirium from drink. The illustration of this form will be:—The subjects of it are older—have the outward and visible signs, and bring the history of a habit of drinking; hard drinking indeed, if the aggregate be looked to—the steady weekly consumption of spirits, to wit—but less hard than the first variety in a given space of time. Those men have kept out of the guard-room, and are shrewd enough to keep aloof from their officers; bear a good

regimental character; are seldom, if ever, in hospital; and although long suspected in the regiment of being secret drunkards, yet are only proved so by some accident—as turning out at night to a fire—or admission into hospital for some trivial disease. At last he is caught, and lodged in the guard-room; or by some means or other he is with all suddenness deprived of his drink; and delirium tremens sets in, appearing from the second to the seventh day after confinement, while perchance the man is awaiting his court-martial.

In short, you get the history of a man who has drank for years and years; during which time he has performed his duty under sharp supervision, and has not suffered from any disease. He is deprived suddenly of his stimulus, and takes delirium tremens.

*Complicated Form.*—This variety will take in the various symptoms of any organic disease present, and complicating the case. It would be impossible and needless to enumerate the various complications, further than to remark that their recognition and diagnosis is all-important; the difficulty of their treatment very great; and that the mortality far exceeds that of the other and simpler forms of the disease.

The visceral or glandular derangements become evident for the first time during the attack of delirium tremens—the man never having been before in hospital,—and are then only arrived at, from the fact that some unusual symptoms, as convulsions, jaundice, œdema, persistence and peculiarity of the delirium, or albuminous urine, make them apparent.

The treatment of such cases must necessarily be modified according to the diseased state and its indications.

Having premised this much, let us turn to the points in dispute, which appear to be: That the theory of the causation or etiology of the disease hitherto propounded is wrong, its pathology wrongly stated, the indications for its treatment misunderstood, and the special modes of treatment themselves (particularly that by opium and stimulants) have been erroneously and injuriously pursued.

It is often better to watch than to correct, and I would ask how many cases classed under the first form have been enumerated with the second and third varieties? I fancy that by far the larger number of the so-called statistics consists of individual cases of the first form, and if so, the generalization from them cannot fail to be vitiated, when applied to the whole disease.

Dr. Watson expressly says, that some cases occur after a long debauch, and others in which the patient has not abstained, but is continually fuddled; and here the disease arises because the man goes from his ordinary positive to the comparative degree.

There is no doubt that those cases are numerous, and their plain inference is "poisoning;" but so much has been made of them, as virtually to exclude the occurrence of other forms.

*Etiology.*—Taking the objections seriatim, the etiology; Watson says, the predisposing cause is drink; the exciting cause, the privation of it. Against this view there is urged the frequency of the disease following a debauch, &c., without any privation; in short, the frequency of the first form. Such frequency it is argued, proves the disease to be the result of poisoning, and the sufficiency of the poison alcohol to form at once the exciting and predisposing cause. The occurrence of some disease, such as catarrh, influenza, gastric disturbance, in a drunkard, may be also an exciting cause. Lastly, the statistics of prisons are against the theory, it is said, of privation. The first objection is shortly this, that we have proof of one cause (toxæmia from alcohol) being sufficient, and hence it is unphilosophical to seek a second. I can only meet this by stating, that there are different forms of the disease, which may have different causes, nay, a plurality of causes. Besides, I shall adduce positive evidence of the disease following the withdrawal of alcohol. With regard to prisons, I can only speak of military ones. No cases occur in them, I am aware, and am not surprised *at the fact; but they* occur antecedently, coming on in the guard-room, *where, if a man be tried,* he awaits his court-martial, probably for many

days; and I state as a fact, that, cases of delirium tremens commonly occur from the first even to the seventh day after the privation of liquor. The guard-room statistics would hence be every bit as strong the other way. I am aware that statistics do not settle the fact, they do not prove cause and effect, viz., deprivation of accustomed stimuli as a cause of delirium tremens; but they cannot certainly be urged against it, for they support at least the view of this privation being frequently an antecedent to the appearance of the disease. Is it a necessary antecedent? I believe it is in some cases. Take the following:

Sergeant D., æt. 89 years,—in daily expectation of a good-conduct medal.—Admitted April 13, suffering from a small boil upon the lobular appendage of the right ear; in perfect health apparently. Upon the afternoon of the 14th I noticed that he was very tremulous and nervous, and asked him privately, about his habits. He denied, in the most positive and awful terms, that he drank hard, and refused my offer of a glass of brandy. Upon the morning of the 15th, I learnt that he had slept badly, and found him suffering from a decided attack of delirium tremens. He talked incessantly, was bathed in perspiration, had illusions, and fancied he saw strange animals, and heard strange noises. His tongue was coated, and very tremulous; the eyes ferrety, the pupils moderately contracted. He was ordered a sharp purgative, and cold douche to the head, followed by a basin of beef-tea. At 12 noon he took ʒi of laudanum, but was sick, vomiting nearly it all; at 2 p. m. he had some warm brandy and water, with ʒjs of laudanum, which he kept down; about 3 p. m. he had more beef-tea, and said he thought he should sleep, lying down for that purpose; I happened to be in the ward at the moment, and my attention was arrested by his livid face and heavy breathing. In about a quarter of an hour, he had an epileptiform convulsion; two or three followed after a short interval, and in about three-quarters of an hour he died with symptoms of apnœa. Besides cold douche, artificial respiration by Marshall Hall's method, and enemata of brandy, were tried without avail. The *post-mortem* was made most carefully by myself, and I could detect no organic disease to account for his death. The left ventricle of the heart, however, was in an advanced degree of fatty degeneration; and, besides some venous congestion of the membranes of the brain, there were a few drachms of fluid in the cerebral ventricles.

Now, I would remark how strong is the tendency of the evidence, negative and positive, of the facts here. A man is entitled to a good-conduct medal—which presupposes eighteen years' absence from the defaulters' book as regards courts-martial. I learnt from his wife' subsequently, that for six or eight years he had drank very hard, and that, although she had never seen him drunk, yet she could not say that he was ever perfectly sober any night; that she did not remember his having ever been in hospital, or suffering from any disease. He comes into hospital, is deprived of his liquor directly, and in fifty-six hours is dead. This appears to me a strong case, but it is not by any means an isolated one; and others equally positive will be cited, far more than sufficient to meet the logical requirements of one grain of *positive* against a bushel of *negative* evidence.

*Pathology.*—Dr. Watson's views may be epitomised thus:—The disease is "Exhaustion with nervous irritation;" the remedy, "sleep." Against this is urged the toxæmic view, and the positive chemical evidence of the presence of alcohol in the cerebral ventricles, and that sleep is an effect and sign of the improvement in the disease, and not the cause of it.

Dr. Watson uses his terms advisedly. He does not pass them for more than they are worth. Are there not certain acquired physiological conditions or states of system which produce uncontrollable cravings and desires? and are not these cravings instinctive desires of a need felt by such an abnormal system? and supposing them unyielded to, do we not get nervous exhaustion and depression? Is it true that horses fed upon arsenic fall into a bad state of health, when its exhibition is discontinued? The facts related of the Styrian and Hungarian peasantry relative to their frequent use of ar-

senic, and the evils which almost invariably arise from its discontinuance; the almost universally spread instinct in man to the use of narcotics, stimulants, &c. (such as opium, betel nut, tobacco, cocoa, with the rest); the growth of the custom into a habit, and the way in which men are impelled to the continuance of that habit, from the fear of the chain of morbid phenomena which follow the cessation of it;—do not these run very parallel to the facts we observe from the use and abuse of alcohol? Chossat's experiments upon the effect of starvation on animals would indicate that the nervous tissues undergo remarkably little relative loss compared with the other tissues, in spite of the great quantity of fat they contain, and their almost fluid consistence. This has been held to explain the cause of the curious psychical phenomena preceding death by starvation. Restlessness, delirium, and prolonged sleeplessness, are common precursors of death in such cases. Now it will be said, that no analogy can be established between such cases and the same phenomena following the withdrawal of alcohol. But I am not convinced of that. The whole doctrine of diets, however satisfactorily it may be settled of a chemical basis, is decidedly not settled upon equally certain physiological data. We have facts in abundance to prove that the chemical value of food is not the physiological one, and that both man and animals live and grow upon substances, and in proportions, perfectly different from what a chemical view would indicate, or conceive possible. When we observe how spirit is meat and drink to a drunkard; how his system affords the proof of an altered and abnormal nutrition; how, in short, he has a special physiology of his own, it seems to me a natural and rational consequence, that the poison to him is no poison, but, on the contrary, a special fuel for his nutrition and development,—albeit diseased. Liebig has shown how the chemico-physiological theory of the action of stimulants upon the human system, is in accord with the actual experience of landlords and others, viz., that a far larger amount of food is consumed by the abstainer from alcoholic fluids, than by one who partakes a moderate quantity of them. It seems to be a well-ascertained fact, that alcohol economises the food and tissues, by arresting the amount of secondary metamorphosis. Then we have the influence of the custom producing a habit, and its known effects upon the body. Can any habit be suddenly discontinued and broken without some, nay, even a grave effect upon the nervous system?

There seems to me to be no end of evidence to prove that the sudden curtailment or withdrawal of any habit may produce nervous exhaustion; and if so, why, irritability is a necessary concomitant and index of that condition!

In regard to sleep. A drunken sailor knows very well that if he can "sleep it off," it is the best and most natural way of terminating his fit. No doubt the tendency to sleep is a sign of improvement in a case of delirium tremens. Dr. Watson and others urge that sleep is the necessary precursor of improvement; but if I understand the objection raised to this view aright, it amounts to this, that before a patient recovers from delirium tremens, and as a sign and effect of his improvement, he sleeps—very probable; but I am sure the *post hoc*, is often a *propter hoc*, viz., not that he sleeps after he improves, but he improves because he has slept.

*Case.*—G. S., aged 30, admitted June 8, 1858, from the guardroom, where he had been confined two days. Had been for years a hard drinker. It was stated (but not upon reliable evidence), that prior to enlistment he had been confined for a few months in a lunatic asylum. Upon inquiry, it was found that he had been drinking very hard for some days, and that the debauch terminated about three days prior to his admission into hospital. He was laboring under all the symptoms of unmistakeable delirium tremens. After the administration of a purgative and a saline antimonial mixture for twenty-four hours, without any amendment, opium in grain doses was commenced, and continued until his pupils became somewhat contracted, when it was omitted. He was allowed milk diet, with a basin of soup at bed-time. *Forty-eight hours having* elapsed in hospital without any sleep, and his delirium, tremor, and symptoms of exhaustion augmenting, it was determined to

give him chloroform, for which purpose another assistant-surgeon with myself, alternately, sat at his bedside all night: and he was kept, at intervals, under its influence for eight hours, during the greater part of which time he slept soundly. At the expiration of this period he awoke, and partook of some broth. He appeared far less tremulous, had lost his rapid delirious conversation, but retained his suspicious manner, and was evidently haunted by illusions. After some interval he was again put under the influence of chloroform, and slept for four hours deeply. Awaking, he was still more rational and restored; and after drinking two bottles of porter, he spontaneously fell asleep. In this state he continued six hours, and awoke rational and well. He was retained in the hospital for some period, on account of some dyspeptic symptoms and boils, and with the view to invaliding on account of his uncontrollable habit of drinking. This man was a dipsomaniac indeed. I think I never beheld features so expressive of a true drunkard in my life. During his fits he seemed to have laboured under certain dominating passions, and these had left such impressions upon the facial muscles as to have permanently altered his whole physiognomy.

Now, here we have a case of delirium occuring in a chronic drunkard, who had been deprived of his stimulus for two days. He is admitted into hospital, and does not sleep, his symptoms becoming worse; by the aid of chloroform he slept for six or eight hours—improves—and by the administration of more chloroform with the aid of porter, he sleeps again, and is cured.

A. B., a mess sergeant in a line regiment, had always been suspected of drunken habits. One day he absconded with some money, was caught, and confined to the guard-room. Whilst there, he became the subject of delirium tremens, and was brought to hospital, with the history of having endeavoured to poison himself with arsenic. This case was a very severe one, for the patient had an ever-present sense of his crime, a fear of punishment, and was, moreover, suffering from all the symptoms of the disease. Numerous means were used (including opium) without any benefit, and a fatal prognosis was formed of his case. The regimental surgeon administered chloroform by inhalation, and procured artificial sleep for many hours. The man awoke so much improved, that its administration was recommenced, and he was quite restored by its aid.

Here we have the symptoms continuing for a certain period without any improvement, while induced artificial sleep or narcotism was attended with such marked improvement, as to have impressed the medical attendant with the belief that his patient owed his life to the chloroform.

It would be tedious to cite other cases proving the same thing. I conclude from them, that to procure sleep is sound, excellent advice, the good effects of which are borne out by actual experience.

*Treatment.*—The use of stimulants (alcoholic) in cases of delirium tremens, with the view of removing the exhaustion, allaying the morbid irritation, and procuring sleep, would be looked upon as even more wrongly directed. If the etiology usually propounded be radically and totally erroneous, then we are guilty of adding more poison to an already poisoned blood, to procure what is not needed, and what the presence of alcohol in the system is preventing—sleep.

G. M., aged 45, but grey, and looking much older, was admitted into hospital for some gastric disorder. This man was, and had for years been, a very hard drinker, and his features, particularly the nose, indicated "potations deep." One morning (after he had been in hospital for some days) I was called to him, He appeared insensible, was breathing very heavily, the face and lips dark and turgid, the pupils contracted. Whilst examining him he had a convulsion of a tetanic character, the body being arched in the position of opisthotonos. The muscles of the fore-arm were so tense that the radial pulse could not be felt. The hearts' impulse was scarcely perceptible. Having thrown a bucket of cold water over the head, I took advantage of a few moments of apparent consciousness to pour a glass of hot brandy and *water into his stomach*; and he recovered almost immediately,

so much so, that the medical officer, whose patient he was, could scarcely credit the state in which I found him.

G. P., aged 36, admitted April 14, 1858, from the guard-room, where he had been confined two days. His disease was unequivocally delirium tremens, and no remedial measures seemed to benefit him. He was tremulous, restless, delirious, and did not sleep even with the aid of the fresh administration of opiates. Two bottles of porter were given him at bed-time, and he was observed to smoke during the day some strong tobacco. His hands were so tremulous that the orderly had to hold the pipe in his mouth. By these means he slept, and slept well, and made a good recovery, after having been dosed unavailingly for four days previously to procure that result.

At this moment I have a soldier in hospital who has always drank freely. He has been under treatment upwards of eight days for trivial bronchitis, and symptoms of incipient delirium tremens appearing now, necessitate the use of alcohol and opium with manifest advantage to the man, as regards sleep, appetite, and the disease itself.

These cases illustrate two points: the actual occurrence of delirium tremens after and during the privation of liquor; and the speedy removal of the symptoms by the re-establishment of the custom, when other means failed.

I have purposely selected these cases, because they illustrate also the fact, that whilst the men were taking their accustomed stimuli, they continued well, for they were men who never appeared at hospital at all.

I shall not give cases of apparent cure by the administration of opium, as they can be found in any work; and, after all, they are no proof that your patient recovered by the treatment, but, perhaps it will be urged, in spite of it.

The most curious cases are those in which the delirum continues for a long period, but in a modified degree; the patient sleeping tolerably every night, eating and drinking, and performing all his functions well. It is well to look out here for some complication (particularly renal or hepatic disease), for it is surprising how small an amount of urea circulating in the system may give rise to a persistence of anomalous symptoms. Besides the uræmic, we have a peculiar and difficultly treated form of the disease when jaundice is present, whether arising from fatty degeneration, cirrhosis, or other hepatic disease. These cases, of course, are more frequently fatal; but I find that, after local depletion, purging, or diuresis, stimulants, more particularly gin, are not only not contra-indicated, but are decidedly useful, more particularly if the patient be an old chronic drunkard.

The most fatal form by far is that in which we have delirium tremens occurring in a person already the subject of typhoid fever—cases by no means uncommon in this climate. Having separated, however, these cases, there will remain many in which a chronic derangement of the nervous system is manifested, the patient sleeping night after night, for longer or shorter intervals, and performing all his functions well.

In a few cases, opium given in full doses at bed-time will secure a deep sleep and manifest improvement. In others, a liberal but regulated allowance of stimulants will prove advantageous, when everything else has been tried in vain. In some, no plan of treatment will succeed, although the patients frequently recover after a long interval, while others lapse into chronic mania, melancholia, or some form of insanity, ending their days in a lunatic asylum.

With reference to that singular phase of our mental life-sleep, Sir Henry Holland advances views, which my observation of the sleep of delirium tremens patients has led me to think perfectly truthful.

He is of opinion that sleep is not a unity of state, but a series and succession of states, ever varying from moment to moment. These variations having every degree of diversity, from complete wakefulness to the most perfect sleep of which we have cognisance. It has long occurred to me that *the sleep of drunkards* and in delirium tremens differs much from the normal *standard of intensity*. Every one must have experienced in his own person,

when he was anxious to awake at a certain hour, how he awakes at that time with a feeling that he has not slept well, or at all, although he may be assured that he has slept very soundly.

Sir H. Holland's observations are so good upon sleep and dreaming, in relation to delirium and insanity, that I shall quote his words:—"I know of no principle so capable of affording a guide, as that which views all the forms of insanity, including delirium, in their relation to corresponding healthy states of mind, tracing this connection through those intermediate grades, which are so numerously exposed to us in the various conditions of human existence. The diversities of mind in what is accounted its healthy state, the effect of passions in suddenly altering its whole condition, of slighter emotions in gradually changing it, and of other incidents of life in affecting one or more particular faculties; its subjection periodically to sleep, and casually to the states of intoxication, somnambulism, and reverie; its gradual transition in fever from a state where there is conciousness of vague and wandering ideas to the state of perfect delirium;—all these furnish so many passages through which we may follow sanity into insanity, and connect the different forms of disordered intellect as well with each other as with the more natural and healthy functions of the mind."

To sum up, I would say that the first and most frequent form among soldiers of this disease requires rarely indeed opium, particularly at the commencement of the attack; in short, no specific treatment is necessary. In the second variety, I would give it cautiously in moderate doses, after free purgation, provided I did not find my patient improving by rest and tranquility. The opium had better be given at any rate in a full dose before the accustomed hour of sleep. Should it not succeed, my experience would indicate a "hair of the dog that bit him," in the shape of porter or hot brandy and water, spite of what has been urged to the contrary. If the surgeon avoid both opium and stimulants, and his patient goes on badly, depend upon it the chances are in favour of another doctor advising one or both of these noxious agents, with much advantage to the patient, to the no little chagrin of the first medical attendant. Where great irritability of the stomach is present, there is nothing better than a sedative dose of calomel (gr. vj.), with or without opium, and a large enema.

Supposing the physiological effects of opium upon the system to be manifested, without sleep or improvement following, I should omit its use for some hours, give my patient some good broth, flavoured with brandy or wine, and induce artificial sleep at night by the aid of chloroform.

I trust it will be seen that the use of opium is advocated as a measure requiring discrimination and caution, but as a reliable one in many instances.

Of course the complicated forms require that the greatest discretion should be exercised in its administration. Where an embarrassed circulation exists, marked by venous congestion of the mucous membranes and duskiness of the face, it is better avoided altogether. Pulmonary emphysema, if extensive, cardiac disease, or indeed any thoracic complication, will require also great care, if they do not indeed prohibit opium in any form.—*Edinburgh Monthly Journal, Oct.* 1860, *p.* 306.

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## 17.—TREATMENT OF DELIRIUM TREMENS BY DIGITALIS.

By G. M. Jones, Esq., Dr. Ballard, Dr. Williams, and Dr. Webster.

[Mr. Jones, of Jersey, treats these cases on a novel plan. His opinion is always valuable. From most other people we should dissent if they gave such an opinion, but Mr. Jones is truly trustworthy. He was led to give digitalis from mere accident. He says:]

As to the dose, experience has taught me that the best dose is *half an ounce* of the tincture given in a little water. In some few cases this one dose is enough, but generally a second dose is required four hours after the first. In some cases, but very seldom, a third dose is called for; but this

hardly ever need exceed two drachms. The largest quantity I have ever given was *half-an-ounce* at first, *half-an-ounce* four hours afterwards, and another *half-an-ounce* six hours after that—making an ounce and a-half in ten hours.

As to the effect of these doses, my impression is that the action is on the brain, not on the heart. The pulse, so far from being lowered in force, becomes fuller, and stronger, and more regular, soon after the first dose. The cold clammy perspirations pass off, and the skin becomes warmer. As soon as the remedy produces its full effect, sleep for five, six, or seven hours commonly follows; sleep is the guide as to the repetition of the dose. No action on the kidneys is evidenced by any unusual secretion of urine. Sometimes the bowels are slightly acted on, but not commonly. I have never once seen any alarming symptom follow the use of these large doses of digitalis. The only case I have lost since adopting this treatment had a tumour in the brain. In three only was other treatment adopted after digitalis had failed to procure sleep; in other words, in sixty-seven out of seventy cases digitalis was the only medicine used, and sixty-six of these patients recovered. I do not mean that these are the exact numbers of those treated; I am certain as to the death, but I may have had more recoveries. I am well within bounds in saying seventy cases in twelve years, and that all of them were well-marked cases of delirium tremens. Slight cases of nervous derangement after drinking I have seen in great numbers; but I speak here only of such cases as required active treatment. My previous experience of the results of the treatment by opium, or some of its preparations, by anti-spasmodics, &c., had certainly been much less successful; the proportion of deaths was larger, and the recovery much less rapid. Again; I have treated more than one patient successfully by digitalis, who, in subsequent attacks elsewhere, has been treated by opium and died; and in many of the cases in which I have used digitalis successfully, opium had been previously given without any good effect.

I will only allude to one case in illustration:—On September 9th, 1860, I was called to see a gentleman, 48 years of age, who was in a very alarming state, having been without sleep four days and nights, having been "muddled" for two months before, and having previously had "fits of the horrors." He had been treated by another practitioner by opium in moderate doses, but had become worse, and when I was sent for it was the opinion of Mr. Spencer Wells and Mr, McCrea—who accompanied me in my first visit—that the case was as bad a one as *they* had ever seen; certainly, *I* never saw a worse. The pulse was almost imperceptible; the skin covered with cold, clammy perspiration; the face deadly pale; the lips blue; the hands tremuously grasping the air; the eye expressive of great fear; the mind gone; he was muttering incoherently. With some difficulty I passed half-an-ounce of tincture of digitalis down his throat in the presence of my friends. In a few minutes he became more tranquil, the pulse was felt more easily, and we left him. After four hours I found that he had not slept, but he was rather more sensible, less tremulous, and warmer. I accordingly repeated the dose. Three hours after that, as he had been still without sleep, though in other respects improving, I gave two drachms more, making ten drachms in seven hours. After this he had some sleep, and had slept at intervals during the night. The next morning Dr. Ballard saw him, with my other friends, and all of them were much pleased with the great improvement manifested. He was sensible, his fears had disappeared, he was very slightly tremulous; the skin was warm, the tongue moist, and the pulse full and regular at 90. The heart's sound and impulse were normal; the bowels had acted once, and urine had been passed in natural quantity. After this he took some broth, drank freely of imperial and lemonade, but took no stimuli of any kind, or any other medicine. He slept uninterruptedly for three hours and a-half in the afternoon, and at intervals in addition. The next night was a good one; *and when he was seen by my* friends again the next morning he was almost *well, and calling out for a mutton-chop.*

*I trust that this narrative* of the results of my experience may induce oth-

ers to follow what I believe to be a very valuable practical lesson; but I must warn those who do so not to try, as I have done, any smaller doses than those I have recommended. They would not only lose valuable time by so doing, but I believe would do harm. Doses of half-a-drachm or a drachm do no good whatever; and the pulse, in some cases where I tried them, became intermitting. I have never seen this effect from the larger doses; on the contrary, a feeble intermitting pulse has generally soon become fuller and more regular, proving, I think, as I said before, and as I again wish to impress on the profession, that the curative action is on the nervous system primarily, and not on the organs of circulation.—*Med. Times and Gazette, Sept.* 29, 1860, *p.* 301.

[Dr. BALLARD, of Islington, corroborates Mr. Jones's opinion, and states as follows what he saw in two other cases, one in the hospital, and another in private lodgings.]

The former was a man apparently about fifty years of age, the other a young man about twenty-five or twenty-six years. Both had been drinking hard up to the period of seizure. In both, the characteristic symptoms of the disease were clearly marked, the skin cold, and the pulse very feeble; in the hospital patient scarcely perceptible. In both cases, the earliest operation of the medicine was seen in the abatement of the nervous phenomena, in greater quietness of manner, and in the disappearance of delusion. Coincident with this the warmth returned to the surface, the pulse resumed its natural fulness, and a healthy perspiration appeared upon the skin. After this improvement had proceeded to a certain extent, both patients slept, but I could not regard the sleep as anything but an index of the favourable progress of the case. The improvement in the general condition clearly did not depend upon its production; first, because marked amendment had preceded it, and, next, because it was not prolonged beyond a very few hours. In no one of the three cases was there any unnatural increase in the quantity of urine passed. My surprise at witnessing the success of this remarkable treatment was only equalled by my admiration of the boldness and courage of the man who could introduce so startling an innovation of our practice. It certainly is not what any one would have expected: that a feebly-acting heart would be roused into normal action by a medicine like digitalis; yet this is one of its earliest effects in the dose given by Mr. Jones.

From time to time very daring doses of digitalis have been administered. Dr. Pereira, in his "Elements of Materia Medica," mentioned the practice of Mr. King, of Saxmundham, who was in the habit of giving, in acute inflammation, doses of the tincture amounting to half-an-ounce or an ounce, without the production of any dangerous symptoms. A pupil of Mr. King once informed me, however, that during this treatment the maintenance of the recumbent posture was rigidly enforced. In neither of the cases which I saw treated by Mr. Jones did any pains appear taken to secure this precaution. In truth, the action of the drug upon the circulation, when it is used in inflammatory disease and in delirium tremens, seems utterly different. The effect in the former case, according to Mr. King, is to subdue the pulse and to render it irregular, an operation confirmed by Dr. Pereira's own experience of large doses; in the latter, the pulse increases in force and fulness.

My own impression, from what I have seen and heard from Mr. Jones, is that in tincture of digitalis we have a true *counter-poison*,—an addition to that class of mutually-antidotal poisons of which opium and belladonna have been the most recent examples. Alcohol is the remedy by which we counteract the depression produced by digitalis: the converse is also true,—digitalis is most remarkably an antidote to alcoholism.

At a meeting of the Medical Society of London, May 12, 1828, Dr, Thos. Williams related an instance of a drunken butcher having taken two ounces of the tincture of digitalis in two doses of an ounce each, in quick succession, without the slightest inconvenience ensuing. This is quoted by Dr. Pereira.

*When first I heard of Mr. Jones's* treatment, I naturally suspected the *quality of his tincture; but on* mentioning this, he assured me that he had

employed tincture from a variety of sources, and among the rest from Apothecaries' Hall, and always with the same result.—*Med. Times and Gazette, Oct.* 13, 1860, *p.* 363.

[This practice of Mr. Jones is recommended also by Dr. A WYNN WILLIAMS, whose opinion is as follows.]

That such doses are as a rule not dangerous when there is any unnatural excitement of the nervous centres, is fully borne out by the fact that even ounce-doses of tincture of digitalis have been prescribed twenty years ago to ward off epileptic attacks, when there was any preliminary warning of their approach. I was under the impression that it was Dr. Billing who so ordered it, but I cannot now find that he did so in any of the editions of his works. In such cases I have been in the habit of prescribing it in half-ounce doses for many years, and have found it by far the most efficacious remedy.

The following is a foot-note, written by myself, many years ago, under the head of Epilepsy:—"I have certainly averted the impending paroxysm by administering very large doses of digitalis, which does not appear to be so unmanageable and uncertain in its effects in large doses as in continued small ones. I have given as much as half-an-ounce of the tincture on the threatened approach of a paroxysm, and never saw any ill effects arising from administering so large a dose. One patient always kept a draught of the above dose in the house."

There is one form of excitement of the brain which I have never had an opportunity of administering it in, namely, puerperal convulsions. In this formidable disease, I decidedly think it deserves a trial.—*Med. Times and Gazette, Oct.* 6, 1860, *p.* 342.

[Dr. WEBSTER, Physician to the Northampton Infirmary, although he would, he says, be timid in adopting Mr. Jones's treatment by digitalis, gives the opinion and practice of Mr. Wood, of Heene, Worthing, who treated a case of this kind with large doses of digitalis. Dr. Webster says:]

Corroborative testimony to the innocuousness of such a dose of our usual pharmacopœial preparation of that tincture, in our climate, to the average constitution of our patients, can from one or the other no doubt be supplied, (Dr. Wynn Williams has given us one); and I would try to report a somewhat analogous case, mentioned to me by Mr. F. B. Wood, of Heene, Worthing, and until recently practising in this town, hoping that others may thereby be induced to furnish their quota of experience *pro* or *con*. Mr. Wood writes to me as follows:—

"In reference to a plan recently suggested in the *Medical Times and Gazette*, by Mr. Jones of Jersey, of treating of delirium tremens by tincture of digitalis, in compliance with your wish I send you a narrative of the case, in which (many years ago) I treated a patient labouring under furious delirium, with, as I then thought, large doses of the same drug. I should commence by stating that I had never previously attended the patient, but had learnt from reliable authority, viz., his previous medical attendant, that the patient, in the six or seven preceding years had three or four similar violent attacks. The treatment then adopted had been—venesection pleno rivo from both arms at once, and frequently repeated; hundreds of leeches to the forehead, by twenties, and twenty-fours at a time; large quantites of calomel, and long continued drastic purgation. There were also several, and severe relapses on each of these occasions; and in one instance, after an interval of three weeks from the first seizure. On the last two occasions, when convalescent, the patient was in such a low and melancholy state, that daily and nightly watching, for three or four weeks, had to be maintained, for fear of the commission of suicide. I was summoned to attend at the last attack, in October, 1844 or 1845, and the residence being several miles away from Northampton—and having been forewarned of the probable nature of the case—I went armed with two ounces af the tincture of digitalis. Upon my arrival I at once gave a tea-spoonful of the tincture, and repeated it every hour for four doses (the second dose having been given in rather less time). The patient then becoming much quieter, and circulation

steadier, I reduced the dose to half-a-drachm every two, three, and at last four hours, during the remainder of the first twenty-four hours from the seizure. The patient had only two or three slight relapses, and these within the first four days. On the third day, when there was a slight relapse, the physician who had previously attended was summoned in consultation. He applied twelve leeches to the temples and forehead, continued the digatalis, and ordered a fair quantity of calomel and colocynth. This was all the blood withdrawn on this occasion, and I confess even that against my judgment. However, when convalescence was established, there were none of the old symptoms of debility, melancholy, &c., as in previous attacks. Altogether the patient took about fourteen drachms of the tincture of digitalis, in the first four hundred hours of his illness."

As this patient has been under my supervision from time to time since Mr. Wood left this town, I can corroborate the fact of there having been no return of the complaint, even though there has too often been a sufficiently adequate amount of the accustomed provocation.—*Med. Times and Gazette. Oct.* 13, 1860, *p.* 364.

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18.—*On the Treatment of Alcoholism.* By Dr. SMIRNOFF.—Dr. Smirnoff states that he has become convinced by repeated trials that the *asarum Europæum* well deserves the reputation it has obtained in Russia of being an excellent remedy for the effects of drinking. The influence of a continuous abuse of alcoholic drinks is first exerted locally, but afterwards dyspepsia is produced; and the nutrition and functions of the entire economy, especially of the central portions of the nervous system, becoming interfered with, the blood itself being loaded with an injurious foreign material, the *dyscrasia potatorum* is at last completely established. The asarum fulfils various indications, acting beneficially on the alimentary canal in those cases in which the digestive powers are so much at fault. Its aromatic principle confers upon it a stomachic power, and regulates the condition of the intestinal discharges, producing vomiting and purging when given in large doses. Its most beneficial action, however, is manifested on the defective appetite, and by its counteracting the invincible longing for alcohol. The horrible sensations with which the drinker awakes in the morning, and which impels him to seek temporary and delusive relief from renewed libations, are much blunted and mitigated by means of a glass of strong infusion of asarum and some other nervine—*e.g.*, valerian. Its immediate effect is often to produce vomiting, and sometimes purging; but the painful sensations at the epigastrium undergo relief, and the appetite becomes invigorated. Persons who have been long habituated to alcoholic drinks cannot, however, have these suddenly suppressed with impunity; and in such cases the author gives the asarum in brandy, applying at the same time a blister or an issue to the pit of the stomach. By this means the normal activity of the stomach becomes excited and the longing for alcohol diminished. The author, however, cannot agree with those who would still allow a small quantity of spirits to habitual drinkers, even when the morbid desire for it has become appeased. The continuous use of a decoction of asarum, even when it does not succeed in extinguishing the desire for alcohol, always supports the powers of the patient; and it is remarkable in some cases, in which the individuals have been long accustomed to periodical intervals of drunkenness, ending in delirium tremens, how much longer these intervals will become, and how much less likely delirium tremens is to recur. The patients themselves are sometimes surprised at the comparative impunity with which they can continue their drinking. The author prescribes three or four glasses a-day of an infusion made with ℥ iij. of asarum root, ℥ j. of valerian root, and ℥ ½ of orange-peel, but he does not state the quantity of water employed. In cases of drunkenness another formula is composed of decoction of asarum (made by boiling from ℥ ½ to ℥ j. of the root) ℥ vj. tinct. of valerian ℥ ij. to ℥ iij., *Sydenham's laudanum gtt.* xij., syrup of orange-peel ℥ ½. A table *spoonful of this is taken every* two hours. He finds from two to five grains

of bismuth taken four times a-day a valuable adjunct. He has also found the following popular Russian remedy of service in cases of drunkenness:—

℞. Ammon carb. ℥ ½; aceti vini, lbj.; oxymel. scill., ℥ ½. Two tablespoonfuls every two hours.—*Med. Times and Gazette, July* 28, 1860, *p.* 91.

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## 19.—ON THE THEORY OF SIMPLE EPILEPSY.

By Dr. Charles Bland Radcliffe, Physician to the Westminster Hospital.

In the epileptic fit itself, the facts, when fairly read, admit of one conclusion, and one only. At the instant of the fall, a corpse-like pallor overspreads the countenance and the pulse dies out at the wrist—phenomena which seem to be only intelligible on the supposition that the arteries are nearly empty of red blood. A moment or two later, and the black and bloated face, the choking sounds, and the absolute suspension of all respiratory movements, show very plainly that the formation of red blood is arrested for the time. During the whole course of the convulsion, indeed, the state is one of suffocation. During the convulsion, that is to say, the supply of arterial blood is cut off at the fountain-head.

There is, however, one fact which, at first glance, might seem to show that there is an increased injection of arterial blood during the convulsion. Such injection is manifestly very imperfect at the onset of the fit, for upon no other supposition can we explain the corpse-like paleness of the countenance, and the feeble and imperceptible pulse. But if the finger be kept upon the wrist during the convulsion, it will be found that the pulse will go on rising until it has acquired a force and fulness which it never had in the interparoxysmal period; and if the hand be placed on the breast, it will be found also that this rising of the pulse is accompanied by increased action of the heart. These facts are evident and unmistakable, but they do not show, as without reflection they might seem to do, and as they are often supposed to do, that *red blood* is being injected in greater quantity into the arteries during the convulsion.

When the process of respiration is arrested, the right side of the heart and the venous system generally are soon gorged and distended with black blood. Under these circumstances, indeed, the gorged and distended state of the right side of the heart may reach a point in which the folds of the tricuspid valve are forced widely apart, and an opening left through which the beatings of the ventricle are made to tell almost as much in driving the blood back through the auricle into the veins, as in sending it onward through the pulmonary artery into the lungs. But it is not right to suppose that the left side of the heart and the arterial trunks are empty of blood; and this may readily verified by watching the changes which take place in the carotid and jugulars of a rabbit during the process of suffocation. On exposing the vessels, the artery is seen to be filled with red and the veins with black blood. On suffocating the animal by tying a ligature around its windpipe, the color of the blood in the artery darkens rapidly, and in about two minutes and a half it is every whit as black as that of the blood moving in the neighboring vein. Nor is the vein gorged and distended and the artery comparatively empty. On the contrary, the artery is felt to pulsate as strongly under the rush of black blood as it did previously under the rush of red blood. Nay, *the pulse of the black blood is actually stronger than the pulse of the red blood*; for, on testing with the hæmadynometer, the late Professor John Reid (who first directed attention to these facts, and who has investigated the condition of the circulation in asphyxia more carefully than any other observer) found the mercury highest at the moment when the blood in the artery had become thoroughly venous and black.

A full pulse and a throbbing heart, therefore, must be looked upon as *natural accompaniments of asphyxia*; and thus the full pulse and the throbbing *heart of the epileptic paroxysm*, instead of showing that a large quantity of

*red blood* is being injected into the arteries at the time, may show that these vessels are then labouring under a load of *black blood*, as they do in asphyxia. And that the full pulse and the throbbing heart of the epileptic paroxysm must have this latter significance is evident, for the livid, black, and bloated head and neck, and the complete suspension of all respiratory changes, show very clearly that black and not red blood is coursing through the vessels at this time.

When the convulsion is over there is little to notice in the state of the circulation and respiration. When the spasms cease, the respiration is speedily re-established, and the re-admission of arterial blood into the system may be attended with some transient and inconsiderable febrile reaction; but this reaction has nothing to do with the convulsion, for when reaction is present, convulsion is absent; and if convulsion returns, it is not till every trace of reaction has first taken its departure.

Regarding, therefore, these facts—the corpse-like paleness and the comparative pulselessness at the onset of the paroxysm, and the signs of positive and unequivocal suffocation by which this stage of paleness and pulselessness is succeeded—and remembering the previous arguments, which show that the convulsion is not to be ascribed to the presence of the stimulus of venous blood, there appears to be only one conclusion, and this is, that the convulsion of epilepsy is connected with the want of a due supply of arterial blood in the vessels.

Nor is it an objection to this view that the convulsions cease when the blood has become thoroughly deprived of its arterial properties. In order to discharge their office of conductors, it is certain that the nerves must be supplied with a sufficient quantity of arterial blood. If, for example, the principal vessel of a limb be tied, the nerves of that limb, wanting their due supply of blood, are unable to carry messages to the mind, or to transmit mandates from the mind to the muscles, until the collateral circulation is sufficiently established; and hence it is a fair inference that there must be a point in the process of suffocation where, wanting a due supply of arterial blood, the nerves must cease to be conductors, and where, consequently, the convulsions will come to an end; for, upon any hypothesis, the convulsions will come to an end when the nervous centres cease to be in proper connexion with the muscles.

But, it may be asked, is there no change in the blood itself? Is there not some important truth in the "humoral theory of epilepsy," as recently advanced in this place by the late lamented Dr. Todd? "I hold," said this distinguished physician, "that the peculiar features of an epileptic seizure are due to the gradual accumulation of a morbid material in the blood, until it reaches such an amount that it operates upon the brain in, as it were, an explosive manner; in other words, the influence of this morbid matter, when in sufficient quantity, excites a highly polarised state of the brain, or of certain parts of it, and these discharge their nervous power upon certain other parts of the cerebro-spinal centre, in such a way as to give rise to the phenomena of a fit. A very analogous effect is that which results from the administration of strychnia, which is best seen in a cold-blooded animal, like the frog. You may administer the drug in very minute quantities for some time without producing any sensible effect; but when the poison has accumulated in the system up to a certain point, then the smallest increase of dose will immediately give rise to the peculiar convulsive phenomena. This is the humoral theory of epilepsy. It assumes that the essential derangement of health consists in the generation of a morbid matter, which infects the blood; and it supposes that this morbid matter has a special affinity for the brain, or for certain parts of it, as the strychnia, in the case just cited, exercises a special affinity for the spinal cord. The source of this morbid matter is probably in the nervous system,—it may be in the brain itself. It may owe its origin to a disturbed nutrition—an imperfect secondary assimilation of that organ—and in its turn will create additional disturbance in the *functions and nutrition of the* brain." And again: "According to the *humoral theory, the variety in* the nature and severity of the fits depends on

the quantity of the poisonous or morbid material, and on the part of the brain which it chiefly or primarily affects. If it affect primarily the hemispheres, and spend itself, as it were, on them alone, you have only the epileptic vertigo. If it affect primarily the region of the quadrigeminal bodies, or if the affection of the hemispheres extend to that region, then you have the epileptic fit fully developed."

This theory is based upon the well-known connexion between the presence of urea in the blood, or carbonate of ammonia resulting from the decomposition of urea—the result of defective renal action—and one form of epileptiform convulsion; and it might also have been based upon the connexion between convulsion and blood overloaded with bile. But if there is any evidence in these facts in favour of the existence of this hypothetical morbid material, there is none in favour of the idea that the *modus operandi* of the material is in exciting a highly polarized state of the brain, if by this state is meant anything like a condition of excitement. On the contrary, it is certain (as will be shown in the next lecture) that the action of the brain and of the nervous system generally is reduced to the very lowest ebb at the time when convulsion is brought about by the accumulation of urea and bile in the blood; and it is not less certain that strychnia, instead of acting as Dr. Todd supposes it to act—that is, by exciting a highly polarized state of certain parts of the nervous centres, acts by reducing the stimulating powers of the blood, and by diminishing the electrical action of both nerve and muscle.

There is little doubt, however, that retained excretions must play an important part in the production of epilepsy. A free discharge in the office of excretion, not only in the kidneys and liver, but in every excretory organ, is essential to the preservation of healthy blood; and it may well be believed that an imperfect discharge of the office of excretion, *in one or other of the excretory organs*, may lead to the accumulation of effete matter in the blood, and that this accumulation of effete matter may be a not unimportant cause in bringing about an attack of epilepsy. But there is no reason for supposing that the blood under these circumstances becomes more stimulating. On the contrary, the conclusion which arises out of the history of the cases where the urine or bile is suppressed, is the natural conclusion, and this is, that blood thus altered is less fit to discharge its several offices; in other words, less stimulating.

Nor does there appear to be any reason for supposing that venous congestion has a more important part to play in the production of epilepsy than that which has been assigned to arterial injection. No doubt the veins of the head and brain generally are congested from a very early moment, but there is a moment antecedent to this in which the death-like pallor of the face is a sufficient proof that the veins were emptier than usual before they became congested. At any rate, the acknowledged anatomical difficulty must be overcome before it can be supposed that Dr. Marshall Hall's hypothesis of *trachelismus*—or the prevention of the return of blood from the brain by the spasm of certain muscles in the neck—has anything to do with the causation of epilepsy.

It would seem, then, as if there was something utterly uncongenial between epilepsy and arterial excitement. It would seem, indeed, as if the spasms, as well as the loss of consciousness and sensibility, were connected with want of arterial blood—empty vessels in the first instance, vessels filled with black blood afterwards. It is not improbable, also, that the blood may have been previously rendered less stimulating by the retention of something which ought to have been eliminated by one or other of the organs of excretion. In a word, the phenomena are entirely in harmony with the previous considerations respecting muscular motion; for according to them, the action of arterial blood is to antagonize contraction, and not cause it.

*Interrogating* the nervous system, the facts are found to have that theo*retical significance which* the state of the circulation and respiration would *lead us to expect.*

These facts will scarcely warrant the idea that epilepsy is connected with anything approaching to over-action of the *brain proper*. On the contrary, everything seems to point to a state which is the very opposite of over-activity. Thus, the comparative want of memory, intelligence, fancy, and purpose, which marks the interparoxysmal condition; the utter annihilation of everything mental in the fit itself; and the gloom and prostration following the fit, are facts which can have no double meaning.

Nor is a contrary opinion to be drawn from the morbid appearances which are disclosed after death. If these chance to indicate previous inflammation, it does not follow that convulsion had any direct connexion with the inflammation as inflammation; on the contrary, the convulsion may have happened before or after the inflammation, when the energies of the brain were prostrate or exhausted—an alternative which we shall see to be the correct one when we come to speak of epileptiform disease connected with special disease of the brain. And surely it is not possible to draw any but one conclusion from the appearances which are common to epilepsy and dementia—pallor of the grey substance, atrophy, chronic softening and induration, dropsical effusion and the rest?

But what of the state of the *medulla oblongata?* for, as Professor Schroeder Van der Kolk has well shown, the seat of the characteristic spasms, the bilateral character of the spasms, and the appearances presented after death, all point to this organ as one which is specially concerned in bringing about the epilectic paroxysm.

The spasms of epilepsy begin in muscles which receive nerves from the medulla oblongata—in muscles, that is to say, which are supplied by the facial, the accessory, the hypoglossal, and the portio minor trigemini; in slighter cases they are limited to these muscles. The spasms of the walls of the chest and abdomen, which are the most prominent and marked features in the complete attack of epilepsy, and which may be so fierce and unyielding as to cause fatal suffocation, also point to the same nervous centre; for a similar state of things is brought about by the action of a strong stimulus upon the great afferent nerve of this centre—the pneumogastric.

The bilateral character of the spasms is another argument that the medulla oblongata is especially affected in epilepsy. The lateral halves of this organ are connected in the most intimate manner by transverse fibres and commissures—much more intimately than the lateral halves of the brain and spinal cord; and hence it is that the corresponding nerves belonging to the two sides of the medulla oblongata are under a stronger physical necessity to act together than that which rules the corresponding nerves belonging to the two sides of the brain and spinal cord. In the case of the two latter centres, the nerves belonging to one side may be paralysed or otherwise affected, without any obvious injury to the nerves of the other side; but not so in the case of the latter centre. Indeed, it is evident that the actions which emanate from the latter centre—the play of the features, the motion of the tongue, the vocal adjustments of the larynx, the respiratory movements, &c.—must at once come to an end unless there be the strictest sympathy and concert in the action of the corresponding nerves of the two sides. Now, in epilepsy the spasms are always more or less *bilateral*, and for this reason, therefore it may be supposed that they have some special connexion with a nervous centre of which one lateral half cannot act without the other.

The appearances after death point also to the medulla oblongata as especially concerned in the production of epilepsy. In an early stage of the disorder, we may fail to find any characteristic changes; but, in confirmed cases, the texture is harder than natural, from the interstitial deposit of a minutely-granular albuminous matter, or else softened, swollen, and exhibiting signs of evident fatty degeneration. The posterior half of the medulla oblongata is redder and more hyperæmic than it ought to be; and, on examining the bloodvessels *in this* congested portion, they are found to be *of thrice their natural dimensions*, and with their walls much thickened and *altered—this dilatation* and alteration being chiefly in the corpus olivare

and in the course of the hypoglossus in the case of epileptics who bite their tongue, and in the course of the roots of the vagus in the case of epileptics who do not bite their tongue.

It is evident, then, that the medulla oblongata is especially affected in epilepsy; but it does not follow, as Professor Van der Kolk supposes, that the essential cause of the convulsive affection is to be found in an exalted sensibility and activity of the ganglionic cells of this centre.

In favour of this view,—that epilepsy is dependent upon exalted sensibility and activity of the ganglionic cells,—appeal has been made to the fact of spasm, to the presence of a full, bounding pulse, and to the freedom from attack which is for some time the fruit of an attack, particularly if this has been violent; but the answer is not necessarily that which Professor Van der Kolk supposes it to be. After what has been said about muscular motion in the first lecture, it is not possible to allow that spasm in itself is an argument in favour of exalted sensibility and activity in ganglionic cells. After what has just been said about the phenomena of the circulation in epilepsy, it is impossible to allow that the condition of the circulation favours spasm by bringing about a more active state of the medulla oblongata, (the functional activity of this, as of every other organ, being in direct proportion to the activity of the circulation of *red* blood in the organ;) for it has been seen that the bounding pulse, to which reference is made, is filled with *black* blood, and not with red. Nor can the freedom from attack, which is for sometime the fruit of an attack, be appealed to as a certain proof that the attack is the sign of the discharge of some overcharge of excitability previously present. On the contrary, it may be argued with some degree of plausibility, from certain facts which have to be mentioned in the next lecture, that the attack was preceded by depression of the circulation and innervation, that the convulsion supervened when this depression had reached a certain point, and that the recurrence of the attack was prevented for a time by the state of reaction in the circulation and innervation, which is a consequence of the convulsion. The case may be one, indeed, of which the history of the rigors of ague may serve as no inapt illustration; for here we have, first, the circulation failing more and more until the bathos of the cold stage is reached; and, secondly, a state of reaction which banishes the rigors most effectually so long as it continues.

It would even seem as if appeal might be made to the appearances after death, and to the actual condition of the circulation in the fit, for positive arguments against the idea of anything approaching to exalted action of the medulla oblongata.

The signs of fatty degeneration can have but one significance—under-action, not over-action. The interstitial deposit, also, implies an equivalent absence of healthy nerve-structure, and so does the dilated condition of the bloodvessels; and this absence of nerve-structure must necessitate a corresponding absence of nervous action. The appearances after death, indeed, if they show anything, show that the medulla oblongata of the epileptic is *damaged in structure*, and because damaged in structure, *weaker in action*, than it ought to be.

The great argument against the idea of anything like over-action of the medulla oblongata in epilepsy, however, is to be found in the state of the circulation; for if, as may safely be assumed, the activity of any organ is in direct relation to the activity of the circulation of *red blood* in that organ, how far from anything like over-action must be the state of things in which, as is the case in the epileptic paroxysm, the vessels are at first comparatively empty of red, and afterwards completely filled with black blood?

Nor can the curious discovery of Dr. Brown-Séquard, that certain injuries of the spinal cord are followed by an epileptiform affection, be construed into an argument that there is anything like a state of exalted action of the *spinal cord* in epilepsy. This curious result, which is brought about by *puncturing* or dividing more or less completely almost any part of the spinal *cord, is developed, not immediately*, but in the course of three or four weeks *after the injury. The attacks*, once developed, occur spontaneously at va-

rious intervals, often several times a day; they may also be brought on by pinching or otherwise irritating the portion of the skin which corresponds to the region of the whiskers in man. This excitable spot is supplied by twigs belonging to the sub-orbitary, the auriculo-temporalis, the second, and perhaps the third cervical nerves; and it is a curious fact, that the irritation which brings on a fit when applied to the skin in which these twigs terminate has no such effect when applied to the twigs themselves. Any other part of the skin may be pinched or irritated with impunity, but this one spot can scarcely be touched without at once bringing on a fit.

These facts are very curious, and, in the main, very unintelligible; but this much at least is evident, they do not countenance the idea of any over-action of the spinal cord in epilepsy. The fact that the epileptiform affection does not make its appearance until four or five weeks after the injury would appear to show very clearly that the fits have nothing to do with that local inflammation in the cord which may be supposed to have been set up in the first instance by the injury. After such a lapse of time, indeed, is it not the natural conclusion, that any over-action of the cord arising from the inflammation produced by the injury must have died out, and left the cord damaged, weakened, under-acting? Nor is a contrary conclusion to be drawn from the excitable condition of the nerves proceeding from the neighbourhood of the cheek or cheeks; for both sides of the face are thus affected, if both sides of the spinal cord have been injured. What the full significance of this curious fact may be we may have yet to learn, but at any rate there is no reason to suppose that this excitable condition of the skin implies an over-acting condition of the nerves or nervous centres concerned in the phenomenon. The excitable portion of skin is not over-sensitive, for the animal manifests no signs of uneasiness when it is handled immediately after a fit. Over-sensitiveness, moreover, would seem to have nothing to do with the matter. At any rate, pain, and not convulsion, is the consequence of handling those portions of the skin of the animal which may have been rendered highly hyperæsthetic by the injury to the cord which brought on the convulsions. It is certain, also, that a somewhat similar condition of excitability is brought on when, as in several experiments related in the first lecture, the skin is cut off from the full influence of the nervous centres; and hence the natural inference would be, that the action of the nervous centres in the epileptic guinea-pig is *minus* rather than *plus*.

As in the former instances, however, so here, we turn to the condition of the circulation and respiration in order to know what is the actual functional condition of the spinal cord in epilepsy; and so turning, we see that the action of the cord under these circumstances must be almost or altogether *nil*. For what action can there be when little or no arterial blood is injected into the vessels?

A similar argument will also dispose of the idea of over-activity of the *ganglia of the sympathetic system* as a cause of epilepsy. It is very possible that the contracted state of the arteries, which is implied by the death-like pallor of the countenance and the comparative pulselessness at the wrist, may show that the coats of the vessels are in a state of spasm; and it is also possible that the cause of this spasm may have to be sought in the sympathetic system; but it does not follow that over-action of this system is the cause. On the contrary, the experiments of Drs. Kussmaul and Tenner show most conclusively that strong epileptiform convulsion is possible when the action of the sympathetic ganglia is entirely suspended by arresting the supply of blood to these organs.

And certainly no opposite conclusion is to be drawn from the vague and undefinable sensations or movements, very varying in character, but all comprehended under the term *aura*—sensations of pain, numbness, tingling, or a feeling of cold vapour; movements of shuddering or spasm, beginning in a distant part and travelling towards the head; for the most probable interpretation *of these symptoms is* that of Dr. Watson—that they are in *some degree analogous to the numb* and tingling feelings which are the *frequent precursors of paralysis* and apoplexy, or to the globus of hysteria—

phenomena which by the most perverse process of reasoning can scarcely be supposed to indicate other than a state of defective innervation somewhere.

But, it may be asked, is there nothing else? Is there no peculiar state of the nervous system in epilepsy? Is there no *morbid irritability?* In order to answer this question, it is necessary to ask another—What is morbid irritability? It is not inflammation; it is not fever; it is some indefinable and negative state which occurs frequently in teething, in worm disease, in uterine derangement, and in many other cases—a state in which the patient is unusually depressed by depressing influences, and unusually excited by exciting influences. But what is this state? Is it anything more than mere exhaustion? In difficult teething, the strength is worn away by pain and want of sleep; in worm disease, the parasites help to starve and exhaust the system; in uterine derangement, the health is undermined, in all probability, by pain and by sanguineous or other discharges. In each case there is unequivocal exhaustion of body and mind, and the signs of morbid irritability appear to be nothing more than the signs of such exhaustion. A weak person is more affected by the several agencies which act upon the body from within and from without, and he is so because he is without some of that innate strength which belongs to the strong person; and the person who is morbidly irritable is, in reality, one who, for want of this principle of strength, responds impatiently to the several stimuli, whose office it is to elicit his vital phenomena. In a word, this undue morbid irritability may be nothing else than the natural consequence of that general want of power, the signs of which are written so legibly upon the vascular and nervous system of the epileptic. There is no necessity, then, to look upon this morbid state of irritability as an evidence of the existence of any peculiar condition in some part of the nervous system; for, thus interpreted, it only shows that the state of muscular contraction is ill antagonized by nervous influence. Thus interpreted, indeed, morbid irritability only becomes another name for inefficient innervation.

The *theory of simple epilepsy*, therefore, which may be deduced from a consideration of the facts relating to the nervous system is in harmony with that to which we have been led by a review of the state of the circulation and respiration in the epileptic; and this theory is one which tallies as completely with the view of muscular motion set forth in the first lecture, as it disagrees with that commonly received opinion according to which the muscles are supposed to contract convulsively because they are subjected to excessive stimulation.—*Lancet, May* 26, 1860, *p.* 512.

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20.—*On the Use of Faraday's Electricity in Chorea.*—In this disease, the action of this therapeutic agent differs widely, according as it is applied to the skin or transmitted through the muscles. Faradization of the muscles checks the irregular contractions, during the whole time of the passage of the current; but when this is intermitted, chorea reappears with the same violence as before the operation, and no trace whatever remains of the electric action. Muscular faradization is therefore inefficacious for the cure of chorea; but it may be advantageously resorted to to contend with asphyxia, one of the most dangerous symptoms of the disease. To effect this purpose, the current must alternately be transmitted through the inspirator and the expirator muscles. Faradization of the skin, on the contrary, is applicable to all cases of chorea, and occasions a very rapid and marked diminution of the movements, and frequently effects a prompt cure of the malady. M. Briquet applies Faraday's electricity to the skin every day or every other day, during five or six minutes, along the entire length of the affected limbs, and chiefly upon the parts most convulsed. In eight choreic girls so treated, the complete cessation of the irregular muscular action was obtained once in *eight, in a second in* twenty-one days, and in the others in twenty-four, *twenty-eight, thirty-three,* thirty-six and forty-seven days. One patient left *the hospital after a fortnight* imperfectly cured. In the majority of the cases

the usual methods of treatment had been unavailingly resorted to for a space of from six to eight and twelve weeks; the faradization of the skin may therefore be asserted evidently to hasten the cure of chorea.—*Dublin Hospital Gazette, May* 15, 1860, *p.* 158.

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21.—*Is the Brain in a state of Congestion or not during Sleep?* By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.—(The general opinion is that during sleep, which is a kind of slight coma, the brain is in a state of congestion, that the pia mater and other plexuses are more or less pressing slightly on the brain. Mr. Hilton brings forward some experiments by Mr. Dunham, one of the Demonstrators at Guy's Hospital, to prove that this opinion is at the least very doubtful.)

A dog having been chloroformed, a portion of the bone, about as large as a sixpence, was removed from the parietal region of the skull by means of the trephine; the subjacent dura mater was cut away, and the surface of the brain exposed. As long as the animal continued under the influence of chloroform, the smaller vessels of the pia mater were turgid with dark coloured blood, and the larger veins were considerably distended. No difference in colour between the arteries and veins could be recognised. The exposed portion of the brain manifested a tendency to rise into the opening through the skull. By and bye the immediate effects of the chloroform passed off, and the animal sank into a comparatively natural and healthy sleep. A very marked change in the appearance of the brain accompanied this change in the state of the animal. As sleep supervened, the vessels gradually emptied themselves; the veins ceased to attract notice by their distension; the exposed surface of the brain sank down to, or below, the level of the opening, and became pale in colour. In the course of a short time the animal was roused, and irritated. A blush seemed to start on the surface of the brain; the vessels of the pia mater became fuller and fuller, and of a bright arterial hue. The contrast between the appearance of the brain during this state of functional excitement and the previous state of quiescence was most striking. The more the animal was excited, the fuller of blood did its brain appear to become, and the higher did the exposed portion rise above the general level. When the animal was allowed to return to its state of repose, the brain again sank down, and reassumed its pale aspect. A full account of these and other confirmatory experiments and observations will be read at the British Association meeting at Oxford.—*Lancet, Sept.* 1, 1860, *p*, 206.

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## DISEASES OF THE ORGANS OF CIRCULATION.

### 22.—ON THE SIGNIFICATION OF THE HEART'S THROB.

By J. MACLISE, Esq., F.R.C.S.

In daring, now-a-days, to state my conviction that the throb of the heart, as felt in the left pectoral region of man or animals, is due, not to the systolic action or contraction of that organ, but really to its diastolic motion or expansion, it may well be said that I should be furnished with good reasons for such belief, since the enunciation of it is in opposition to all recorded physiological opinion and the consequences thereby incurred.

When we reflect, however, that the opinions of even the highest authorities on a question of this nature cannot be otherwise, under the anatomical circumstances, then more or less speculative, according as the veiled phenomena happen to be interpreted, this should give a place in the arena of debate to any one disposed to enter. The heart, as functional within the enclosing thorax, is impossible *to be* demonstrated. Its motions of systole and *diastole, which we infer, (and* reasonably) to be necessary for circulation, *are performed out of view.* And when, after experimental mutilation of

form and mechanism, the heart is exposed, we see, instead of the free expansion and contraction which we expected to witness of such an organ, but a mere alternating spasm and relaxation of its muscular fibres without producing any change in the capacity of its interior. Whether the heart be detached from its connections and removed from the thorax, or still holding its natural relative position by its vascular trunks, it manifests characters in common with all muscular substance. In neither condition can we discover the organ performing that motion, which would account for its throb—its measured, positive, dynamic impulse anteriorly against the left thoracic side; and, because there are anatomical facts to prove that the heart's systolic action cannot effect this beat, and hence leave us no choice but to regard it as the effect of diastole induced by some physical condition of the organ in the thorax of the living body, and only when so situated, I have no hesitation in laying before your readers the two following propositions:—

1. *The heart's throb cannot be caused by its systolic action.*
2. *The heart's throb can be caused by its diastolic motion.*

Amongst the anatomical evidences which appear to me to prove the first proposition directly, and the second indirectly, I need only mention a few, for these are sufficient:—The living heart *in situ* is enveloped by the lungs in all parts of its periphery with the exception of a portion of the right (anterior) side of the right ventricle, near its base, which portion, of greater or less area in different cases, is placed close behind the fourth, fifth, and sixth left costo-sternal cartilages. The apex of the heart is at all times, both during inspiration and expiration, covered by the left lung intervening between it and the thoracic wall. The apex, therefore, cannot strike the thoracic left side, as it is supposed to do by those who believe that the heart's throb is owing to systolic action; and, even if such a locomotion of the heart were otherwise possible, the closely investing bag of the fibro-serous pericardium, which binds the organ to the diaphragm, would be sufficient to prevent its occurrence. If, then, to any particular part of the heart, by reason of its being in close apposition with the thoracic parietes where the throb is most appreciable, that impulse can justly be assigned, it can only be that portion of the body of the right ventricle which is permanently uncovered by the left lung; and, in respect to this part, it remains to be considered whether the throb is caused by its diastolic or its systolic motion.

The physiological phenomena which, I think, should induce us to ascribe the heart's throb to diastole of the body of the right ventricle, and to this part only, must of course negative the assumption that it can be the effect of systole, or of any other of those many various and oftentimes futile causes which have been recorded.

The heart, as a hollow muscular organ, must perform its contractions, although these be vital, according to the physical law—that is to say, the larger space which the heart, while diastolically full, occupied in the thorax, must in respect to that organ be diminished when this is systolically expelling its contents from its cavities through the arterial system. The diminution of the heart by systolic contraction on the blood is answered by the dilatation of the artery as recipient of that fluid; and, conversely, the diastolic enlargement of the heart as recipient of blood must be answered by recession of the artery as conductive of the blood. This being the, so to speak, natural librative motion of the organ and its arterial vessels, it ought to appear, I think, at first sight self-evident that of the two states—viz., increase of volume or capacity, and decrease of the same—the former is the more likely to cause the pectoral impulse or throb, and as if signifying that the diastolic heart knocked at the breast for larger room, so as the more amply to receive and pass the rising venous tide.

When the heart has performed systole, all its diameters have become lessened towards a common centre, which is in its septum; and consequently it may be reasonably assumed that the external paries of the right ventricle *must* tend rather to separate itself from the thoracic wall than be impelled *against it, for now the blood is* in efflux from the organ. On the contrary, *when the heart is being set* in diastole, all its diameters become increased

from the same cardiac centre; and necessarily, the wall of the right ventricle must become suddenly pressed against the front of the left thoracic wall, for now the blood is in afflux to the heart: and that its entry into the organ is *sudden*, I, for my own part, find every reason to believe. Systole, or action, is followed by diastole as quick, as reaction; and these being the only two motions of which the heart is capable, and in the performance of which, alternately, without interval, through life it is engaged, the latter motion, I am persuaded, for the reasons specified, is the cause of the pectoral beat. I have found it possible to imitate it in the dead subject in this manner:—Having tied the brachio-cephalic veins as closely as possible to their origin in the superior vena cava, the free-vented nozzle of a large syringe full of size solution was inserted into the inferior vena cava close to the foramen quadratum in the diaphragm. The thorax was left, as naturally, entire; and the lungs were fully dilated through the trachea, so as to support the heart laterally and posteriorly, and keep the right ventricle in apposition with the left costo-sternal region. On placing my hand over this region, while the syringe by a quick motion of the piston-rod suddenly injected that ventricle, I felt an impulse similar to the heart's throb. Again, in order to prove that it is to the diastolic part of this ventricle which is uncovered by the lung, and not to the systolic apex of the heart, that the throb is due, the heart, circumstanced as above, was injected with wax, and this allowed time to set, when, on exposing the organ, the body of its right ventricle was found to have been flattened against the corresponding part of the thorax. This flattening I take to be indicative of the natural tendency of the diastolic motion being sternal, and therefore that it is this motion which causes the throb. With this conclusion all the other vital phenomena accord. The heart's beat and the arterial pulse are not synchronous, which necessarily they would be if the former were the effect of systole.—*Lancet, July* 28, 1860, *p.* 95.

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## 23.—ON THE PREVALENCE OF HEAT APOPLEXY AMONG SOLDIERS DURING THE HOT WEATHER CAMPAIGN IN 1858.

By Dr. Charles Alexander Gordon, C.B., Surgeon, 10th Regiment.

[Generally speaking, it is not among the soldiers who are permitted to go out freely during all seasons and in the heat of the day, that sunstroke is most liable to occur. Confinement of the men to their barracks during a great part of the day in the hot season, is a fertile source of tubercular disease, yet this practice is extremely common in India.]

It is the subject of common remark in India, that one of the metereological conditions under which "heat apoplexy" chiefly occurs, is when the breeze for a time ceases—the sky becomes obscured by a film of dark, negatively electrified clouds, and a sense of oppression hangs like a weight upon the mental as well as bodily energies. At such times, not only do men become the subjects of this disease, but the lower animals are not unfrequently attacked, and die suddenly from it.

During the operations against the rebels in the Arrah district, a very remarkable influx of cases of this disease occurred among the British portion of our force on 23rd and 24th May, 1858; and from letters subsequently received, we learned that a very remarkable number of seizures occurred on the same dates among the soldiers employed on field service throughout the Presidency.

This circumstance tends to confirm the opinion, that the proximate cause of the disease is lost balance of the cerebro-spinal and sympathetic nervous power, induced, no doubt, by particular modifications in the conditions of atmospherical influences, the precise nature of which our present means of investigation do not enable us to detect.

Recent occurrences *in India have* attracted attention to deaths from what *is called sun-stroke more than heretofore*; the experience of medical officers *in that country impressing them* with the opinion, that soldiers if not ex-

hausted by long previous service on a campaign, run comparatively little risk of attacks of this nature, however great may be their exposure, so long as they are successful in the military operations in which they are employed.

It is far otherwise, however, when the system is lowered by previous severe duty, insufficient food, by disaster, or any other of the ordinary causes that depress the vital energies; under such circumstances the men are prone to become affected by the disease when exposed to the sun, more especially if they have to march in "close order" through jungle tracts, where there is very imperfect circulation of air.

Mr. Hill publishes a tabular summary of all the recorded cases of the disease to which he has access. The table contains records of 47, and of this number the following are the alleged causes of the attack:—

| | |
|---|---|
| Exhaustion, fatigue, and exposure to the sun | 5 |
| Exposure to sun alone | 9 |
| Cause not noted | 2 |
| Heat in camp | 6 |
| Drink and exposure to sun or hot winds | 9 |
| Drink, exhaustion, and heat (whether with exposure not stated) | 12 |
| Heat, without exposure and without drink | 2 |
| Heat alone (not stated whether or not with exposure) | 2 |
| Total | 47 |

—*Indian Annals, July*, 1858.

We thus find that exhaustion or fatigue has had more or less to do with the disease in seventeen of all the cases above noted.

Next to depression from long-continued fatigue or imperfect food, the most prevalent predisposing cause of heat apoplexy is the habit of dram-drinking, in which all soldiers indulge more or less. During the hot season of 1855, it was found that men of the Bengal Artillery, stationed with the 10th Regiment, died in great numbers from sun-stroke, while our soldiers did not suffer in an unusual proportion from the disease. Inquiry elicited the fact, that the men of the Artillery, each day at noon, ran through the intense heat from their barracks to the canteen, rapidly swallowed their allowance of rum, ran back, partook of a heavy meat dinner, and threw themselves upon their cots, where they snoozed away, in half-unconsciousness, till the time arrived for them to dress for their evening exercise. The men of the 10th Regiment, at the same station, were never permitted to partake of spirits until after sunset. This plan was subsequently adopted in the Artillery, and the disease ceased.

In *Indian Annals* of July, 1858, it is stated that when the 67th Regiment was ordered from St. Nicholas Mole, St. Domingo, upon an expedition up country, the troops, previously to marching off, were supplied with a full ration of spirits. It was, as might have been foreseen, speedily consumed; and the men marching through a dry, sultry country, that furnished no water, fell down at almost every step. Nineteen actually died upon the road.

The system of allowing the soldiers to dine in the middle of the day, while there is a prevalent tendency to heat apoplexy, is objected to by many medical officers. While we were employed on field service during the hot season of 1858, the soldiers were permitted to dine, when praticable, and they wished to do so, in the evening. The experiment was upon too small a scale to justify any conclusions being drawn from it, nor did any particular result follow. If we are to judge from what occurred in the 20th Regiment at Lucknow, however, the dangers of the soldiers of taking heavy mid-day meals at such times is apparent, and seems among these men to have induced this disease.

Dr. Cheans states, that "On 10th June, (1858), the 20th Regiment *marched out of* Lucknow, and reached Allumbah about 9 a. m. From want *of tents, the men were exposed* to the sun's rays and heat of the sun for *several hours.*

"Before evening, 56 admissions had taken place in a force of 497. Three men died within four hours. Next day, 23 admissions from fever; on 12th, 5 admissions, 4 of which showed symptoms of apoplexy, 1 proving fatal about 3 a. m.; the fifth was a case of cholera.

"During the remaining three days there occurred 6 cases of heat apoplexy, 1 of which proved fatal; 2 of cholera; and 79 of fever. It was particularly observed that most of these cases of heat apoplexy occurred between the hours of 3 and 7 p. m., when the men had dined, and the sun falling, the heat had become more sensible and oppressive."

It is certainly not a little remarkable that notwithstanding the severe duty and exposure to which our men were subjected at different times between 1st April, 1857, and 31st March, 1858, not one became affected with heat apoplexy; dysentery having been the disease by which almost the entire mortality was occasioned. Vertigo and other premonitory symptoms occurred by no means unfrequently; but they did not attain that degree of intensity that would constitute pure sun-stroke,—cold affusion and brief seclusion from direct solar heat being all that was necessary for their recovery.

Other regiments, however, were less fortunate; and personal knowledge enables me to say that soldiers were most liable to the disease when, after a debauch of spirits over night, they were forced to march armed and accoutred during the heat of the succeeding day. This was only what might have been anticipated,

In an inquiry into the proximate cause of heat apoplexy, we shall find the remarks of Mr. Hill to be well worthy of our consideration.

This author alludes to one of the well-known effects of exposure to great heat, by which the oxygenating power of the atmosphere becomes impaired as a consequence of the expansion created by increased temperature. He then goes on to make some observations, the truth of which was illustrated in my experience of the disease during the mutinies.

It follows, that the oxygenating power of the atmosphere being diminished, the respiration must become accelerated; the circulation also increases in rapidity, and, as a result, the temperature of the body rises. The imperfect decarbonization of the blood which results from this condition, is in a measure illustrated in everyday life in India; giving rise, as it no doubt does, to the listlessness, lassitude, want of physical energy and mental vigour, as also to the extraordinary tendency to somnolence, which manifests itself to persons newly arrived in that country.

As the expansibility of the circumambient air increases, so likewise does its capability of receiving other gases, whether in the shape of malaria, miasma, or effluvia, emanating from the earth or disengaged from soils or other sources, often under circumstances inexplicable and conditions unknown. Moreover, this expanded condition of air favours the more ready disengagement of these noxious principles. It follows, that a great quantity of pernicious ingredients are inhaled, and are in an unusually concentrated form brought in frequent and close proximity to the blood, the absorbing powers of which are increased by the circumstances which give rise to the conditions now under notice.

The skin is an outlet for pernicious products that are taken into the system. If it perform its functions in a proper manner, they will be quickly expelled; but if this conservative function ceases by perspiration being checked, or from any other cause, the inhalation of these noxious principles continuing,—then, indeed, are induced a succession of evils constituting in their integrity an attack of fever or of heat apoplexy, according to the other causes which may be in operation at the time.

Diminished evaporation from the skin in a most hot condition of atmosphere thus explains why we then suffer most (in health), and why hot climates are most unwholesome. A moist state of atmosphere is also favourable to absorption: hence night is the most unhealthy time in malarious districts, because then *morbific emanations* are most readily taken into the system; *and hence also, no doubt,* arises the circumstance, already alluded to, *that attacks of the present disease* more frequently occur in persons during

the night-time than when they are exposed to the direct rays of the most powerful sun.

Another circumstance of no little importance may also be explained on the same supposition—namely, the greater liability of men to be attacked by the disease when they are crowded in masses than when they are in parties of inconsiderable size, or altogether isolated; as also the fact, that a body of soldiers, marching in close column along a narrow road through dense brushwood, where the atmosphere is but little agitated, are far more liable to suffer from heat apoplexy than they would be if marching along the open country, where the poisonous emanations arising from their own bodies are instantly carried away by the breeze, even if that breeze be intensely hot, as it certainly is in India during May and June. Of this we had numerous illustrations during the campaign against the Sepoys; and I may here observe that our great preventive was abundance of water, carried by natives, by camels and elephants, with which soldiers and officers had continually to drench themselves while pursuing the Sepoys through the tracts of jungle, as, for instance, in the province of Bahar, where they took shelter.

Mr. Hill accounts for heat apoplexy thus:—

1. The lungs are the organs primarily deranged; becoming surcharged with blood, rendered impure by noxious products. The good effects of cold affusion may thus be briefly stated:—As stimulating the flagging respiratory functions through excitomotory influence, moistening the surface, by which exosmosis and endosmosis are established; and, from evaporation being induced, the temperature of the body is directly reduced.

2. Nature determines to the skin; and its functions, being already weakened or deranged, become ultimately stopped. It thus becomes dry: dry and highly congested as it is, superficial oxygenation of the blood partially goes on, and hence the increase of heat.

3. The brain is not primarily affected, but becomes so from the derangement of arterialization of the blood.

4. The congestion of blood in the vessels of the membranes (and skin is apparently due to a loss of tonicity in the structures, and an inability in the heart to overcome this congestive state; and it may be noted, if this explanation be correct, that it may account for the benefit of arteriotomy, by taking the oppression directly off the heart, which venesection would not do.

Heretofore it has been found that the abstraction of blood in a case of sun-stroke was almost invariably followed by death,—the operation of leeches was too slow; in fact, medical treatment in its more severe forms was almost powerless, and hence the frightful rate of mortality already stated.

It is to be remarked, however, that the general rule was to abstract blood from a vein, usually at the ordinary place of venesection; and that with this view, the patient was made to sit up in bed, or was supported in that position.

In these cases it frequently happened that death was almost instantaneous; while, on the other hand, the fatal issue scarcely seemed to be accelerated if venesection was not employed, or, in fact, whatever was the remedy brought into use.

[Dr. Gordon then proceeds to relate two cases which were treated by freely throwing cold water over the head and upper part of the body; then a brisk purge was given of calomel and croton oil. In one case this sufficed; in the other, in which the symptoms were fully developed, the temporal artery was divided and allowed to bleed freely, and vesication on the nape of the neck was occasioned by the application of a hot spatula. Dr. Gordon sums up the paper thus:]

The following, as a rule, are the measures from which advantage is to be chiefly expected in the treatment of heat apoplexy:—1. Arteriotomy; 2. Cold affusion to the head and epigastrium; 3. Rapid counter-irritation, by the application of a hot spatula to the nape; and 4. Internal counter-irritation by the administration of croton oil.—*Edinburgh Med. Journal, May, 1860, p. 926.*

## 24.—ON THE USE OF LARCH-BARK IN HEMORRHAGES.

By Dr. J. M. O'Ferral, First Medical Adviser to St. Vincent's Hospital, Dublin.

There is no accident that causes more anxiety to the physician, as well as to the patient, or his friends, than internal hemorrhage, and none that from its suddenness and danger renders empirical treatment more justifiable. In such a case time is everything; life is ebbing away, and we cannot wait for the development of the physiological effects of medicines, the principle of whose action we fancy we understand. There is no occasion, therefore, in which we require to be provided with so many expedients in the event of failure of those we have already tried. When a new styptic is proposed, it becomes the duty of the clinical physician to avail himself of the opportunities afforded by a large hospital, and to record as early as is consistent with accuracy the results of his experience. The larch-bark has been proposed as a remedy in purpura, and analogy naturally suggested it as a means of controlling similar conditions of the mucous membranes, often of an alarming nature. The first class of cases in which I directed its employment was the intercurrent hæmoptysis of phthisis; in some of these, the usual remedies—ipecacuanha, lead, tannic or gallic acids, &c., had been employed without effect, when the larch-bark was tried. The hemorhage became diminished and soon ceased, and the remedy rapidly began to acquire an importance in our estimation. As confidence in the drug increased, it took an early place in the prescriptions for such cases, and up to the present moment has given such satisfaction, that it is the prevailing remedy in my wards for this affection.

There is another class of cases, often of an exceedingly obstinate nature, in which it has proved of signal service—cases of uterine hemorrhage. Some of those cases have depended upon the presence of fibrous tumours connected with the uterus; others upon carcinomatus degeneration of the organ. In both the use of larch-bark has been followed by a cessation of this dangerous accident. In some cases of hæmaturia from various sources —renal or vesical—the blood in the urine disappeared under its use.

In hæmatemesis I have not yet ventured to try this medicine, as I have not experienced failure from the ordinary remedies—turpentine, gallic acid, &c., since its introduction.

The form in which I am in the habit of exhibiting the larch-bark is that of the tincture, in doses of half-a-drachm or a drachm every third hour.—*Dublin Hospital Gazette, July* 2, 1860, *p.* 190.*

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# DISEASES OF THE ORGANS OF RESPIRATION.

## 25.—ON THE LARYNGOSCOPE.

(By a Berlin Correspondent of the 'Med. Times and Gazette.')

[The laryngoscope was an instrument formerly in use by the profession. It was originally invented by an English surgeon.]

In Liston's 'Practical Surgery,' page 417, we read, under the head of "Ulcerated Glottis," the following remarks:—"A view of the parts may be sometimes obtained by means of a speculum—such a glass as is used by dentists,—on a long stalk, previously dipped in hot water, introduced with its reflecting surface downwards, and carried well into the fauces." This pregnant hint of Liston's remained unnoticed till 1855, when Garcia published a most valuable series of auto-laryngoscopic investigations, instituted for the purpose of elucidating the mechanism of the human voice. In these experiments the image of the larynx was reflected from a mirror placed against the soft palate, so as to be received upon a second mirror placed in front of the observer *(auto-laryngoscopy.)* An elementary knowledge of

* *This paper may be compared with one by Dr.* Hardy, of Dublin, 'Retrospect,' vol. xxxix, p. 91.

catoptrics will suffice to explain the principles upon which Liston-Garcia's method of investigation is founded. The examination itself is conducted in the following manner:—A metallic mirror—varying in size from six to fourteen lines in diameter, in shape either square with rounded edges, as recommended by Czermak, or oval, according to Türck's proposal, or, as it has been found very convenient by Dr. Levin, of Berlin, semi-circular, with a concave inferior margin—soldered to a slightly flexible metallic handle,—shown in the drawing half the real size—is to be introduced into the well-

opened mouth, and fixed in such an angle against the uvula and soft palate as to throw incident luminous rays upon the larynx, and to reflect an image of the parts thus illuminated into the eye of the observer. To prevent the mirror from becoming dim by condensation of vapour upon its surface, it is necessary to warm it previous to introduction by dipping it into hot water or holding the unpolished surface over the flame of a small spirit-lamp. Garcia made use of the direct rays of the sun in his experiments: as this source of illumination, however, is not always available, and even, if so, attended with obvious inconveniences in practice, Czermak proposes the use of a perforated concave mirror of 7—12″ focal distance, by which the light of an ordinary lamp can be concentrated upon the larynx-speculum, the eye of the observer being applied to the perforation. As the distinctness of the image will depend upon the brilliancy of the illumination employed, it will be found advantageous to concentrate the light of the lamp upon the concave mirror, by means of a powerful bi-convex lens. Dr. Levin, of this city, has devised a highly-convenient apparatus for this purpose, consisting of a tin tube carrying a convex lens of two and a half inches focal distance, and of about the same diameter, which, by means of a simple contrivance, can be fixed horizontally over an Argand lamp after the shade has been removed.

The perforated concave reflector can either be held between the teeth of the observer, fixed on a suitable ivory handle, as recommended by Czermak, or attached to a large spectacle-frame, according to Stellwag's proposal, or it can be suspended from a support screwed to the corner of the table on which the lamp is placed. The latter contrivance will be found the most convenient for practical purposes. I think it was first introduced by Dr. Levin.*

It will be most convenient to place the lamp to the right of the patient, who is to be examined in the sitting posture, his hands resting upon his knees, his body slightly advanced, and his head slightly reclining backwards. According to Professor Traube's advice, the lamp, concave mirror, and larynx-speculum ought to be on the same level, and the angle formed by the rays incident upon, and reflected from, the concave mirror as acute as possible. On this account it will be wise to place the lamp a little behind the patient. The observer supports the head and chin of the patient with his left, and introduces the larynx-speculum with his right hand, looking through the perforation of the concave mirror, by means of which he illuminates the pharynx.

By causing the patient to sound alternately the Roman vowels, *a*, *e*, the velum and uvula will be raised so as to admit of the mirror being introduced with greater facility. In pressing the speculum against the soft palate and uvula, great care must be taken to avoid touching the posterior wall of the pharynx, the palatine arches, and the base of the tongue to prevent the supervention of vomiting and deglutition. "In this manner," as Czermak says, "it is possible to look into the very depths of the pharynx, to obtain a distinct image of the individual parts of the larynx, and, as I first demon-

* *Mr. Yearsley has requested us to state that he has used Mr. Avery's ear-lamp in this way for several years past.—Ed. of 'Med. Times.'*

strated in my own person, to see the bifurcation of the trachea, reflected through the widely-opened glottis, with the tracheal rings, shining through the thin mucous membrane." p. 9.

Of course, considerable practice and a certain amount of dexterity is required for successful handling of the laryngoscope, notwithstanding the simplicity of the principle upon which the method is founded.

The difficulties are mainly owing to the great irritability of the palate, which in some individuals is so considerable as not to tolerate the contact of a foreign body; others are unable to keep their mouths open for any length of time, or to command the position of the tongue, which ought to be well flattened and protruded. Same patients, as Professor Traube correctly remarks, suffer from a kind of "moral nausea," threatening to vomit as soon as they are told to open their mouths. This extreme irritability can be overcome by methodically accustoming the parts to the contact of foreign bodies, as it is often requisite prior to surgical operations on the palate. I remember reading that bromide of potash has the power of lowering the sensibility of the pharyngeal mucous membrane; it might deserve a trial in very refractory cases.

In general, however, the irritability of the uvula and soft palate will be found very inconsiderable, so that they can be raised and pressed against the posterior wall of the pharnyx without any inconvenience to the individual experimented upon. In Professor Traube's clinic I have seen an individual sitting for nearly ten minutes with the larynx-speculum applied to the fauces, so that fifteen medical men who were present could successively examine the reflected image of the glottis without any reflex phenomena supervening to interrupt the observations.

In this case the mouth of the patient was held open by a very convenient instrument, devised by Dr. Levin. The handle of the larynx-mirror is attached by a ball-hinge to the upper bar of the mouth-speculum, so as to admit of the larynx-mirror being easily adjusted for the purpose of demonstration.

In the fifth chapter, Czermak details his method for obtaining a view of the posterior surface of the velum, the naso-pharyngeal cavity, &c., and he represents the image obtainable by rhinoscopic investigation, the commencement of the Eustachian tubes being also rendered visible. Wilde has already investigated the latter by a similar method.

To obtain an image of these parts, a speculum must be introduced under the velum with its reflecting surface turned obliquely upwards, so as to illuminate the naso-pharyngeal cavity. A speculum is proposed for this purpose, to which a sliding wire-hook is attached, for the purpose of raising the velum.

Examinations of this kind are, of course, surrounded by numerous difficulties, and can only be expected to succeed if a combination of favourable circumstances obtains.

The auto-laryngoscopic observations instituted by Czermak for physiological purposes are mainly confirmative of the results obtained by Garcia's celebrated investigations, and his work will amply repay perusal to those who are interested in the important questions involved in the study of the mechanism of the human voice.

The pathological observations which conclude the work, twenty in number, illustrating most varied and interesting forms of laryngeal disease, as revealed by the larynx-speculum, are calculated to convince the most sceptical of the great advantages which must accrue to the practitioner from the adoption of this method of investigation.

The possibility of the eye serving as a guide for the hand in the topical treatment of affections of the larynx and deep parts of the pharynx is also proved by some of these observations. You must permit me to reserve my detailed statement for a future communication. Two of these cases—the first and third—during the course of which laryngotomy had to be performed, on account of stenosis of the larynx, are of particular interest, being the first in which by a novel adaptation of laryngoscopy the glottis was investigated

from below, This was effected by introducing a small mirror attached to a suitably bent handle, with its reflecting surface turned upwards, into a fenestrated tracheotomy tube. By illuminating this speculum with a concave reflector of the most brilliant and accurate images of the lower aspect of the glottis, &c., were obtained, and the nature of the pathological changes affecting the parts clearly ascertained. This method promises to be of great importance for the diagnosis and treatment of deep-seated affections of the larynx, particularly in cases of laryngeal tumours which cannot be attacked from above. By reversing the reflecting surface of the mirror introduced into the tracheotomy tube, the deep parts of the trachea might also be explored.

I will conclude by stating, that those who wish to occupy themselves with laryngoscopy will do well to follow Turck's advice, and commence by studying, with an excised larynx, the outlines of the image reflected from the speculum, as the altered position of the parts in the reflected image is at first very puzzling to the unpracticed observer. According to Czermak, auto-laryngoscopic exercises constitute the very best preparations for attaining efficiency in this branch of diagnosis. In plate 1, of Czermak's monograph an apparatus is figured, by which auto-laryngoscopy and demonstration can be combined.---*Med. Times and Gazette, May* 5, 1860, *p.* 455.

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## 26.---ON DEMONSTRATIONS WITH THE LARYNGOSCOPE.

By Jonathan Hutchinson, Esq., Assistant-Surgeon to the London Hospital, and Surgeon to the Metropolitan Free Hospital.

[This instrument seems to be exciting interest just now; we hope its popularity may continue, and that its operations may assist us in applying proper remedies. Professor Czermak has been showing how the interior of the glottis may be exposed to view, in different hospitals. We hope that it will not end in being *run after*, like as was the speculum uteri and potassa fusa some years ago.]

The instruments employed were a round mirror for illumination, in the centre of which is a round hole—as in that of the ophthalmoscope—and a small reflector, mounted on a handle for introduction into the pharynx. The light must be either full sunlight or that of a moderate lamp in a dark room. Nothing is easier than by aid of the mirror to illuminate the pharynx. This being done, the next step is to introduce the small reflector, previously warmed, into the throat, gently pressing it against the uvula and velum. The patient must now be told to make slowly a prolonged *ah, ah, ah.*—in other words, to expire gently, so as to elevate the epiglottis. If this is done, and if the pharyngeal reflector have been properly placed, a full view of the interior of the glottis, true and false vocal cords, &c., is at once obtained.

Dr. Czermak exhibits the employment of the instrument on himself, and has acquired by long training, great dexterity in bringing the parts into view, and apparently entire freedom from all sense of irritation while doing so. He employs a second reflector, by which he is enabled himself to see exactly what he is showing to the spectator. Having introduced the mirror, he first showed the vocal cords, epiglottis, arytenoid cartilages, &c., in a state of rest, then emitting a variety of notes, made the cords narrow the aperture, vibrate, and go through their various evolutions with most astonishing rapidity and clearness. Having shown the closure of the orifice by the lateral approximation of the cords, he lastly brought the arytenoid cartilages and the base of the epiglottis into apposition from behind forwards a feat which evidently required considerable effort to accomplish. Among those present at the London Hospital, were Mr. Luke, Dr. Peacock, Mr. Gowland, and many others, and all expressed themselves as highly satisfied with the clearness of the demonstrations. A like degree of gratification was, I understood, given to Mr. Paget, Mr. Bowman, &c., who had had private *seances*, and also at the other hospitals which the Professor visited.

Having concluded his illustrations of the state of the larynx in rest and in motion, Dr. Czermak next proceeded to show us the posterior nares and Eustachian tubes. This was accomplished by holding the soft palate downwards and forwards, while a reflector was introduced into the upper part of the pharynx. In this view the septum narium (its posterior edge), the opening into the posterior nares, the terminations of the middle and inferior turbinated bones, and the orifices of the Eustachian tubes were clearly seen.

Nothing could have been more instructive or more completely satisfactory than the Professor's demonstrations on himself, and he also succeeded very well on one of the specators present. When, however, we came to cases of laryngeal disease, or what might be termed laryngoscopy under difficulties, the results were different. I had in attendance two patients on whom tracheotomy had been performed, and in whom we were exceedingly desirous, for purposes of treatment, to ascertain the exact state of the larynx. In the first of these, a man now under Mr. Luke's care, the operation was performed about four months ago, and he is as yet quite unable to breathe without the canula. The man has been a sweep, and it was thought within possibility that some growth of epithelial cancer might exist in the larynx. Although the man was patient, and did his best to assist the examination, yet his inability to accomplish a slow expiration, defeated the Professor's attempts to obtain a view, since the epiglottis covered the interior parts. The second patient was a woman, the subject of tertiary syphilitic disease of the pharynx and larynx, for whom, about two years ago, I performed tracheotomy with much temporary benefit. A few weeks after the operation she became able to dispense with the canula, and has not since worn it. She has, however, had several attacks since, in which her laryngeal symptoms have been greatly aggravated, and she is permanently aphonic. The probability is that her rima glottis is much narrowed by cicatrices resulting from syphilitic ulceration. Her pharynx is also very much contracted by cicatrices, and this obstacle sufficed to entirely prevent the success of the Professor's endeavours to get a glimpse of her laryngeal structures.

It must be admitted that these two cases both presented unusual difficulties; nevertheless, they are those in which the aid of the instrument was most especially wanted. It is probable, however, that, after a few more trials, the patients might be so trained as to allow of a satisfactory examination. In Ophthalmoscopy the observer alone requires training; in inspecting the larynx, however, it is requisite not only that the manipulator should be dexterous, but that the patient should be able to assist him, both by tolerance and by accomplishing certain efforts which few at the first trial are able to do. By a little training the velum palati becomes much less irritable, and spasm is much less readily excited by the pressure of instruments. It is well known that training may do much in the way of preparing a patient to assist the operator in cases of staphyloraphy. Certain remedies, as, for instance, the bromide of potassium, and, possibly, the local application of chloroform vapour, may be found useful in inducing anæsthesia of the pharynx. By these it is very possible that the future application of laryngoscopy to practical medicine and surgery may be much facilitated. In chronic cases admitting of delay, and in intelligent patients, no doubt something might be done in instructing them to practice self-examination by the aid of the mirror, and thus accustom the parts to the contact of the reflector.

Professor Czermak told me that he thought he had not inspected the larynx in more than 500 individuals altogether, most of these, of course, being in those suffering from disease of the part. Out of this number, in no fewer than fifteen or sixteen he had been able to demonstrate the presence of polypoid growths upon it,—a fact of very considerable importance as regards the practical uses of the method. He stated that, with patience and time he did not believe that there would be more than ten per cent. in whom he could not succeed in getting the desired view.

The Professor has already left London on his way home, but it is in prospect, he informed me, that a pupil of his, Dr. Semeleder, should shortly

visit us with the intention of remaining some time, and opening a class for instruction in the use of the laryngoscope. In the mean time, not a few will probably be anxious to try for themselves. The needful instruments (according to Czermak's patterns), if not already in stock, will, I believe, very shortly be so, both at Mr. Weiss's and Mr. Ferguson's.—*Med. Times and Gazette, Sep.* 8, 1860, *p.* 236.

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## 27.—THE DIAGNOSIS OF PHTHISIS BY THE MICROSCOPE.

By Dr. Frederick James Brown, Rochester.

(This paper was read at the Gravesend Meeting of the British Medical Association; and the specimens exhibited were prepared by Dr. Andrew Clark, of London; they showed the presence of the elastic trabeculæ of the air-vesicles, and furnished evidence of the destruction of the lung-tissue.)

A few years ago, the pulmonary trabeculæ were discovered in the sputa in phthisis by Schrœder van der Kolk and by Dr. Andrew Clark, independently of one another. The former considers, or did consider, the examination of the sputa valuable only early in phthisis, whilst Andrew Clark holds that the examination is useful at all periods, as being the only *certain* means of detecting the disintegration of the lung and the increase of the disease. It is a fact that the disease often increases whilst the patient is getting fat, and appears sufficiently well to deceive himself, his friends, and his medical attendant.

The views of Dr. Andrew Clark, briefly expressed, are as follows:—

1. The parietes of the air-vesicles are indivisible; that is, they cannot be separated into layers. They are made up of membrane with bloodvessels, and yellow elastic tissue arranged as trabeculæ.

2. The air-vesicles are lined by scaly epithelium. The epithelium is easily demonstrated in the child; it is more difficult of demonstration in advanced life. It plays an important part in all chronic structural lesions of the lungs. It is absent in emphysema. The existence of an epithelium of the air-vesicles is denied by Bowman, Rainey, and Beale. They admit the appearance of cell-particles in the air-vesicles, but they believe them to be only the nuclei of the capillaries. They admit, also, the presence of nucleated cells that cannot be ascribed to the capillaries, but they believe them to be the product of morbid action.—Dr. Clark's reply is, that no lung is found without them.

3. Previously to the tuberculosis there is a disorder of the assimilation, which may be termed pretubercular cachexia.

4. Phthisis commences with the deposit of tubercle, and is a term common to all the stages of pulmonary tuberculisation. Phthisis is the name assigned to a certain anatomical lesion of the lung, and is to be understood in a different sense to that in which the older physicians regard it; for with them phthisis is hectic fever arising from pulmonary taberculisation. Phthisis, in the language of most practitioners, consists of hectic fever, due to softened tubercle in the process of elimination. This is symptomatic phthisis. Anatomical phthisis, so to speak, consists in the lesion of the lung, whether attended or not by hectic fever. The distinction resembles that made betwixt the pneumonia before Laennec's time and that pulmonary lesion recognised as pneumonia subsequently to the introduction of auscultation into practice.

5. Elimination of softened tubercle is the third stage of phthisis, and may go on in one part whilst deposition occurs in another; consequently, there is a mingling of the stages of phthisis—regarding this disease in an anatomical light.

6. Tubercle is a retrograde metamorphosis of the structural elements and of the fluid secretions (if there be any) of the part in which it may be found. Tubercle is not a deposit from the blood, but is a product of vitiated-nutri*tion—not a simple exudation*, but a complex result of perverted vital action. *When tubercular matter* accumulates and presses upon the bloodvessels, it

may excite irritation, in which case congestion is not unfrequently set up, which terminates in an exudation, which becomes mixed with the tubercular matter, and afterwards tuberculises. Such exudation in the first place is just like the exudation that takes place on an inflamed pleura: but the exudation being placed under unfavorable circumstances, does not become developed into a tissue, but degenerates into tubercle.

7. The air-vesicles are the seat of tubercle in the lung.

8. When the tubercular matter accumulates to such an extent as to compress the walls of the air-vesicles, the entire structure of the walls disintegrates, except the elastic trabeculæ (which are alone *characteristic* of the air-vesicles).

9. The elastic trabeculæ are seen in the sputa, lying in disorder amongst the disintegrated elements of the tissue, and are readily detected by the aid of the microscope.

10. The elastic trabeculæ are present in the sputa during the elimination of the tubercle, and constitute the diagnostic mark of the eliminative stage.

11. If elimination take place with only minute deposit in the lung, the microscope will reveal the disease sooner than the pleximeter and stethoscope; but where there is a considerable mass of tubercle in the lung, without elimination of any portion, percussion and auscultation, taken in connection with the general symptoms, will detect the disease, whilst the microscope gives negative results. Again, minute deposit in a quiescent state will be overlooked, although both the ear and the eye be employed for its detection.

12. The physician can confidently declare that phthisis exists when he finds the elastic trabeculæ in the sputa.

13. Phthisis commences with the deposit of tubercle, and tends to one of two issues:—(*a*) to recovery by absorption, by calcification, or by induration and pigmentary transformation; (*b*) or to suppuration and the establishment of hectic fever.

14. It is yet uncertain whether recovery or progressive disease be the more frequent.

15. The air-vesicles may remain filled with tubercle in a state of complete quiescence without softening or elimination, for years. This is a state of things that the microscope will not determine. It must be worked out by the study of symptoms, and of auscultatory signs.

16. The parietes of the air-vesicles remain intact in phthisis until one or other of the following events happen. (*a*) The tubercular matter accumulates to such an extent as to compress the walls of the air-vesicles with such force as to empty or obliterate the capillaries, when, nutrition being suspended, disintegration ensues, and the tubercular matter in one vesicle comes in contact with that in the adjoining vesicle, and so the two masses cohere. (*b*) The tubercular matter softens from any cause. (*c*) The tubercular matter sets up pneumonia, which breaks down the whole mass.

17. The elastic trabeculæ of the air-vesicles cannot be confounded with any other yellow elastic tissues *likely to be found in the sputa*, because of their character and arrangement.

18. Tubercle is often found in the tonsils; and the elongated cells of the tonsil-tissue, arranged as they are in an areolar form, might mislead an inexperienced observer. A little attention will obviate this apparent difficulty. The quasi-bands seen in sections of the tonsils consist of separate pieces, easily recognised as being separate, whereas the pulmonary trabeculæ are continuous and wavy. However, the distinction is made certain by chemical tests. The pulmonary trabeculæ resist the action of both acetic acid and liquor potassæ (inasmuch as they consist of yellow elastic tissue) whereas the elongated tonsil-cells resist acetic acid only, and yield to liquor potassæ, which renders them invisible.

19. The microscopic examination of the sputa enables us to detect softening tubercle in the *process of elimination*, which could not otherwise be detected; *it confirms or corrects* the diagnosis between phthisis, dilated

bronchi, chronic bronchitis, &c. ; and it determines the actual progress of the lesion of the lung.

Thus it is seen that the microscope confirms the opinion of the vulgar as respects "the spitting up of the lights," which is now an undoubted fact.

In conclusion, allow me to state my conviction that the detection of the yellow elastic tissue of the air-vesicles in the sputa is *one of the great discoveries* rendered to pathology by the microscope. The importance of being aware of the true state of matters in any given case, cannot be overrated; and I am sure that you will allow that when once we can pronounce a man to be under phthisis (using the term in the anatomical sense) we are likely to use hygienic and therapeutic means with greater precision and with greater prospect of success than if we considered the case to be one of catarrh, simple dyspepsia, or that *monstrum horrendum* "debility."—*British Medical Journal, April* 21, 1860, *p.* 302.

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## 28.—ON THE IMPORTANCE OF THE FUNCTIONS OF THE SKIN IN THE PATHOLOGY AND TREATMENT OF TUBERCULAR CONSUMPTION.

By Dr. Toulmin, St. Leonard's.

(Being an abstract of a paper read before the Harveian Society.)

Dr. Toulmin commenced by offering as the proximate cause of tubercle in all cases, "the breathing of impure air, and air in so small a quantity as to render it impure, especially during the night." He observed that wherever this was the continued state of existence, the result must be a deficiency in the red globules of the blood, and as the consequence of this, the deposition of plastic fibrin in an incomplete state of oxygenation, and, therefore, of organization, and thus incapable of being ultimately got rid of by change of matter. It consequently remained as an extraneous adventitious substance in the system, offering to the observer all the characteristics of tubercle. To explain the discrepancy which appears in the rich (who have no want of oxygen in the air they breathe) being equally subject to phthisis as the poor, he drew the attention of the Society to the importance of the respiratory functions of the skin, as proved by the almost instant death that occurs on closing the cutaneous pores by artificial means—as in varnishing and gilding the skins of rabbits and other animals; and observed, that in consequence of the coldness of our climate, and other causes, the higher classes of society were certainly not in the habit of making the washing the whole surface of the body a part of their daily toilet, and, consequently, that the exuviæ momentarily forming on the surface of the skin, the joint production of the sordes from within, combined with the *débris* of the cuticle, soon render the skin more or less impervious, although the individual might be in the habit of changing his linen daily. As an illustration of this state of skin, the author referred to acne, so frequently seen on the face, as being in reality the general state of the skin of a large proportion of society, especially in the earlier periods of life, when phthisis generally shows itself; and that the free entrance of air, as well as the exit of carbonic acid through the skin, being thus impeded, the same imperfect oxygenation of the blood ensues as is produced in the poorer classes by breathing mephitic air. He remarked, that for the removal of this state of skin the only means were to be found in instituting a full and free diaphoresis by the aid of artificial heat, the result of which, in first softening, and then expelling, large quantities of inspissated sebaceous matter, after the surface of the body had been washed clean with soap and water, was surprising. The author drew the attention of the Society to the fact, that the use of the hot-air bath, as a therapeutic agent, was no innovation on the established practice of the profession, as it was the mode of bathing practised by Hippocrates, Galen, and Celsus, and that the *universality of the practice* was shown by the fact, that the remains of such *baths had been found in* every colony of the Roman empire.

The author enunciated some novel doctrines, both as to the nature and treatment of phthisis. For instance: If tubercle be imperfectly organized fibrin, then it should be looked upon as a blood-disease; and seeing it is found in other parts besides the lungs without destroying life, its deposition in them should not be considered as disease either of the lungs or air-tubes, but as an accidental circumstance, killing mechanically by its ulceration extending to the surrounding lung tissue. Again, he called in question the propriety of sending consumptive patients abroad to a warm climate during any stage of the disease, as, although in the later stages of the complaint the air-tubes sympathized with the tubercular irritation, a warm atmosphere seemed more congenial to the patient's feelings, still in the earlier stages, when a cure was practicable, the breathing the open air of our winter (at least on the south side of the island) was most important; and he instanced as a proof that breathing cold air did not cause the complaint, the remarkable fact, that tubercular consumption is not to be met with in high northern latitudes.

The treatment of phthisis was considered under its hygienic and medical aspects. Under the former, and particularly in the earlier stages, the patient was recommended to live in a high, dry, and marine atmosphere, *on* the downs, rather than *under* them; to be as much as possible in the open air; to use all sorts of athletic exercises (avoiding such as accelerate the pulmonic circulation) suitable to the sex and strength of the patient, by which a more rapid change of matter is effected, together with absorption of already deposited tubercle, as well as the deposition of more healthy—i.e., of more highly organized matter. Medically, the treatment was comprised in a few short aphorisms, the first and most important being,—Keeping the functions of the skin in healthy action by means of the hot-air bath. 2ndly, Anointing the whole surface of the skin daily with some oleaginous matter. 3rdly. Keeping a local ulceration always patent by means of an issue or seton. 4thly. The exhibition of some one or more of a large variety of tonic and antiseptic medicines—all admirable adjuvants in improving the general health, (if selected in conformity with the function most sympathizing with, and reacting on, the disease,) but powerless in arresting the specific lesion in question, without the previous "Open sesame" of the hot-air bath, followed by the aspersion of cold or tepid water. The author expressed himself as fearful of saying all that might be advanced in favour of this treatment in phthisis, seeing that the disease was universally considered incurable.—*Lancet, June* 14, 1860, *p.* 37.

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## 29.—ON DYSPNŒA AS A MECHANICAL CAUSE OF CONGESTION OF THE LUNGS.

By Dr. Andrew H. Smith.

In the 'American Medical Times' for July 21, Dr. Andrew H. Smith has published an ingenious essay in explanation of the congestion of the lungs which results from severe or protracted dyspnœa. After referring to the theories of Haller, Goodwin, Bichat, Kay, Wintrich, and C. J. B. Williams, which chiefly attribute the impediment to the presence of venous blood in abnormal situations, he says that the degree of congestion capable of being explained by any of these theories may be estimated by examining the lungs of an animal destroyed by section of the medulla oblongata. In numerous instances in which he has performed this experiment, he has always found that, while some amount of pulmonary congestion was present, it was insignificant in comparison with that produced by occlusion of the trachea; although the chemical condition of the blood must be the same in the one case as in the other. He therefore argues that there must be another cause for the production of congestion by dyspnœa—and that this cause is to be found in the increased energy of the respiratory movements.

*Dr. Smith observes that, in the* mechanism of respiration, there are two

avenues of ingress into the lungs—one by the air-passages for the air, the other by the bloodvessels for the blood; and that, in fact, we respire blood in precisely the same way and by the same mechanism as we respire air. The amount of blood drawn into the lungs in an ordinary inspiration is small, owing to the fact that the space acquired within the chest is readily filled out by air admitted through the trachea. But, obstruct the entrance of air, and at the same time increase the force of the inspiratory effort, and the amount of blood entering the lungs may be greatly increased. The atmospheric pressure upon the vessels within the thorax is rendered less than that upon the vessels of the body generally, and a rush of blood from every direction towards the thoracic cavity is the result. Hence in every case of dyspnœa depending upon obstruction of the air-passages, there must be a congested state of the lungs at the termination of every inspiratory effort. That the congestion which has thus been produced during inspiration is not relieved during expiration, is due to the following circumstances:—1. The amount of blood to be expelled is increased; 2. The interval between the inspirations is diminished, so that less time is allowed for its expulsion; 3. The tenacity of the vessels is impaired by their over-distension, so that the expelling force is hereby diminished.

The following experiment has been performed by Dr. Smith to show the difference between the degree of congestion resulting from non-aëration of the blood, and that produced by the additional effect of increased energy of the respiratory movements. A ligature was placed around the trachea of an animal, and drawn so tightly as to prevent completely the entrance of air into the lungs. At the same instant that the ligature was tightened, the thorax was freely opened on one side, so that the air passed without obstruction in and out of the pleural cavity. By this means the influence of the respiratory movements upon the lung of that side was entirely suspended, while with respect to the other lung, it remained comparatively unimpaired. On examining the lungs after death, the one on the side which was opened was found to be but slightly congested, while the other was gorged with blood. The chemical condition of the blood must have been the same in both lungs, since both were equally excluded from the atmosphere. The effect of any change taking place in the action of the heart, or in the pulmonary vessels, must also have been the same in one lung as in the other.

Dr. Smith infers from the preceding reasonings, that in every case in which the respiratory movements are increased to any considerable extent in force or frequency (the entrance of air being obstructed), there must be a proportionate degree of congestion of the lungs as a necessary mechanical result.

The operation of this principle is well illustrated in a case of bronchitis. At the outset of the disease, the bronchial mucous membrane is congested, its vessels turgid with blood. This condition implies a thickening of the membrane and a diminution of the calibre of the tubes, and hence an obstruction to the entrance of air into the lungs. At the same time, the accompanying febrile movement quickens the circulation, and produces an increased demand upon the lungs. The respiratory movements become more frequent and forcible; and the expansion of the thorax not being responded to by a prompt and sufficient influx of air into the lungs, a compensating increase in the amount of blood flowing in is the consequence. The pulmonary vessels, already overloaded, receive a new supply; and their tonicity, already impaired by the effect of the disease, is still further diminished by this increased distension. With every increase of congestion, there follows an increase of obstruction, and therefore of dyspnœa, which, in its turn, gives rise to a further increase of congestion. And so the process continues, the congestion aggravating the dyspnœa, and the dyspnœa aggravating the congestion, until finally the tension within the vessels arrives at such a point that a serous effusion takes place into the bronchial tubes, mingling itself with the products of inflammation. By this means the tension is relieved, and the actual stasis which would otherwise result is avoided. But if, as is

often the case, the feebleness of the patient renders a sufficient expectoration impossible, or if the bronchial tubes, labouring under the disadvantage of having lost their ciliated epithelium, and of having their circular muscular fibres paralysed, cannot relieve themselves of the accumulating mass, then suffocation is inevitable, unless the process can be arrested by the intervention of art.

From the above considerations, and from the results of numerous experiments on animals and of a few upon the human subject, Dr. Smith has been led to believe that, in cases in which the cause of the obstruction cannot be directly attacked, the most direct and certain means of relieving the dyspnœa is by affording to the lungs an atmosphere which shall contain, in the volume which finds access to the air-cells, a quantity of oxygen equal to that respired in health. By this means the demand for oxygen is supplied, without the necessity for those violent muscular efforts which aggravate the cause of the dyspnœa, while at the same time they exhaust the strength of the patient.—*British Medical Journal*, *Sep.* 15, 1860, *p.* 726.

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## 30.—OBSERVATIONS ON THE TREATMENT OF ASTHMA.

By T. L. Pridham, Esq., Bideford, North Devon.

Much has been written of late on the symptoms and treatment of asthma—perhaps the most painful and apparently alarming disease to which human flesh is heir.

The observations which I have to make on the disease are based on the experience of nearly one hundred cases which have come under my medical care. I may not throw much new light on the subject; yet I am desirous to record the result of my experience and my treatment, as well as my observations on the peculiarities of this most distressing complaint, in the hope that some little may be added to the stock of information which has already been collected on the subject.

It is now many years since a deep-rooted impression was made on my mind, by that memorable man, Abernethy, "that in order to treat disease, whether of a local or constitutional character, it was absolutely necessary to keep the stomach in that state, so that the food which was taken into it should be properly digested, in order to secure good blood, and consequently healthy secretions and healthy action of the body generally; and that in proportion as the surgeon or the physician attended to these particulars, so in proportion would the means placed within the reach of the practitioner be more or less successful."

With these impressions, which I believe to be most useful to the medical man, I started in my profession. I had not been long in practice, when a severe case of what was called humid asthma presented itself for my treatment. I found that it had been of many years standing, and frequently seemed, from its violence, to threaten the life of the patient. The attack, however, would subside after a copious expectoration had been thrown off from the lungs; the man at the same time becoming almost exhausted for want of nourishment, which he was not for a time able to take, except in the smallest quantities. After the lapse of a few weeks, the man would rally, eat and drink largely, and go about his work until another paroxysm threatened, when I was again in attendance. On each occasion of an attack, I used to say, "This surely will be the last;" so severe and imminent did it appear.

This state of things went on for some time, till at last the thought occurred to me, How is it that asthmatic patients generally live to a good old age? They do not die of consumption; nor do the lungs undergo any other change except the dilatation of the air-cells, which dilatation of the cells does not much interfere with their normal action when free from an attack. Thus considering the matter, it appeared to me probable that the original cause of disease was in the overworked powers of digestion; and I was borne

out in this supposition by the circumstance that the patient had respite from attacks as soon as a certain amount of expectoration had been thrown off, and his stomach had for a season rested. Was it not then probable that impurities in the blood were formed by imperfect digestion, which were thrown off by means of the lungs? How far my ideas were correct, the following statement of treatment will, I think, go far to prove.

I speak now of what is called *dyspeptic asthma*—a disease which I believe peculiarly hereditary; that is to say, when the disease has once made its appearance, those who inherit it will ever be liable to attacks from imprudent diet; for I am convinced that an asthmatic person can never with impunity eat and drink as other people do. I may here state that, out of the number of cases of which I have attended, I found hereditary predisposition in nine out of ten. And if it be true that asthma is an hereditary disease, I will endeavour, by the evidence of cases which I intend to publish, to show that wherever it has existed in any member of a family for one, two, or even three generations back, exciting causes, such as imprudent diet, an attack of bronchitis, an attack of influenza, derangement of the liver, or any other functional derangement, atmospheric influences, peculiar odors, influences of the mind either for joy or sorrow, may at any time or period of life bring to light this peculiar disease, which has hitherto been latent in the system. It is fortunate that stomach-derangement from imprudent diet, &c., is, as far as my experience goes, the most frequent exciting cause—consequently more under the control of medical treatment, provided the patient has resolution enough to second the efforts of the medical attendant in carrying out a strict system of diet and regularity; in fact, it may be said that the scales are thus held with disease and suffering on one side, and comparative health and comfort on the other.

Most of the other patients inherited gout, and in some instances both diseases were traced in the family; and in more than one instance where this was apparent, I found that there were alternate attacks of asthma and gout, which is, I think, an additional evidence that both diseases are hereditary, and may be considered blood-diseases. What the peculiar element in the blood may be which produces either asthma or gout, it is to be hoped scientific researches may one day decide.

To return to my first case. The man in question, possessing some good sense, although he gave himself to beer-drinking and gross eating, as he said, to get up his strength after an attack, after having heard my views of his case, replied, "Well, sir, your remarks appear reasonable enough, and I will endeavour to follow your advice, and not eat and drink so much." I wrote down a system of dietary and general management, which for a season he followed with advantage; after this, he became tired of the plan, and again fell into intemperate habits, with an increase, if possible, of all his original fearful sufferings. He at last became reduced in circumstances, and was obliged to apply for parochial aid. I thought I had now a good opportunity of trying the effects of diet with medical treatment on this patient, and I persuaded him to go into the union house. There I took good care that he should not deviate from my prescribed treatment; for I placed him under *lock and key*, ordering his food to be regularly given him, whilst he had a trustworthy attendant when he left his room for the purpose of air or exercise.

His appearance and symptoms at this time were as follows:—His countenance bore the signs of great distress. His shoulders were elevated; the eyes protruded; he had no appetite; the stomach was greatly distended after eating; pulse 70. feeble, yet regular; the tongue was coated, with fissures in it. There was great emaciation, and inability to lie down in bed or to walk up an ascent. The secretions from the bowels were dark. The urine was loaded, showing an acid deposit.

After giving the patient an alterative pill and a saline aperient, I ordered him the following diet, which was to be regularly weighed out to him, and the hours of meals most strictly attended to. Breakfast at eight o'clock—half a pint of green tea or coffee, with a little cream, two ounces of dry

stale bread; dinner at one o'clock—two ounces of fresh beef or mutton without fat or skin, two ounces of stale dry bread, or well-boiled rice; three hours after dinner, half a pint of weak brandy and water, or toast-water *ad libitum*; supper at seven o'clock—two ounces of meat, with two ounces of dry bread. He was not allowed to drink within one hour of his dinner or supper, or till three hours after; at other times, he was not limited. Open air exercise was ordered to be taken as soon as the office of digestion had been performed, but short of fatigue. In addition to this, I ordered him three grains of the extract of conium four times a day, at the hours of seven, twelve, five, and ten; the dose to be gradually increased to five grains four times a day. Under this treatment, in a few days the whole of his distressing symptoms subsided. At the end of a months he was allowed three ounces of meat twice a day. At the end of two months, he had gained flesh and strength, so that he was able to follow his occupation as a carpenter, which he continued to do without intermission for some years, adhering strictly to the rules laid down for him, which he had now the good sense to adopt.

My views with regard to treatment in the case were simply these: not to give the stomach more to do than it could well accomplish; not to dilute the gastric juices by fluids before or soon after eating, but allow them to perform alone their proper functions; and to quiet the nerves of the stomach by means of a mild sedative, which prepared it for the due performance of the office of digestion. By this plan the stomach soon began to crave for food, and a longing for the hour for taking nourishment appeared—good evidence that the powers of digestion were recovering.

Is it not reasonable, then, to suppose that a moderate quantity of food, well digested, supplies the system with more pure and nutritious blood than a large quantity of ill digested food can possibly do? It appears to me not unreasonable that the poison, as it may be called, which is generated by food ill assimilated, finds its way into the circulation; and hence arises the misery of hereditary asthma, which nothing but a paroxysm, and generally a copious expectoration will relieve.

I now proceed to the further evidence of cases which have been treated by me under the sedative and strict dietary system; and in doing so, I think I cannot give stronger proof of its efficacy than by quoting communications from patients themselves, as evidence of the relief they have obtained, and the life of comparative ease and comfort to them after adopting the system. But, before I go further, I would remark on the absolute necessity of procuring the best prepared extracts, which form of medicine I have invariably used for the purpose required. I have more than once been disappointed in my treatment, when my prescriptions have been sent to ordinary druggists, with whom sufficient care has not been taken in this matter; therefore I strongly advise medical men to procure their extracts from, or send their prescriptions to, some known first-rate practical chemist, whose reputation greatly depends on the purity of his chemical preparations. Genuine extracts retain for a long time their distinctive odor, remain moist, and do not mildew. The sedative extracts which I have generally used are those of henbane, conium, belladonna, and stramonium, gradually increasing their doses and frequently substituting one extract for another, as symptoms appeared to demand.

It is not my intention to enter into the more scientific department as to the supposed causes and effects of this mysterious and capricious disease, so ably treated of in the pages of this journal by Dr. Hyde Salter, whose remarks I have read with much interest; my object is simply to record my remarks on the history of cases as reported by my patients, together with observations made by myself during my professional attendance, which may, I trust, lead to the mitigation of sufferings. My own opinion is, that it is impossible, from the present knowledge we possess, to come to the *real causes* of the disease in asthma; but still I trust practical observations may be valuable, inasmuch as they may lead to scientific researches, not only as regards the state of the blood in asthmatic patients, but also as regards the

peculiar expectoration which is thrown off, through the medium of the lungs, in the different forms of the disease, which appears to me to be nature's effort for relief; for it is my strong impression, as I have before stated, that it is in the blood that the secret lies, as, the more healthy is the state of the powers of digestion, so in proportion is the freedom of the patient from disease in dyspeptic asthma. Then, again, as regards persons being affected in certain localities, whilst perfectly free in others, may not the peculiar state of the atmosphere add impurities to the blood in some districts, whilst in others a more congenial atmosphere will lessen, and in some instances entirely remove, the impurity which exists in the circulating fluid in those predisposed to the disease by hereditary causes? I have apparently, seen several instances of this kind, which I will hereafter name.

I will now proceed to record the history of cases of dyspeptic asthma.

*Case* 2.—In the year 1855, I was requested to visit a clergyman, upwards of 70 years of age, from the north of England, who had been residing in this town for some weeks, for the benefit of his health. I had frequently remarked him in the streets; and not knowing the exact history of his case, I considered he was in the last stage of consumption, so emaciated, so feeble and distressed, did he appear. I think I never saw any one walking at large so emaciated. I learned from him, that he had been afflicted with asthma for ten years, that during that time his clerical duties had been suspended, and that, in fact, he was quite a martyr. In this time of his affliction, he had sought every possible advice, both in London and in the country, but without any material benefit or relief; the general opinion given him was, that he would never recover, and that he must bear his affliction as best he could. He was not able to lie down in bed; and, for years, every night he had anticipated death before the morning came, when, however, a copious heavy expectoration would with difficulty be thrown off from the lungs, and as the day advanced he became somewhat relieved, was then dressed, and passed the remainder of the day in great discomfort. Having inquired into his mode of living, I was told pretty nearly as follows:—At six in the morning he had a cup of strong coffee; at nine he had tea or coffee, toast, eggs, or a chop; he lunched at one, generally on bread, cheese, and porter; he had a substantial dinner, with porter and wine, and a fair quantity of port wine after dinner; he had tea or coffee in the evening, as well as supper afterwards. My remark was, "And for all this amount of nourishment, you are as thin as any human being can well be." He replied, "I have been invariably told that, without such support, I cannot live a month; and I take it to make up for the large amount I expectorate in the morning." I examined his chest and found neither tubercular deposit nor any lesion of the lungs; but evidently, from the very clear sound emitted on percussion, the lungs were emphysematous, particularly under the right clavicle, whilst lower down a dull sound was emitted. In the morning, the pulse was seventy in a minute, but as night came on it rose to ninety or a hundred. His tongue had a very morbid coat—large, with fissures in it; his stomach was generally distended, and, though he ate so much, he had never any great desire for food; his bowels were irregular; the secretions dark; the urine was loaded with a pink deposit. At the conclusion of my first visit, I surprised him by saying, "You will considerably improve in health under the treatment which I intend to adopt, and, although your age is such, I should not be surprised at your one day being in harness again." His reply was, "That is utterly impossible; my disease has taken such hold on me for so many years." He further said, "he looked forward to nothing but death, and he often prayed to be released from his sufferings." I went on to remark, that if my treatment was to benefit him, seeing it was an extreme case of dyspeptic asthma, he must make up his mind to one thing, and unless it was fully carried out, he must not expect to derive any good result; that was, he must eat just one fourth what he was daily in the habit of taking, and, in addition, his food must be weighed, and taken as regularly *as the clock struck*. His reply was, "If that be your plan, you will *soon shorten my days*." I begged him to try it for one week, which he at

length acceded to. I commenced my treatment on the 23d of April, by giving an alterative dose of blue-pill, and a saline aperient. I then ordered three grains of the extract of henbane four times a-day, and the same system of dietary as prescribed in my first case, allowing him, in the form of animal food, only two ounces of it twice a-day. The result was, that at the end of a week he was able to lie down in bed and sleep several hours; his morning expectoration had lessened considerably; his appetite improved: his tongue cleared; the distension at his stomach lessened; the urine was clear; his spirits lightened; and he could exert himself with much more ease. He was now fully satisfied that the plan had so far succeeded; but his great fear was that he should be starved, by reason of so small a quantity of food being allowed him. At the end of ten days, I substituted the extract of conium for that of henbane, ordering him four grains four times a day. From this time, he improved rapidly until he left the town to return home, on the 14th of May. On the 19th of June, he wrote me from London, where he was staying with some friends: "I cannot be sufficiently thankful for the comparative ease and comfort which, through God's blessing, and your skilful management, I have now enjoyed for the last six weeks. The weather has been most unpropitious, notwithstanding which, I have not entirely avoided the cold; my nose is rather 'stuffy' from its effects, but no symptoms of asthma have yet appeared, and I trust, by my strictly adhering to your regimen, I may be spared any return." On the 30th of June, he wrote, from his home in the north of England—"I find the air here much keener than in Biddeford or London, and it has come chiefly from the east. I cough a little in the morning, on first waking; but I have felt nothing like asthma. I have adhered very generally to your rules, with only the addition of more bread to my breakfast and tea, which you have allowed me. My secretions appear all right. I am, as you saw me, almost a skeleton; but I am in hopes, however, that the wholesome food which I get here will be beneficial." I may remark here that the months of May and June, in 1855, were unusually cold, the east winds generally prevailing. On the 12th of July, my patient wrote: "I am perfectly free from asthma; my appetite is very good, and I greatly desire that my daily allowance may be increased; and the number of my pills, I think, may be reduced." I now ordered five grains of the extract of conium three times a-day, and increased his daily allowance of food a little, keeping strictly to the hours of taking it. On the 7th of December, my patient informed me: "Notwithstanding the severity of the cold, and the dampness of the atmosphere, I am quite free from asthma. I now take my medicine only twice a-day; it appears to have a wonderful effect. I am able to lie down in bed all night; and I sleep six or seven hours at a time, and feel no difficulty in breathing, and I trust this winter I shall be able to remain at home without a return of my fearful disease." On the 17th of Dec., he wrote to say he was still going on well—"Some days since, I imprudently exposed myself too much to the cold and damp, and was consequently seized with a shivering fit, and afterwards fever. I was confined to my bed for forty-eight hours; but no asthma made its appearance. I weighed myself last week; and notwithstanding my late attack, I found I had gained seven pounds in weight since the previous weighing."

Since the date of the last letter, I have occasionally heard from my patient, who still applies to me for advice whenever his complaint, from any imprudence in diet or irregularity, threatens an attack; and, to make my prediction true, I hear he has occasionally resumed his parochial duties as rector of his parish.

*Case* 3.—The third case of dyspeptic asthma was also in a clergyman, whose appearance was very different from that of the last, he being stout and lethargic. He came to be under my care from the county of Sussex in 1856. He had been afflicted for seven years, and his difficulty of breathing was constantly on him whilst awake, particularly after a full meal. On examination, I *found his lungs greatly* oppressed, and dull on percussion; his *heart's action was very feeble and slow*; his tongue coated and brown; the

stomach greatly distended with flatus, which appeared to distress him greatly; the urine was loaded; he was unable to lie down in bed, and had heavy expectoration in the morning. He still performed his parochial duties, though with the greatest difficulty. Having cleared out the bowels by means of an aperient, I placed him on the sedative snd strict dietary system, giving three grains of the extract of conium four times a-day, with a pill every other night of two grains of each of the extracts of taraxacum and simple alœtic pill.

He came under my medical care on the 1st September. On the 8th of the same month, I had the following *bulletin* from him. "My health improves rapidly under your mode of treatment, and I was able yesterday to repeat the responses at church with ease to myself; and I took a walk without feeling the least oppressed in my breathing or fatigue to myself. I have been afflicted with asthma for more than seven years, and have consulted several eminent physicians without anything like the relief which I even now enjoy. A week only has elapsed since I consulted you; and yet, I am thankful to say, my chest is so much relieved, that I am able to walk several miles without being obliged to rest in order to fetch breath." On the 18th Sept., he wrote:—"By this time you will be anxious to know how I am going on. The weather has certainly been very fine and clear since I commenced your mode of treatment; but many a year has elapsed since I have had such freedom from pain. On Thursday last, I walked up a steep hill of half a mile without stopping once; and on the following day, I bore the fatigue of a long journey into Sussex, without finding my chest at all oppressed. Last Sunday was the grand criterion; and I can assure you my voice was remarkably clear, and my chest but scarcely influenced by the enemy." On the 2nd October, he wrote:—My health, on the whole, is very satisfactory. Since I last wrote to you, the weather has been very cold and stormy; notwithstanding which, I have breathed more freely than I have for many a year, and I now go about the active duties of my calling with confidence."

Another letter, a month afterwards, tells me he is quite well; since which time I have had no communication with my patient, but I learn from his friends that, thinking that he had remained so long free from complaint, he might with impunity return to his former way of eating and drinking whatever came in his way, he has consequently since experienced returns of his complaint, as might naturally be expected; for, as I have before stated, an asthmatic patient, who inherits asthma, can never with impunity eat and drink as other people; and although I gradually increased the amount of support taken in this case, yet the patient was not satisfied, but allowed his inclination to overrule his better judgment.

The above case must be admitted to be strong proof how much the disease is influenced by the manner in which the office of digestion is performed, and how much the patient has it in his own power to live a life of comparative ease and comfort.—*British Med. Journal, June* 9 *and July* 28, 1860, *pp.* 434, 570.

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31.—*On the Essential Nature of Asthma.* By Dr. H. Hyde Salter, F.R.S.—There are two ways (which I have not mentioned in my work), as indicated to me by my friend Dr. Brown-Séquard, in which bronchial spasm, when once established, may be kept up by the very conditions which it generates; one is, the power which carbonic acid gas possesses of producing contraction in smooth muscles. In asthma, the deficient standard at which respiration is carried on, and the diminished interchange of the gases, produces an accumulation, in the air locked up in the air passages, of carbonic acid to an unusual degree. This, by the action to which I have just referred, sets the bronchial muscles still further contracting, and thus increases the very condition which at first caused the accumulation of the effete gas. In this way, asthma keeps up asthma. The other way is by the bronchial spasm stimulating the afferent or perceptive nervous filaments, and thus giving rise to reflex muscular contraction; just as the stimulation of the

sensitive roots of the spinal nerves produces reflex muscular phenomena in the parts to which the corresponding motor nerves are distributed. In this way, muscular spasm becomes a stimulus to muscular spasm.

But in both these, as in the other ways I have indicated in my book, the bronchial spasm is secondary to an antecedent nervous condition.—*British Med. Journal, July* 28, 1860, *p*, 589.

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32.—*On the Proper use of Stramonium in Hay-Asthma.* By Dr. EDWARD LAWFORD, Leighton Buzzard.—[The thorn-apple or stramonium was first introduced to our notice from the island of Ceylon. Authors differ remarkably in their testimony as to its use—some reporting favourably, others considering it well nigh valueless.]

Smoking the herb is the plan generally recommended, and in those who are easily brought under its influence this plan is, doubtless, occasionally of considerable service; but with the majority little or no effect is produced by this mode of procedure. True, smoking, if long continued, may produce nausea and slight narcotism; but the paroxysm is urgent, and *immediate* relief is required. Under these circumstances, I have been in the habit of directing the patient to introduce portions of the stem and leaf of the plant into his pipe, as before, to smoke it, and inject the smoke thus obtained from the mouth into an inverted ale glass; to continue this procedure until the glass is filled with the stramonium fumes, and then to place the glass (still inverted) over the mouth, and take one long and deep inspiration. The effect is instantaneous, a sense of suffocation for the moment, then copious expectoration of ropy mucus, and immediate relief.

Stramonium, in addition to its antipasmodic and narcotic properties, is a *direct, powerful, and instantaneous expectorant*, if properly applied, and in this consists its peculiar value in this distressing complaint; but in order to effect this object, it must be brought into *absolute contact* with the air-cells of the lung; simple imbibition through the mucous membrane of the mouth will not bring out its powerful expectorant properties, and hence those who have looked for benefit from smoking the drug simply, have been grievously disappointed, and have pronounced it valueless.—*British Medical Journal, Aug.* 18, 1860, *p.* 657.

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## 33.—ON THE VALUE OF TONICS IN ASTHENIC DROPSY.

By Dr. R. S. SISSON, Cheltenham.

(It has been shown by Dr. Handfield Jones, that some cases of asthenic dropsy are curable by tonics alone, and are unaffected by diuretics. In the following paper Dr. Sisson shows that other cases, though benefited by drugs of the latter description, require the former to complete a cure.)

MRS.——, aged forty-five, has been ailing more or less for the last ten years, during which time she has been frequently under treatment with only temporary benefit. For the last three years she has had swelling of the face, abdomen, and legs. Catamenia have appeared only once in that time, and then in excess. Has had an eruption on the skin, unaffected by treatment. For the last two years or more she has lived on slops, forced down without appetite. Has had frequent vomitings, with diarrhœa, blood being often passed with the stools.

May 16th. Found her in bed, presenting the following symptoms:—Face much bloated; abdomen very protuberant; body generally anasarcous, with large dropsical swellings in depending parts; much emaciated in parts not concealed by effusion; patches of eczema on back of hands, feet, and front of legs; great dyspnœa; horizontal position intolerable; incessant cough, expectoration thin and frothy; complaints of great pain in the chest; on percussion, *marked dullness on the right* side, over lower half of lungs *posteriorly and anteriorly; loud crepitations*; mucous rales; pulse 100, feeble

urine turbid, scanty, of dark-brown colour, not coagulable by heat and nitric acid; stools liquid, dark, and offensive. To apply turpentine stupes to chest; chloride of mercury, three grains; powdered rhubarb, ten grains. To be followed by an aperient draught.

17th. Ordered bitartrate of potass, one scruple; vinegar of squills, spirit of nitrous ether, of each ten minims; compound decoction of scoparium, two ounces, thrice a day; beef-tea, and half an ounce of brandy every four hours.

The patient improved most satisfactorily up to the 25th, when her husband summoned me in haste, stating that his wife was dying. I found her gasping for breath; pulse at the wrist scarcely perceptible; had complained of great pain in the chest. Apply turpentine stupes to the chest; hot bottles to the feet, and give one drachm of brandy. Nutrients more frequently; continue diuretic mixture and brandy.

29th. The patient was delirious last night; got out of bed, and went into the adjoining room. This morning she is nervous and fidgety. Pulse 120, weak. Half an ounce of brandy every two hours; continue diuretic; twenty minims of tincture of opium at bedtime.

30th. Slept well last night; much improved; dropsy disappearing, except in face and legs; urine much increased, clear. She called my attention to a swelling in the right iliac region, for which she had been previously under treatment. Now that the ascites was removed, I perceived a tumour in this situation, of the size of a large fist; painful; movable; outline distinct; fluctuation perceptible. As vaginal examination was objected to, I did not press it in her present state, intending, however, to do so at a future time. To apply six leeches, and fomentation.

June 1st. Pain in iliac region removed. Apply six more leeches.

6th. At this time all the more urgent symptoms had disappeared, but the legs became enormously distended, and covered with one mass of petechiæ. The catamenia reappeared; excessive and pale. Discontinue the diuretic mixture. The legs to be bandaged. Ten minims of tincture of muriate of iron thrice a day.

From this time the patient recovered most rapidly, the bloated cheeks, however, remained unchanged, showing that the cure was not yet complete. Ordered to bandage the face tightly with handkerchiefs, and to continue the iron. In a few days my patient had the satisfaction of seeing her face reduced to its normal proportions. The eczema entirely disappeared, and by the end of June she had not only regained her strength, but stated that she never felt better in her life. I now pressed for an examination of the swelling in the side, when she assured me, that after a minute examination, not a vestige remained.

Here was a case of acute pulmonary œdema, supervening on chronic ascites and general anasarca, in a patient debilitated by long disease and inability to take substantial food. Her history was most unpromising, her brother having died of dropsy, her mother dropsical, and a sister, about whom I have since been consulted, in the same state. To a young practitioner like myself, it may be supposed that the case was one of no little anxiety. But still, after a most minute examination, being satisfied that no organic disease existed, and believing in the wonderful power of drugs when properly employed, I predicted a perfect recovery. But when the ascites had disapeared, and my attention was called to the tumour in the iliac region, my favourable prognosis was shaken. Had I continued the routine practice with diuretics, not only should I have failed in effecting a cure, but I anticipated a return of all the symptoms which had disappeared. What diuretics failed in, iron accomplished, and has, I believe, rendered permanent cure, which would otherwise have been of short duration. What would nature—what would the much-vaunted homœopathy have done in such a case? Allowed disease to have its sway, and hurry the poor patient with rapid and *unerring strides* to a premature and unnecessary death. Upon what pathological condition did the dropsical effusion in this case depend? Doubtless *the more liquid portions* of this greatly impoverished blood found a ready

exit through the walls of the capillaries, relaxed from want of their proper stimulus—a healthy blood. The action of the iron is then obvious.

And now comes the most important question. What was the tumor in the iliac region? Could it be ovarian dropsy? Such was my diagnosis. But we are told that the contents of an ovarian cyst are never absorbed. The vaginal walls, however, may give way, and the contents be discharged by that channel. Was the supposed return, then, of the catamenia on the 6th of June such a discharge? I think it possible; but as a vaginal examination was not made, a doubt may exist whether the tumor was ovarian at all. Here is a tumor dating from a miscarriage ten years ago, gradually rising out of the pelvic cavity towards the abdomen, increasing as it proceeds; at times painful; movable; with defined outline and perceptible fluctuation. I ask, if not an ovarian tumor, what was it?

The extracts I have made of this case seemed to me as few as were compatible with the right understanding of so important a subject.—*Lancet, Aug.* 11, 1860, *p.* 132.

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## 34.—ON PARALYSIS OF THE BRONCHIAL MUSCLES.

By Dr. James F. Duncan, T. C. D., Physician to the Adelaide and Simpson's Hospital.

The morbid changes occurring in the muscular tissue of the pulmonary system do not appear to me to have attracted that attention from pathologists which has been so readily extended to every other structure in the body. It is quite true, that we have for a very long period been accustomed to recognise spastic contraction of these fibres as a frequent cause of some forms of dyspnœa; and various appropriate remedies have been resorted to for the relief of those diseases, suggested by the idea of their convulsive nature. But the opposite condition of impaired contractile power, if it has not been totally overlooked, has at least been greatly disregarded. No attempt, that I am aware of, has been made to ascertain the peculiar symptoms and signs arising from such a state, or to determine the treatment suitable for its removal or relief. And yet, judging from analogy, no reason can be assigned why such a condition as that of paralysis, or impaired vital energy, should not occasionally occur here, as well as in other muscles; or why, when such a condition does occur, its existence should not be discoverable by a careful observation of its attending phenomena.

Several circumstances naturally suggest themselves as serving to account for this omission. In the first place, the small size and obscure development of these muscles give us the idea that they are to be regarded rather as a rudimentary structure, preserving the analogy of form with the same parts in other animals, than as an apparatus designed for any special use in the economy of man. Very few practical physicians, I believe, look upon these muscles as performing any efficient part in healthy respiration; and hence they are not prepared to expect any material inconvenience to be produced as the result of their impaired action. In the second place, the fact that care has been taken, by the very organization of the parts, to prevent the respiratory process being materially interfered with by morbid changes in the vital action of these fibres, renders the occurrence of these changes, when they do take place, less obvious to the general observer. Not being voluntary muscles, neither the patient nor the physician is placed in a position to discover the loss of their contractile energy, while the manner in which they are associated with the elastic structures of the lungs is calculated to counteract the injurious effects that would be sure to follow, were collapse of the air-passages a necessary result of their paralysis. In the last place, morbid changes in the muscular structure generally are not of a nature to be recognised by microscopic examination. Changes in other tissues are too obvious to be overlooked. The mucous membrane may be congested, *or thickened, or stripped* of its epithelium. The serous membrane *may lose its transparency*, or be covered with an adventitious layer

of lymph. The air-cells may be ruptured, enlarged, or otherwise altered. The parenchyma of the lungs may be hepatized, infiltrated with purulent matter, or œdematous, all of which can be easily recognised by the senses. But the muscular structure seems to defy alike the scalpel of the anatomist and the lens of the microscopist. Even atrophy, a condition, with the occurrence of which in other muscles every pathologist is familiar, becomes, in the instance before us, an uncertain phenomenon, to be discovered with difficulty, because the normal structure itself is so delicate that it seems scarcely possible to decide whether the appearance, in any particular case under examination, is really a morbid condition or not.

I have said that analogy would lead us to expect that occasionally, at least, these fibres should exhibit the condition of diminished vital energy, or paralysis. To Dr. Stokes the profession is indebted for having, many years ago, enunciated a pathological law as regards muscular structure, which has been universally recognized since as a well-established principle. The principle is this, that when muscular fibres have been long exposed to a continued irritation from the existence of inflammation in an adjacent structure, those fibres which at first are thrown into a state of unnatural contraction, by contact with the inflamed parts, at last become paralyzed from over-stimulation. It was in this way he explained the bulging out of the intercostal muscles, which is universally observed in the advanced stage of empyema. But this principle, which is universally admitted, when applied to muscular fibres in contact with serous membranes is equally applicable to muscular fibres in contact with mucous membranes. In fact, it has been so applied to explain appearances discovered in the dead bodies of persons who have died of ileus. Now, if this be true, and there seems no reason to doubt its accuracy, it follows as a matter of course, that long-continued inflammatory action in the bronchial mucous membrane ought frequently to impair. or even destroy altogether the contractility of the muscular fibres which lie in close contact with the under surface of the inflamed bronchi.

This idea was strongly impressed upon my mind by a case which I had recently under my care in the Adelaide Hospital, which presented some features of a peculiar character, that appeared to me to tally with what might be expected to follow from impaired energy of these fibres, and which derived immediate and obvious benefit from a plan of treatment adapted to such an hypothesis. Under the impression that this idea may be well founded, I venture to lay the particulars before the Association, in the hope that more extended observations may confirm the conclusions at which I have arrived, or, at all events, that the hints which I now throw out may lead to some practical improvements in the treatment of particular forms of chronic bronchitis.

Eliza Simpson, a married woman, forty years of age, the mother of nine children, was admitted into the Adelaide Hospital, December 12, 1859, labouring under a hard cough, with frothy and scanty expectoration, and suffering from intense dyspnœa. Her history was this: she was a laundress by occupation; twelve months previous to her admission, when in a state of perspiration, she had gone out of her washing-room into the open air to gather in some clothes, when the weather was very severe, and snowing. In this way she got cold, which affected her breathing, and from which she had never recovered; within the last four or five weeks her condition had got very much worse, her sufferings being greatest at night; for several weeks she had been totally unable to lie down.

At the period of her admission she was a picture of suffering; she was worn out from want of rest; looked pale and emaciated; her lips were livid, and her respiration, though only 18 in a minute, was forcible and laboured. On examining the chest, it was of a rounded form, hyper-resonant on percussion, with a feeling of increased resistance under the pleximeter, and with loud, sonorous, and cooing râles, audible in every direction. There *was no* lateral or antero-posterior enlargement of the thorax during respira*tion: it moved, as a whole*, as if the ribs and cartilages formed one firmly *united and unalterable* piece, and the only increase its capacity admitted of

was by the elevation of its upper margin, and the depression of the diaphragm.

The manner in which the respiration was carried on sufficiently accounted for the pain and difficulty she felt at night. The involuntary muscles not being able, without assistance, to maintain the necessary expansion of the chest to carry on respiration, each act of respiration was, to some extent at least, an effort of volition; and as the condition of sleep implies the abeyance of sensation, she was obliged to remain awake, to keep herself alive. But there was one feature in her dyspnœa which could not fail to strike the most ordinary observer, and which appeared to me to give it a peculiar character, and that was, the great disproportion which existed between the duration of inspiration and that of expiration. In health, so far as I have been able to observe, the act of inspiration is really longer than that of expiration; at all events, I am satisfied that it does exceed in duration the period of time occupied by expiration. But in Simpson's case, this ratio was reversed, the duration of expiration being fully three times as long as that of inspiration. This could be seen by the eye, as well as felt by the hand, and heard by the stethoscope. I am well aware, that in chronic bronchitis, a prolonged expiratory murmur, attended by sonorous and sibilous râles, is one of the commonest and best known signs of the disease. But even in cases where the stethoscope reveals this sign, it is rare to see the difference manifested in a marked manner to the other senses, as it was in this instance.

How is this prolonged effort of expiration to be accounted for? I believe the usual way of explaining it is, by referring it to the diminished force with which the air is expelled from the lungs, as contrasted with that with which it enters; hence, when any mechanical impediments exist to the free exit of the air, owing to congestion of the mucous membrane, or the existence of the products of inflammation on its surface, it must take a longer time than usual for the accomplishment of the effort. But if this were true, how does it happen that such a phenomenon is never observed in acute bronchitis, where such congestion undoubtedly occurs, as well as in the chronic affection? Rejecting for this reason the explanation just given, I attribute the prolonged expiratory murmur to impaired action of the muscular fibres. So long as they retain their healthy vigour and contractility, weak as they are, they materially assist the act of expiration by simultaneously diminishing the volume of the lungs; but when their vital power is impaired, by reason of the long-continued irritation to which proximity to an inflamed mucous membrane subjects them, they no longer assist, or assist but very feebly and imperfectly the other agents, whose office it is to diminish the capacity of the chest.

This, then, is what I would consider as the distinctive sign of paralysis, more or less perfect, of the muscular fibres of the bronchial tubes.

But to return to our case. She was treated on her admission with stimulating expectorants, and turpentine stupes at night, but with very little relief. A variety of expectorants were tried in succession, but without advantage. On December 24, she was put upon a mixture containing nitric acid, which agreed rather better with her than any she had previously used.

On the 26th the galvanic coil was used, and a current of electricity directed through the chest from the side of the neck to the pit of the stomach. This gave her no annoyance, but was productive of no benefit. On the 27th the report was:—"Looks a little better, but there is no material change; sleeps very little (all her sleep had to be taken in the sitting posture, or sleeping in bed with her head raised up); her breathing is very difficult." On that day she was ordered pills, containing half a grain of extract of nux vomica, and one grain of ipecacuanha, one to be taken three times a day, and her nitric acid mixture to be continued. The effect of this treatment was most surprising, and, I must confess, most unexpected. That night she was able to lie down and sleep all night; her breathing was much easier, and her expectoration free. She looked cheerful; and her appetite, which had been very deficient during the whole period of her illness, was *improved. That was the* first night, as she stated, that she had *been able for seven weeks to lie down* and sleep all night. Instead of broken

snatches of rest in an uneasy posture, she was able, without inconvenience, to resume the natural recumbent posture, and to enjoy the refreshment that none can appreciate so well as those who have been long deprived of it. This treatment was continued with the happiest effects during the remainder of her stay in the hospital; with the exception that, on the 31st of December, the quantity of the extract of nux vomica was increased from half a grain to a grain for a dose; not because the former quantity had in a measure lost its effect, but under the idea that it might the sooner enable her to return to her employment. The effect of the pills, as she described them herself, was to relieve her breathing, promote expectoration, increase her appetite, and keep her bowels open. I certainly was not prepared to find nux vomica a promoter of sleep, believing, as I do, that it is in many of its properties an antagonist to medicines of the narcotic class. Yet so it was: the poor creature, who before was almost unable to close an eye, began to sleep in the day-time, to her own infinite relief. It is scarcely necessary to say, that I do not regard this result as contradictory in any measure to the physiological properties of nux vomica. It was the consequence, not of its direct, but of its indirect action. It did not narcotize the sensorium, but it stimulated the muscular apparatus of the pulmonary system, and by so doing, removed a mechanical impediment to the proper performance of the respiratory functions. On applying the stethoscope to the chest, it was found that the bronchitic râles had lost much of their intensity; they were shorter and looser, and the expiratory murmur, as contrasted with the inspiratory, was less prolonged. Corresponding to this change in the phenomena of auscultation, there was a corresponding change perceptible to the eye and hand in the relative duration of inspiration and expiration. The former having lost somewhat of its voluntary character, was neither so forcible nor so short, whilst the latter, though still preternaturally longer, scarcely exceeded the former in duration. At the period of her discharge from the hospital, January 6, 1860, the two effects were very nearly equal in length of time.—*Dublin Quarterly Journal; May*, 1860, *p.* 299.

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35.—*The Successful Treatment of Hooping Cough, by Increasing Doses of Sulphate of Zinc and Extract of Belladonna.* By Dr. FULLER, Physician to St. George's Hospital.—[In the last volume of the 'Medico-Chirugical Transactions,' Dr. Fuller publishes a paper on the tolerance of belladonna by children. The following abstract of a paper read before the Harveian Society of London, contains a sufficient epitome of that aricle.]

Dr. Fuller commenced his paper by calling attention to the commonly-received opinion, as stated in most books on the subject, that, "Hooping-cough must run a certain course." He combatted this opinion by reference to the results of his own experience, and then proceeded to point out the causes which had led to its general adoption. Among these he mentioned the inefficiency of the treatment ordinarily employed, and the neglect of all measures likely to subdue the tendency to spasm on which the continuance of the whoop depends. Dr. Fuller regarded the complaint as consisting essentially of bronchitic irritation, usually not very severe, accompanied by reflex spasm of the air-passages, and expressed his opinion that in most cases the spasmodic symptoms are those which demand our most serious attention. Not only is the spasm a most painful symptom, but it is one which may give rise to life-long mischief. His object, therefore, had always been to subdue the spasm as speedily as possible, and the practice which, until the last twelve-months, he had found most successful for the purpose, was the administration of sulphate of zinc in rapidly increasing doses. Dr. Fuller then referred to the discovery he had made as to the tolerance of belladonna by children, which is recorded in a paper published in the 'Medico-Chirurgical Transactions,' *and he briefly* stated the facts which are there detailed in *extenso*, as to the *amount of belladonna* which may be given with safety, and the conditions *which should be observed* in its administration. The conditions he specified

were, first, the remedy should be given at least four times daily, and should be administered at first in small doses, which may be increased day by day or on alternate days, by a corresponding amount. He pointed out that mere dilatation of the pupil need not be regarded as a bar to its administration, and stated that if the precautions just referred to are observed, the daily dose of the extract of belladonna may be safely increased up to a scruple, or half-a-drachm without the production of any unpleasant symptoms. He then proceeded to state that he had brought these facts to bear on the treatment of hooping-cough, and from the conjoint use of sulphate of zinc and extract of belladonna, in rapidly increasing doses, had obtained results exceeding his most sanguine expectations. Rarely had he found the whoop to last above twenty-one days, and in some instances it had subsided in ten days. The mode in which Dr. Fuller proceeds is to give the zinc and belladonna as soon as the whoop declares itself. If the attack is accompanied by much febrile excitement, and bronchitis irritation, he prescribes a cough-drop containing a drachm of antimonial wine and a drachm of ipecacuanha wine to two ounces of water, and, if necessary, applies a blister to the chest. Of the cough-drop, a larger or smaller amount is given according to circumstances. In all cases, however, the zinc and belladonna are perseveringly administered. To children under three years of age he usually begins by giving one-sixth of a grain of extract of belladonna, and half-a-grain of sulphate of zinc four times daily; and to children above that age one-quarter of a grain of extract of belladonna and a grain of sulphate of zinc. These remedies are given in solution in water, and the dose of each substance is increased by a corresponding dose daily, or on alternate days; so that the child who began by taking one-quarter of a grain of the extract and one grain of zinc, at a dose, would be taking one grain of the extract and four grains of the zinc at a dose, either on the fourth, the sixth, or the eighth day, according to the rapidity with which the dose is increased. Dr. Fuller concluded by citing cases illustrative of the value of this treatment; assured the members of its safety, and urged its general adoption.—*Med. Times and Gazette, May* 26, 1860, *p.* 539.

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36.—*On Chronic Bronchitis and Emphysema.*—Of the number and variety of affections of the pulmonary organs which constantly present themselves for treatment at our hospitals, perhaps there is none so distressing and so difficult to relieve effectually, as the above complication. Chronic bronchitis alone requires much attention and care, but when interlobular emphysema is combined with it the disease is quite incurable, and our efforts can be at best but palliative. Although this double form of chest disease is one of the peculiarities of the aged who are subject to constant exposure, it is pretty commonly witnessed also in middle-aged and even younger persons; and in many instances wherein it is seen in apparently strong and vigorous individuals, the emphysema has been the primary affection, originating probably in rupture of the air-cells during forcible bodily efforts. To take a single example out of a number in illustration:—A hitherto strong and robust man (Nathan N.), aged forty-four years, a gas-stoker, was lately admitted into St. Bartholomew's Hospital, under Dr. Farre's care, with extensive emphysema and chronic bronchitis, which he has had for nineteen years. The suffering from dispnœa and shortness of breath during that long period, in a strong man, may be readily imagined. Now, he was suffering from the consequences of this double malady; there was anasarca of the legs, body, and face; the prominent parts of his rounded back were swollen like puddings; the face was dusky, the lips blue, and the breathing very distressing. He was subject to epistaxis; the urine contained no albumen. In the treatment, attention was paid to the bowels; sinapisms were applied to the chest; cupping was resorted to between the shoulder-blades; he was ordered a mixture of decoction of senega, carbonate of ammonia, *tincture of squills, and nitrous ether*; and was besides put upon *brandy. The improvement by* treatment was but transitory, for he became

moribund in the fourth week, and died shortly afterwards. As we learnt from Mr. Sprague, clinical clerk, there was strong pulsation in the external jugulars, and a systolic murmur of the heart, some days before death.

We might give many more illustrations of this malady even in St. Bartholomew's Hospital alone, but the present is sufficient for our purpose. Sometimes powerful medicinal treatment will do much in removing the urgency of the symptoms, but it never cures. And this leads us to ask the question, whether some relief might not be afforded by tapping the lung through the parietes of the chest (as we do in cases of thoracic fluid effusions), for the purpose of letting a certain amount of air escape, and so reduce the great pressure constantly exerted by the distended lungs upon the heart and its bloodvessels. At any rate the suggestion is worthy of remembrance in desperate cases.—*Lancet, July* 7, 1860, *p.* 8.

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37.—*Pills of Carbonate of Ammonia in Chronic Bronchitis.*—By Dr. JOHN WILLIAMS, of Cork.—Dr. Williams recommends the following formula for administering carbonate of ammonia in pills, which, he says, are of great service in chronic bronchitis, and especially in those cases where the bronchial secretion is viscid and expectorated with difficulty;

℞. Ammoniaci, gr. ij; pulv. ipecac. gr. ½; morphiæ mur. gr. ⅛; ammoniæ carb. gr. ij; mucilaginis acaciæ, q. s. Misce. Fiat pilula.

The pills should be at once coated with a varnish of balsam of tolu, dissolved in chloroform, and they should afterwards be kept in a bottle.—*Pharmaceutical Journal.—British American Journal, June*, 1860, *p.* 266.

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## DISEASES OF THE ORGANS OF DIGESTION.

### 38.—ON THE DIETETIC INVENTIVENESS OF HUNGER.

[Captain MARCY, in his interesting work 'The Prairie Traveller,' relates that when his party were yet eighteen days from the settlements of New Mexico, their supply of provisions, including tobacco, were entirely exhausted, and this when undergoing the severest labour.]

In this destitute condition we found a substitute for tobacco in the bark of the red willow, which grows upon many of the mountain streams in that vicinity. The outer bark is first removed with a knife, after which the inner bark is scraped up into ridges around the sticks, and held in the fire until it is thoroughly roasted, when it is taken off the stick, pulverized in the hand, and is ready for smoking. It has the narcotic properties of tobacco, and is quite agreeable to the taste and smell. The sumach leaf is also used by the Indians in the same way, and has a similar taste to the willow bark. A decoction of the dried wild or horse mint, which we found abundant under the snow, was quite palatable, and answered instead of coffee. It dries up in that climate, but does not lose its flavour. We suffered greatly from the want of salt; but by burning the outside of our mule-steaks, and sprinkling a little gun-powder upon them, it did not require a very extensive stretch of the imagination to fancy the presence of both salt and pepper. (There can be no doubt that the nitre and charcoal would to a great extent supply the want of salt. Soldiers and huntsmen have long resorted to this use of gunpowder under emergencies.) We tried the flesh of horse, colt, and mule, all of which were in a starved condition, and of course not very tender, juicy, or nutritious. We consumed the enormous amount of from five to six pounds of this meat per man daily; but continued to grow weak and thin, until, at the end of twelve days, we were able to perform but little labour, and were continually craving for fat meat.—*Lancet, April* 28, 1860, *p.* 435.

39.—*On the Absorption of Substances at the Stomach, and their Excretion in the Urine.* By Dr. LIONEL BEALE, Physician to King's College Hospital.—The rapidity with which weak solutions are absorbed from the digestive organs, and secreted by the kidney, is marvellous. In Mr. Erichsen's well-known experiments, it was shown that ferrocyanide of potassium could be detected in the urine within a minute after it had entered the empty stomach. These interesting conclusions were derived from experiments made on a case in which, from the deficiency of the anterior wall of the bladder and abdomen, the orifice of the ureters could be seen, and the urine collected as it trickled from them. A German suffering from this terrible malformation was in London two years ago, and I think many of you had an opportunity of seeing him, and observing how very soon after a large quantity of water had been swallowed, the rate of the flow of urine from the ureters increased.

Anything interfering with the absorption of fluid from the stomach or intestinal canal will necessarily affect the secretion of urine. In various cases where the contents of the alimentary canal are in a condition unfavourable for absorption, but a very small quantity of urine is formed. Dr. Barlow has gone so far as to say that the seat of an obstruction in the intestine can be ascertained by noticing the quantity of water excreted in the form of urine. When close to the pylorus, it is stated that scarcely any urine is separated. In ordinary cases of what is known as sick headache, where, from temporary stomach derangement, little absorption occurs for some hours, no urine is secreted perhaps for twelve hours or longer. The termination of the attack is marked by the very free and rapid action of the kidneys.—*British Med. Journal, July* 28, 1860, *p.* 576.

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## 40.—ON THE USE OF CREASOTE IN THE TREATMENT OF DYSENTERY.

By Dr. W. T. GAIRDNER, Physician to the Edinburgh Royal Infirmary.

[Of the four cases of dysentery which occurred at the Edinburgh Royal Infirmary, under Dr. Gairdner's care, during a period of three months, three recovered quickly under the use of large injections of warm water, and moderate doses of opium, chiefly administered by injection.]

One case (James A., æt. 56) was exceedingly severe, and required a protracted treatment. The patient had been ill for several months before admission; he was very much reduced, and extremely pallid. The stools, no longer bloody to any considerable extent, were passed in small quantities at a time, with great suffering, and were extremely fœtid. I had great apprehensions as to the result in this case; for, from the slight effect produced by opiates and injections, and the severe tenesmus, with tenderness on pressure along the descending colon, I formed the opinion that considerable disorganization of the lower bowel existed. I determined, under these circumstances, to try a remedy which I have frequently found useful in subacute and chronic dysentery,—viz., the addition of creasote to the large emolient and detergent enemata, which I employ in all cases of dysentery whatever, and which I believe to be by far the most important element in the treatment of that disease in all its forms. As I am not aware that creasote is in general use in the treatment of dysentery, I may state that I was led to its employment some years ago, by the statements of Dr. Wilmot as to its efficacy in an epidemic prevailing in the Union Workhouse of Pembury. I have employed it, however, somewhat differently from Dr. Wilmot, who administered it in large doses (℥i.) merely suspended in gruel. It appeared to me more likely to do good, and with less risk, if employed in smaller doses in solution, so as to be applied uniformly and certainly to the whole surface of the diseased mucous membrane. I have, therefore, usually employed the mistura creasoti of the Pharmacopœia, and have added one to six ounces of this, according to circumstances, to each large injection. It

has been followed in several cases by the best possible effects; and I can safely recommend it as a valuable addition to the resources of the physician, in cases of dysentery attended by great irritation and fœtor of the evacuations. In the present instance, I employed a solution of creasote in glycerine,* which was prepared for me by the Messrs. Smith of Duke Street, to whom I had applied to find a neutral solvent for creasote, for another purpose. The relief afforded in the case of James A. was very remarkable. The injections were repeated twice or thrice a day, the dose of creasote in each being from ℥ v. to ℥ x., and the patient experienced so much benefit from them, that he proposed of his own accord to take the remedy by mouth also. The intense irritation subsided, and the stools became much less offensive. It is right, however, to remark, that the remedy appeared to lose some of its power after a time, and that this man was finally restored to greatly improved health under considerable doses of ipecacuanha, which was well borne by the stomach, and seemed to have a very decided effect in removing the remains of a very dangerous and exhausting disease.

In a case of obstinate diarrhœa from Bright's disease, and also in the case of John W., alluded to above, creasote was also tried as a *dernier ressort;* but only with the effect of showing that its good effects are probably limited to cases of genuine dysentery, accompanied by ulceration of the great intestine. It failed, in the others, to afford even temporary relief.—*Edinburgh Medical Journal, May* 1860, *p.* 994.

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41.—*On the Use of Santonine in the Treatment of Lumbrici.* By Dr. S. Brisbane, Galashiels.—(In the treatment of ascarides, santonine is of little or no use, but Dr. B. says:)

I may mention, however, that I have used it in the lumbricoïdes with the most surprising, or I had almost said, specific effects. Much depends on the method of administration. Before using santonine I had tried Dr. Watson's method of treatment by steel and purgatives alternately, with only trivial benefit. I also tried santonine by itself, and found it scarcely more efficacious than the former, the only advantage being that from its small bulk, it could easily be administered to children.

On resorting to the method suggested by Kuchenmeister of administering it with castor-oil, the effect was most gratifying. The first patient to whom I administered it in this way, a child fifteen months old (and who had been treated by the two former methods without benefit), passed no fewer than thirty-six worms, varying in size from a few inches to nearly eighteen inches. On repeating the dose several more were expelled. The effect on the child's appearance was really wonderful, from being a tiny emaciated creature, with a ravenous appetite, &c., it in the course of a short time assumed a plump, healthy aspect.

Since that time I have used it very extensively, and know many instances where from twelve to forty and even in one instance fifty-nine worms have been voided, and that, too, where Dr. Watson's method and the santonine *per se* had signally failed. Indeed I may say, where children have previously passed worms (which is considered the only unequivocal evidence of their existence), that I have never known the santonine and castor-oil together fail to eject more of them when administered.

I usually order from two to eight grains of santonine, stirred in from a tea to a table spoonful of castor-oil, according to age, to be administered at night and a dose of senna the following morning.—*Medical Times and Ga-*, *June* 9, 1860, *p.* 580.

*proportions used were creasote 20 minims, to glycerine one oz. The materials require to be to be well rubbed up together in a mortar. The result is what appears to me a perfect though Mr. Smith doubts its being so. The subject deserves the attention of the framers w National Pharmacopœia.*

## 42.—ON HYDATID TUMOUR OF THE LIVER.

By Dr. Garrod, Physician to the University College Hospital, &c.

(The liver is perhaps of all other organs the most liable to be infested by hydatids; luckily something can sometimes be done in these cases, whereas when in most other organs little or nothing whatever can be done.)

Assuming that an hydatid tumour of the liver has been satisfactorily made out, the question naturally arises in the mind of the physician, whether there is a single hydatid or a number of them present; for, unfortunately, it often happens that they exist in countless numbers, disseminated throughout the hepatic tissue. Dr. Watson in his lectures, mentions the case of a girl who died in the Middlesex Hospital, whose liver contained thousands of these globular bodies, the enlarged gland having, by its pressure, completely sealed up a portion of the inferior cava.

Experience proves, however, that it is seldom that more than three or four are developing in various parts of the organ at one time; but it is this circumstance which constitutes the difficulty, and we may say the danger of interference; for if one is collapsed through evacuation of its contents, the others enlarge, and the patient dies at last from exhaustion.

Dr. Budd has afforded some useful hints on the treatment of hydatid tumors of the liver, which are of great practical importance. It is well known that hydatids sometimes spontaneously lose their vitality; the sac shrinks up, its inner surface secretes a peculiar matter compared to glaziers' putty from its appearance, and the tumor ceases to be of any further danger or inconvenience beyond that which arises from its mere presence. Now Dr. Budd proposes to imitate this state of things by art, if practicable, and he thinks it probable that it may be attained by medicine; but so far, the latter, except in a very few instances, has proved wholly unsatisfactory. He advises the tapping of the hydatid cyst through a *fine exploring trocar*, by means of an exhausting pump, drawing off all the water, and taking the precaution that not a particle of air shall enter the cist—the danger of putrefaction of the dead hydatids being thus completely obviated. An instrument of the kind is prepared by Weiss, according to the plan recommended by Dr. Bowditch of Boston, U.S. Dr. Budd further advises the precaution of inserting the trocar downwards, with the outer extremity of the instrument at a higher level than the body, so that as the liquid rises in the canula it will expel the air from it. If the vitality of the hydatid is completely destroyed in this way, should others enlarge and become prominent, they may be submitted to the same plan of treatment if suppurative action does not spontaneously occur; should this take place, however, the nature of the case becomes changed to one of a more serious character. If there be but a single cyst in the liver, and no fresh growth make its appearance, a cure may be considered to have taken place.

The three examples of this affection which we record to-day were all tapped, but not with the precautions recommended by Dr. Budd; hence, in two, suppuration ensued; whilst in a third, at St. Thomas' Hospital, the sac had already undergone that process, and it was treated like a large abscess. In the following example the tumor was remarkably prominent just below the ensiform cartilage; it was tapped by a very fine trocar, and four ounces of aqueous and colorless fluid were withdrawn. This was followed by local peritonitis and rigors, which ushered in the suppurative crisis; and in three weeks from the first puncture an evacuation of pus was effected by a trocar. Afterwards matter with fœtid gas escaped from the old opening, and at the present time the former continues to flow to the extent of a few ounces daily. The general bulk of the liver has become diminished and the disease is now chronic; but it is most probable that there is only a single hydatid in this instance, although at first it was suspected that there might be more.

*Jane J., aged nineteen, admitted* March 23d, 1860. Has enjoyed good *health up to two years ago*; the catamenia appeared at fifteen, and have

always been regular until six months back, when they became irregular. Two years ago she had an attack of jaundice, which continued for four months, but did not confine her to bed. She states there was no tumor in the hepatic region at that time. After the attack of jaundice her health remained perfectly good again until last November, when she felt shooting pains in the back, under the right scapula, not extending down the arm, and at the same time discovered a swelling in front near the lower end of the sternum. This tumor gradually increased in size, and she soon felt an aching pain in it of a continuous nature. The patient thinks it was about two months after the discovery of the first that she noticed another small swelling beneath this. The swellings gradually enlarged, and became more painful, and at the same time her appetite began to fail. A month ago she was admitted into the hospital under Dr. Murphy's care, and the tumors have been painted over with compound tincture of iodine every day, without, however, diminishing them, or giving much relief. Since her second admission, another small swelling has made its appearance, external to the other two. Bowels have always been regular.

March 27.—Present state: Patient is thin and somewhat emaciated; muscles rather irritable; but in a better condition than on admission a month since. Hepatic dulness extends as high as the level of the right nipple, and below to horizontal line through umbilicus. From about three inches below the right nipple considerable bulging is observed, extending to the left, two or three inches beyond the median line. This bulging seems to be composed of two protuberances, the larger occupying the epigastrium and part of the right hypochondriac region, the smaller below and somewhat to the left. No tenderness felt over prominent parts, but much below them. On pressure over the large central protuberance some sensation of elasticity is experienced, and sudden pressure on one side is propagated to the other. Conjunctivæ not turgid. On examining the back, some distinct but slight bulging is seen in hepatic region, and dulness on percussion extends about an inch higher on the right than on the left side. Slight curvature of the spine is noticed, the concavity towards the right side. She was put upon sarsaparilla, three grains of iodide of potassium, and decoction of cinchona, thrice a day.

28. A fine trocar was passed into the center of the larger mass in the epigastrium by Mr. Erichsen, when the suspicion of the hydatid nature of the tumour was confirmed; for a clear fluid (four ounces) was drawn off, which was found to contain minute flakes, each consisting of a collection of well-formed echinococci. After the tapping a compress was applied, and, with the exception of slight vomiting, no bad symptoms appeared.

30. Some pain and tenderness in the region of the puncture.

31. Much relieved from pain; tumour not much diminished in size, but the note over it is quite tympanitic.

April 1. Had an attack last night of rather severe rigors, with the return of pain over the swelling. To-day the pulse is 130, very weak; cheeks flushed; suffering great pain over the region of the liver on the slightest movement, deep inspiration, &c. Slight tympanitis of the abdomen.

3. Has less pain to-day; no return of rigors; still much tenderness over the hepatic region.

10. Pain has varied since the 3rd, but she feels much better; the central tumour appears larger and more tense.

19. Yesterday about six or eight ounces of thick fœtid pus were obtained by tapping the centre of the tumour. The tumour became flaccid for a time, but to-day is again distended. The complexion and the eyes are less yellow than they were.

24. On the night of the 22nd the tumour gave way from the old opening, apparently discharging air at first, then watery fluid, and afterwards thin fœtid pus. Yesterday the discharge of matter continued; but to-day it has *ceased. The patient* remains comfortable as before.

*May 1. Since the last* report the tumour has been discharging a mixture *of pus and blood. The* skin nearly natural in color.

22. For the last few days the tumour has been again plugged up and very painful; patient much exhausted; pulse 130. Ordered a mixture of quinine.

24. The abscess discharged two or three teacupfuls of pus, after which she became better.

June 5. The tumour much diminished in size, and discharges pretty freely.

July 5. She remains much the same; very weak; unable to get up. Two grains of sulphate of iron to be added to her quinine mixture.

Aug. 2. She suffers less pain; discharge still continues, and amounts to about four ounces in the twenty-four hours; has not lost more flesh; the tongue is clean; and she is free from pain. She is picking up her strength, and is daily gaining flesh.—*Lancet, Sept.* 1, 1860, *p.* 310.

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## 43.—ON SOME CHEMICAL QUESTIONS IN CONNEXION WITH DISEASES OF THE LIVER, AND THEIR TREATMENT.

By Dr. Thudichum.

[There are many points in this paper respecting practice, which we ought to attend to. Hitherto, affections of the liver have been treated very empirically. Calomel and blue pill, with some aperient, have often been the sole remedies employed without any scientific object in view. In a former paper Dr. Thudichum referred to gall-stones, in the centre of which he had detected certain peculiar formations, which he thought were casts of the biliary ducts. This opinion has since been confirmed. Dr. Thudichum goes on to say:]

As matters now stand, essential constituents of gall-stones seem to be cholochrome, cholic acid, and earthy salts. In man, gall-stones contain a large amount of cholesterine in most cases; but that is a secondary and accidental ingredient, and is mostly crystallized around the other matters forming the nucleus. There are human gall-stones which, like those from the ox, contain no cholesterine. Others are said to consist entirely of cholesterine—a statement which requires to be verified by fresh analyses, which must prove the absence of cholic acid. Stearate and palmitate of lime are usually present in human gall-stones. The idea of an inspissation of mucus in the formation of gall-stones was shown by the author to be untenable; the binding material of the concretions is cholic acid.

The process of the formation of gall-stones was explained by Dr. Thudichum to be perfectly analogous to that which produces phosphatic calculi in the urinary bladder. The compound amido acids (taurocholic and glycocholic) split up under the influence of a cause of which the nature remains to be ascertained—probably a ferment from the intestinal canal. They deposit cholochrome and a portion of cholic acid, some salts, and a little fat, under the influence of acetic acid derived from glycocoll. In man, gall-stones contain cholesterine; this has, in the healthy bile, been held in solution in taurocholate of soda; but when the taurocholic acid is decomposed, the cholesterine is set free, and is deposited on any particles which may be at hand. In other words, the process of formation of gall-stones is the following:—

Healthy bile contains taurocholic and glycocholic acids, in combination probably with soda. Under the putrefactive change, these two acids split up; the taurocholic acid yielding taurine; and the glycocholic acid, glycocoll. Cholate of soda is left; the glycocoll is destroyed, and is transformed into acetate of ammonia. Valerianic acid is also formed, and combines with the ammonia from the glycocoll. Some free acetic acid then sets free the cholic acid from the cholate of soda, and the cholic acid is then precipitated in combination with cholochrome, so as to form casts of the tubes. This takes *place in the ox; in man, cholesterine* is also deposited in consequence *of the breaking up of taurocholate* of soda, which has held it in solution.

Within the last few weeks, Dr. Thudichum had examined the products of decomposition of fibrine, and had found amongst them cholesterine; which, with a gritty matter and an oil-like body, were left when all the matters soluble in water were removed. The presence of cholesterine and gritty matter in atheroma led to the question whether this condition was not a simple result of the decomposition of fibrine or of albuminous matter from which the earthly particles had been washed out. It was further a question whether the cholesterine in gall-stones might not in part arise from the decomposition of cholic acid.

Dr. Thudichum then noticed that the healthy action of the liver-cells consists in the absorption of certain albuminous, fatty, starchy, and saccharine matters, and their transformation into the ingredients of bile. When this becomes impaired, the cells assume a fermentative tendency, leading to the same description of decay in the albuminous matter as that which is induced by putrefaction or by treatment by acids or alkalies. He then proceeded to point out that the general principles elucidated in his paper could be applied in the treatment of all liver diseases. In jaundice with obstruction of the biliary ducts, we should make early use of nitric acid or nitro-hydrochloric acid. He had with great benefit given a watery solution of nitrous acid. It is easily made, and may be drunk as a pleasant lemonade. It is less apt to produce griping, which, during courses of nitric acid and aqua regia, is not rarely complained of. This acid not only destroys and lixiviates the deposits of cholochrome and other amido-acids, but it also tones the digestive organs, and acts antiseptically. Nevertheless, a remedy to stay the abnormal action of the liver-cells we are yet in want of. Creasote is beneficial. In all cases where the disorder of the liver proceeds from the intestinal canal particularly, creasote, charcoal, astringents like gallic acid, and preparations of iron, particularly the tincture of the sesquichloride given together with some nitric acid, are highly beneficial. In a case of desquamation of the epithelium of the kidneys consequent upon chronic jaundice, he had exhibited this latter mixture with the most decided benefit.

Nitric acid was introduced into the treatment of these diseases by Annesley. Many have used it since with variable success. Dr. Thudichum believed that it is frequently not borne because it is not sufficiently diluted. The more it is diluted, the quicker it is assimilated, and the more certain are its effects.

It might appear paradoxical that a free acid should travel through the blood, and act upon certain parts through tissues. We are, nevertheless, obliged to admit the possibility of that occurrence. Thus oxalic acid and many other acids have been proved to have passed through the blood uncombined, and have been found in the urine in a free state. Even allowing that blood so mixed with nitric acid might show a precipitate, there is no reason why a molecular precipitate in the blood, which is full of molecules of its own, should not again dissolve.

All liver diseases require the greatest attention to the intestinal canal. Mild purgatives, of salts or fruit, Pullna water, Seidlitz powders, waters containing one per cent. of ordinary chloride of sodium and a little sulphate, are the best remedies. If stronger purgatives are required, the vegetable ones deserve preference. The fatal error of treating with mercurials the various disorders of the liver, he could not sufficiently deprecate.

The diet should be juicy, include fruit and vegetables, and eggs and meat. Starch and fat are always badly digested.

The skin must be well attended to. Baths, with some aqua regia, stimulate the skin and open the epidermis.

In true bilious attacks, with indigestion, a furred, sometimes yellow tongue, constipation, sickness and vomiting, headache, yellow appearance of face, and albuginea, he had found the use of blue pills mostly hurtful. Whenever he had been called upon to treat such an attack, his treatment had to deal with the effects of the biliosity and blue pills combined. In cases *where such attacks, without* previous excesses, become habitual, gall-stones *are not rarely developed.* I have known two cases end in phthisis. One,

that of a gentleman, married, about forty years of age, is yet under observation. In many cases of phthisis, in which gall-stones are found after death, this habitual biliosity did not rarely precede the disease of the lungs. But phthisis may also produce gall-stone disease.

Lastly, he adverted to the use of soluble phosphates. He had made the observation that bile contains no small amount of phosphate of soda. Without this salt, the assimilation of the fats must needs be imperfect. Considering, therefore, that the absence of the phosphate may be hurtful, and that its presence never has been found to be so, he was inclined to give it a further trial. It should be given in the food, together with chloride of sodium, or in a dilute solution in water. Since he exhibited before this Society the action of phosphates upon fatty acids, with which they produce an emulsion ready for transition into the blood, he had thought of combining the acids of cod-oil with phosphates, in order to make this substance more generally digestible.

Liebig has long since shown that the most pleasant manner of giving soluble phosphates consists in the administration of beef-tea; but where this extract cannot be had in a quite reliable form, a weighed quantity of the ordinary phosphate of soda, say to the amount of half a drachm or a drachm per day, or more where it is liked and well borne, is preferable.

In bile, soda preponderates over potassa. In the substance of the liver the proportions are reversed. The relations of these two bases require further study. It is not probable that the one may substitute the other, and *vice versa*; and hence the want of potassa may become a source of disease. It is known that this base prevails in the muscles, soda in the blood.

By thus attacking the liver from all sides, we shall soon succeed in obtaining a surrender of its secrets. This organ can in this way only be stimulated, while it is out of the reach of so-called medicinal stimulants. Torpid liver, sluggish fever, and the like, may be useful practical denominations; but they are not based upon any positive evidence. We want much less cholagogues than we require remedies to appease, subdue and change the activity of the liver. The organ is accused of much of which it is not guilty. It is by the mouth that most diseases enter; and it is from the insults received from the direction of the intestinal cavity that this patient and enduring organ mostly derives its detriment.—*Lancet, Oct.* 20, 1860, *p.* 388.

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## DISEASES OF THE URINARY ORGANS.

### 44.—ON THE TESTS FOR ALBUMEN IN THE URINE.

By Dr. Lionel Beale, F.R.S., Physician to King's College Hospital, &c.

The most important soluble substances present in the urine in disease, but absent in the healthy secretion, are *albumen*, *biliary constituents*, and *sugar*. The clinical importance of these is so great, that they deserve special attention.

*Albumen.*—The occurrence of albumen in the urine has been regarded as a most important pathological fact ever since the connexion between dropsy and disease of the kidneys was established by Dr. Bright. Albumen, it need scarcely be said, is absent from the urine of healthy persons, although now and then it may be detected for a short period of time in the urine of individuals who are not suffering from any serious or permanent derangement of health. The presence of albumen must always be regarded by the physician as a point of serious importance, although at the same time, *per se*, it cannot be taken as evidence of the existence of any organic lesion, unless it has been clearly detected from day to day for a certain time. Many of the cases which give rise to the escape of serum from the vessels in other parts of the body, independent of disease, will determine its transudation through the walls of the renal capillaries, and, as a matter of course, it will be found in the urine.

To recognise with certainty the presence of a substance in the urine having so important a bearing in the discovery and interpretation of certain morbid processes, as albumen, is obviously a point of the utmost importance to the practitioner. In the examination of the urine of patients, simple tests are at once applied, in order to determine if this substance be present or absent. In a majority of cases. such information is easily obtained; but occasionally an instance occurs in which, without great care, an erroneous conclusion is likely to be arrived at after the ordinary tests have been applied. As this question is one of very great practical importance, and of much interest, I propose to consider it at somewhat greater length than is usual in works devoted to the clinical examination of urine. The reactions to which I shall refer are not imaginary, but have actually occurred; and I have known instances in which albumen was stated to be absent when the urine contained a large quantity; and other specimens have fallen under my notice, which, although they really contained none, yielded a precipitate having many of the characters of albumen. Let us then investigate the phenomena *seriatim*.

1. *Albumen is usually precipitated from its solution upon the addition of a few drops of nitric acid.* Heller recommended that acid should be allowed to flow to the bottom of the tube containing the urine. In this manner, three strata are formed, the lowest stratum consisting of the pure acid, above which is the precipitated albumen, while the upper stratum consists of the fluid containing the albumen uncoagulated.

2.—*Albumen is also generally coagulated by the application of heat* (140° to 167° Fahr.). If very dilute, a higher temperature is required. The best way of testing urine by heat is the following:—An ordinary test-tube is about half filled with the urine, and is to be held by the lower part. Heat is applied to a point near the *surface* of the fluid; the tube being shaken a little at the time, to prevent the glass being cracked. The slightest precipitate cannot fail to be observed, as the fluid below remains perfectly unchanged. When urates are present, this plan is very useful, as we get three distinct strata; the upper one formed of coagulated albumen; the next clear, in consequence of the solution of the urates at a temperature somewhat below that necessary for the coagulation of the albumen; and lastly, the unchanged urates.

3. *If the solution of albumen be alkaline, no precipitate will be produced by heat. We are, therefore, generally directed to neutralise the alkali by an acid before heat is applied.* If excess of acid be added, the albumen is, of course, precipitated in the insoluble form, without the application of heat.

Frequently specimens of urine are met with which exhibit one or more of the following peculiarities, which tend either to make us believe that albumen is present when it is not, or cause us to conclude that it is absent when the urine contains it.

1. Upon the application of heat, the specimen may become turbid in consequence of the precipitation of phosphate. The reaction of the urine in this case would generally be neutral or feebly alkaline; but sometimes urine depositing phosphate on the application of heat is of a decidedly acid reaction.

2. Upon the addition of nitric acid, the specimen becomes turbid, in consequence of the decomposition of the urates held in solution in the urine, and the decomposition of uric acid in a granular state. If the acidified urine be boiled, it usually becomes clear with the development of a pinkish or brown colour, consequent upon the decomposition of the uric acid.

3. Upon adding nitric acid to some specimens of urine of high specific gravity, an abundant precipitate of a crystalline character is produced. This consists of nitrate of urea, and is easily recognised by its crystalline character. It seldom appears immediately, and is hardly likely to be mistaken for albumen, except by a most superficial observer.

4. After adding a drop or two of nitric acid to urine suspected to contain *albumen, in order to* render it distinctly acid, no precipitate is produced *upon boiling, although a large quantity of albumen* may be present. This is con-

stant in all specimens of albuminous urine, and shows the importance of never boiling urine suspected to contain albumen in a tube which may contain, by accident, a drop or two of nitric acid.

5. Cubebs, Copaiba, and some other resinous substances, taken internally, are said to give rise to precipitates in the urine which are liable to be mistaken for albumen.

*A Precipitate produced in Urine containing no Albumen.—Phosphate resembling Albumen.* The precipitate of phosphates is very readily distinguished from albumen by its solubility in a little acid. Upon the addition of a few drops of nitric acid, the turbidity instantly disappears, and the solution becomes perfectly clear.

*Uric Acid resembling Albumen.* When a precipitate of uric acid in a minute state of division is caused in consequence of the decomposition of the urates by nitric acid, its nature may be ascertained by allowing the mixture to stand for sometime, when the minute granules gradually increase in size, and at length become crystals, the nature of which is at once recognised upon microscopical examination. In some cases the crystals may be seen to form under the microscope. This precipitation of uric acid on adding nitric acid often leads to mistakes, and albumen is stated to be present in urine which really does not contain a trace. Several cases of this precipitate have occurred to myself, and I have heard of many others in the practice of friends. It has happened in the wards of our hospital, that the precipitate produced by nitric acid has been inferred by the clinical clerk to consist of albumen, when subsequently no precipitate could be obtained. The fallacy was explained by careful examination afterwards.

Dr. G. O. Rees has met with urine affording this precipitate of uric acid on the addition of nitric acid in cases of typhoid fever. Most of the instances which I have met with occurred in cases of liver affection. I extract two or three as examples.

*Urine from a Patient suffering from large Hydatid Tumours of the Liver.* A small quantity of urine was filtered, and, upon the addition of a little nitric acid, a precipitate was produced. After standing a little while, this was examined by the microscope, and found to consist of minute crystals of uric acid. These were dissolved upon the application of heat; but, as the solution cooled, they were deposited again in the form of much larger crystals.

A specimen of urine exhibiting the same peculiarity contained excess of urea. Upon the addition of half its bulk of nitric acid, the mixture became nearly solid, from the formation of crystals of nitrate of urea. The deposit in this instance consisted partly of urate of soda.

Another example of which I have kept notes, occurred in a man aged 49, suffering from *rheumatic fever*. The urine was acid, specific gaavity 1027, and containing much urate of soda. The practitioner who first saw the case boiled a portion of the urine. It remained clear; and he said, therefore, that it contained no albumen. A physician afterwards tested a portion of the same urine with nitric acid; and, finding that an abundant precipitate was produced, affirmed that much albumen was present. The deposit produced by nitric acid was found, by subsequent examination, to be dissolved by heat; and, when a portion was examined in the microscope, its true nature was decided by the presence of numerous uric acid crystals.

*No Precipitate produced in Urine containing Albumen.—Albumen not coagulated by heat when a little nitric acid is present.*—Upon the careful addition of a drop of nitric acid, the precipitate at first formed when the acid comes in contact with the urine, slowly dissolves as it descends toward the bottom. Upon boiling this *acidified solution, no precipitate of albumen will take place.* Upon the further addition, however, of nitric acid, the albumen is precipitated.

This reaction has often led to mistakes. Not unfrequently albuminous urine has been poured into a test-tube which contained a trace of the nitric acid remaining from some previous experiment; and, upon boiling the mixture under these circumstances, no precipitate of albumen has occurred. If *a few drops of a dilute solution* of nitric acid be added to a portion of albu-

minous urine in a test-tube, and the mixture boiled, no precipitate will take place. In fact, the addition of a little dilute nitric acid will prevent the coagulation of albumen by heat. Dr. Bence Jones was, I believe, the first to explain this fact, in a communication to the editor of the 'Medical Gazette' (vol. xxvii, p. 289); and, so far as I can ascertain, no other explanation for the circumstance than that offered by him has been given. Dr. Jones thinks that the solution of albumen is owing to the formation of a nitrate of albumen which is soluble in a weak solution of nitric acid, even although boiling, but insoluble in a mixture of acid of moderate strength. Dr. Bence Jones has also shown that albumen is not always precipitated from very acid urine upon the application of heat. From some experiments I have made, however, I have been led to conclude that the above result depends upon the decomposition of the phosphates by the nitric acid, and the consequent development of free phosphoric acid in which acid albumen is freely soluble. This view has been confirmed by some experiments which I made sometime since on the subject, and which have since been many times repeated. A weak solution of albumen was treated with a few drops of chloride of calcium, and afterwards with a little ammonia. After having stood for twenty-four hours, it was filtered. In this manner, any soluble phosphates present were removed. The solution was then tested as follows:—

1. Albumen was precipitated by the application of heat, or by the addition of nitric acid, as usually occurs.

2. A very small quantity of dilute nitric acid did not prevent the coagulation of the albumen by heat.

3. After the addition of a few drops of phosphoric acid, the fluid no longer coagulated upon being boiled.

Some of the same solution as the above, which had not been treated with chloride of calcium and ammonia, afforded the same results upon the application of the tests as other albuminous solutions. A few drops of a weak solution of nitric acid, or a little phosphoric acid, prevented the precipitation of the albumen by heat. The addition of phosphoric acid to an albuminous solution, or a soluble phosphate and a little nitric acid, prevented the precipitation of the albumen by heat.

These results, therefore, led me to conclude that a trace of nitric acid prevents the coagulation of a moderately strong solution of albumen by heat, in consequence of decomposing the phosphates and setting free phosphoric acid, in which the albumen is soluble. When, however, excess of nitric acid is added, its action predominates over that of the phosphoric acid, and the albumen is precipitated.

At the same time, it must be admitted that there are several facts connected with the behaviour of weak solutions of albumen with acid, and under the influence of heat, which are not satisfactorily explained; and forms of albumen having different reactions are from time to time met with. The whole subject requires further careful investigation.

Scherer describes a variety of albumen which is only imperfectly coagulated by heat. It is probable, however, that many of the peculiar reactions met with from time to time depend upon the presence of other substances dissolved with the albumen, rather than upon any peculiar properties of the albumen itself, or the existence of a variety of this substance.

*Other Tests for Albumen.*—Albumen is precipitated from its solutions by alcohol, alum, and many metallic salts, as those of lead, mercury, copper, and silver. Bichloride of mercury is employed as a test, and ferrocyanide of potassium precipitates a solution of albumen to which acetic acid has been added. These salts will, however, produce precipitates in solutions of other substances allied to albumen.

*On Estimating the Quantity of Albumen in Urine.*—The quantity of albumen varies much in different cases, sometimes amounting to a mere trace; while, in other instances, a proportion not much inferior to that present in serum has been met with. In order to estimate the quantity of albumen, it is only necessary to add acetic acid, and heat the urine in a water-bath to a temperature of 194°, or until it boils. The precipitate is to be collected on a

weighed filter, well washed, dried, and weighed. The albumen always contains a small quantity of earthy salts, which are obtained by incineration. The residue must be deducted from the weight of the dried precipitate.

*Albuminous Urine*, from a patient with acute inflammation of the kidney. The deposit contained numerous granular casts, but no fat-cells were present; specific gravity, 1015; acid. The albumen coagulated by heat and nitric acid.

*New Substance allied to Albumen.* Dr. Bence Jones obtained a new substance allied to albumen from the urine of a patient (under the care of Dr. Watson and Dr. MacIntyre) suffering from mollities ossium. The urine was slightly acid; specific gravity, 1034·2 (*Phil. Trans.* for 1848, p. 55.) The deposit consisted of phosphate of lime, and cylinders of fibrine. Phosphates were precipitated by heat; but the urine was cleared by adding a drop of acid. No precipitate was produced by nitric acid; but, after being heated and left to cool, it became solid. The solid material was redissolved by heat, and precipitated again when the mixture became cool. On some days, the urine coagulated by boiling; on others, prolonged boiling produced no change. A specimen, which did not coagulate by boiling, was carefully examined. It was acid; specific gravity, 1039·6. It contained much urate of ammonia, phosphate of lime, and oxalate of lime.

The new substance was precipitated from the urine by alcohol, well washed, and ultimate analyses were made. It contained 1·09 per cent. of sulphur, and ·20 per cent. of phosphorus. This substance is the *hydrated deutoxide of albumen*. It was soluble in boiling water, and the precipitate produced by nitric acid was redissolved by heat, and it formed again as the mixture cooled. A similar substance occurs in small quantity in pus, and in the secretion from the vesiculæ seminales. The urine contained 66·97 parts per 1000 of this substance—an amount equal to the quantity of albumen in the blood. The patient was passing about 35 ounces of urine daily, which would contain upwards of 1000 grains, or more than two ounces of this new material.

Dr. Bence Jones recommends that this substance should be looked for again in acute cases of mollities ossium. He suggests that the reddening of the urine upon the addition of nitric acid might lead to its detection.—*Brit. Med. Journal, April* 21, 1860, *p.* 297.

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## 45.—ON THE IMPORTANCE OF ALBUMEN IN THE URINE IN A CLINICAL POINT OF VIEW.

By Dr. Lionel Beale, F.R.S., Physician to the King's College Hospital.

In the majority of cases, in which the urine contains a very large quantity of albumen, and especially if the urine be of specific gravity of 1020 or higher, the inference will be that the case is an acute one, and that this large quantity of albumen has not been passing from the kidney for any length of time. By far the majority of these acute cases recover if the patients are placed under favourable circumstances. In some instances, however, the circulation through the kidney becomes more and more obstructed; the urinary constituents accumulate in the blood and seriously impair the various actions going on in the body, and especially affect the nervous system; and death results, probably preceded by coma and sometimes by convulsions. Occasionally pus is found in considerable quantity in the uriniferous tubes. Sometimes the acute stage passes off, and the albumen, although it diminishes in quantity, does not entirely disappear from the urine, and the acute attack afterwards proves to have been the commencement of chronic kidney-disease.

If the quantity of albumen be small, amounting nearly to milkiness or opalescence when heat is applied, or nitric acid added to the urine, and especially if the urine be pale and of specific gravity 1012 or lower, we should suspect that the lesion giving rise to the escape of the albumen was chronic,

and would ultimately destroy life. If the proportion of urea to the other constituents of the solid matter were large, we should form a more favorable opinion than if the percentage in the solid matter were very much less than in health. In the latter case, a great part of the renal structure would probably be involved; but, in the former, there would be reason to think the disease had only affected a certain number of the secreting tubules. There are, however, some exceptions to these general statements. Patients have passed small quantities of albumen in the urine for many months, and it has afterwards disappeared. In other patients the progress of the disease is exceedingly slow. I have known a man pass urine of the character above mentioned for upwards of twelve years; and I believe that this might go on for twenty years, or even longer, the patient perhaps dying at last of some other malady. Under favorable circumstances, the life of patients suffering from certain forms of chronic kidney-disease may undoubtedly be prolonged for years; but it must always be borne in mind that such persons are more likely to suffer from exhausting influences, cold, fatigue, &c., than others; and an attack which would cause no alarm for the safety of a man previously in good health, might be rapidly fatal to them.

It is very important that the medical officers of Life Insurances should be aware that there are many instances of persons having chronic disease of the kidney, who are not themselves aware of it. Neither is there in many of these cases anything in the appearance or history of the person that would cause the physician to suspect the true nature of the case. The discovery of albumen in the urine, or of casts in the deposit, is sometimes the only point which leads the practitioner to a correct knowledge of the doubtful nature of the life. Now, if one of these persons experienced a severe attack of catarrh, his life might be endangered; and any exposure to cold would be very likely to set up acute inflammation of the kidneys, already impaired by disease, and thus prove fatal. The only way to discover such a condition, is to institute a careful examination of the urine in every case; but to carry this out practically, it must be confessed there are many difficulties and objections. A microscopic examination of the deposit in the urine will probably throw much light upon the case, and enable us to diagnose the condition with much greater precision than is possible from a mere chemical examination. We cannot be too cautious in arriving at a prognosis in these cases. Every circumstance connected with the individual case must be carefully considered, and the state of nutrition, the progress the disease has made in a given time, the state of other organs, the constitution of the patient, his circumstances, temperament, &c., must be passed in review. Even with the greatest care, we shall seldom feel justified in expressing any but a very guarded opinion as to the probable duration of life.

Albumen is always found in small quantity in urine containing pus. We shall therefore meet with it in cases of inflammation of the pelvis of the kidney (pyelitis), and in cases of inflammation of the bladder and of the mucous membrane of the urinary organs generally. Whenever blood is present in the urine albumen is detected; for, if blood-corpuscles escape from ruptured capillaries, a certain quantity of serum must at the same time pass through the same apertures. The coloring-matter of the blood is sometimes passed in urine in a state of solution; but in this case also, a certain quantity of albumen is present.

Dr. Bence Jones detected albumen in the urine of a patient who passed spermatozoa. The urine passed in the morning contained spermatozoa and albumen. The evening specimen contained neither. On a subsequent examination, no albumen could be detected.

I have detected albumen in many cases of pneumonia during the period of hepatisation of the lungs. It is present, also, in the specimens of urine first passed after the period of suppression in cholera. In some cases of acute rheumatism with pericarditis it is observed; and it has been occasionally detected in continued fever. In puerperal fever it is often met with, and in puerperal convulsions it is almost constantly present. Dr. Lever found that it was absent in only one case out of fifty. The pressure of the gravid

uterus is probably a cause of the albuminous urine met with in some cases of pregnancy, but it cannot always be referred to the same cause, for it occurs at an early period of pregnancy, when the uterus is too small to exert much pressure. Dr. Tyler Smith considers that it is to be accounted for by an influence exerted upon the nerves, in those cases in which it is not connected with organic disease. After intermittent fevers albumen frequently escapes in the urine. In cases in which any physical impediment to the return of blood in the emulgent veins or inferior cava exists, and in some cases of obstructed portal circulation, as in cirrhosis of the liver, traces of albumen may be detected in the urine. In anæmia, and in cases of dropsy depending upon an impoverished state of the blood, albumen is often passed. Sometimes a large quantity of blood extractive matter is also present. After long continued hemorrhages, when dropsy occurs, we not unfrequently find albumen in the urine. In these cases it does not depend upon kidney-disease, but upon the state of the blood. Just as serum escapes from the capillaries of various tissues of the body, it is prone to transude through the renal vessels. Lastly, in persons who have suffered for many years from affections which produce alterations in the capillary walls, albumen may pass off in the urine; and towards the termination of exhausting diseases it is frequently present.

In those cases in which its presence depends upon obstruction to the circulation in the kidney, the impediment may be *functional or temporary*, or it may be *organic and permanent*. As examples of the presence of albumen depending upon temporary congestion, may be adduced certain cases of pneumonia and cholera, cases of acute dropsy, and of dropsy consequent upon scarlatina, with many others. Fatty degeneration and chronic nephritis may be brought forward as instances of structural disease, giving rise to such impediment to the circulation of the blood as to cause albumen to transude through the capillary vessels.

In the majority of cases, the vessels of the Malpighian tuft doubtless form the precise seat of the escape of albumen; but there are reasons for believing that albumen sometimes passes from the capillaries surrounding the convoluted portion of the uriniferous tubes, and in some instances from those in contact with the straight portion. ('Archives of Medicine,' vol i., p. 300.)

In most cases in which albumen occurs in the urine, casts of the uriniferous tubes are also found; for with the serum a certain quantity of coagulable material transudes, and this becomes solid while it lies in the tube, of which it thus takes a mould, and entangles in its meshes any loose bodies, as particles of epithelium, &c., which may happen to be in the tube at the time. In the first series of cases alluded to, casts are often absent, or, if formed, they are perfectly transparent. On the other hand, where the structure of the kidney is altered, the casts often afford evidence of the nature of the lesion. We shall consider this part of the subject in the next lecture. Albumen is, however, often present without any deposit, so that for its detection we must rely solely on chemical tests.—*British Med. Journal, April* 28, 1860, *p.* 315.

## 46.—ON RENAL CASTS.

By Dr. Lionel Beale, F.R.S., &c.

In many cases of congestion, and in inflammation of the kidney, a spontaneously coagulable material is effused into the tubes, and coagulates there, forming a *cast or mould of the tube*, which is gradually washed out by the fluid which is secreted behind it, and thus it finds its way into the urine, from which it may be easily separated for examination.

A cast is composed of a coagulable material which is effused into the uriniferous tube; and, becoming solid there, it entangles in its meshes any structures which may be in the tube at the time, and forms a mould of the uriniferous tube. The characters vary very much in different cases, according

to the state of the tubes and the part in which the effusion of the matter takes place. Various substances are often entangled in the cast; and, by observing the character of these, we are often enabled to ascertain the nature of morbid changes going on in tubes at the time the cast was being formed. Great difference of opinion has been expressed with reference to the nature of the material of which the cast is composed. By some it has been termed fibrine; but the striated appearance always present in coagula of this substance is not found in the cast. Others have considered the cast was composed of albumen; but it is not rendered opaque by means of those reagents which produce precipitates in albuminous solutions. Not more than five years since, it was stated by two observers in France and Germany of high reputation, at least in other branches of scientific inquiry, that the cast really consisted of the basement membrane of the uriniferous tube. How such a statement could be made by any one possessing even a slight knowledge of the anatomy of tissues, it is difficult to conceive.

The transparent material probably consists of a peculiar modification of an albuminous matter possessing somewhat the same character as the walls of some epithelial cells, the elastic laminæ of the cornea, the walls of hydatid cysts, &c. I think it not improbable that these casts of the uriniferous tubes may really be composed of the material which, in health, becomes converted into epithelial cells. In disease, this substance, perhaps somewhat altered, passes into the uriniferous tubes, and coagulates there. If this be so, the formation of the cast must result from depraved nutrition, and the material must pass into the tube from the intertubular capillaries—an idea which receives some support from the fact that occasionally casts are formed although no albumen passes into the urine. According to this notion, it is possible that a cast might be formed quite independently of any congestion or morbid condition of the Malpighian tuft; but, as a general rule, there can be no doubt that serum escapes as well as the coagulable matter, and albumen is found in the urine.

The diameter and general character of the cast will be determined by the state of the uriniferous tube at the time of its formation, as the researches of Dr. Johnson have indisputably proved. If the epithelium be abnormally adherent, the cast will be very narrow; if, on the other hand, the epithelium be removed, it will be of the width of the tube. Should the epithelium be disintegrating, the cast will afford evidence of the change. If in a state of fatty degeneration, fat-cells will be entangled in it. In hemorrhage from any part of the secreting structure, blood-corpuscles are present; and when suppuration occurs, the cast contains pus-corpuscles. When the transudation of the coagulable material occurs in a tube to which the epithelium is intimately adherent, or in a tube whose walls are smooth, the cast will be clear and perfectly transparent. The import of all these different characters is fully discussed in the works of Dr. Johnson; and several interesting cases, under observation for a considerable period of time, will be found reported in Dr. Basham's work.—*British Med. Journal, July* 21, 1860, *p.* 556.

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## 47.—ON MORBID CHANGES AFFECTING THE STRUCTURE OF THE KIDNEY.

By Dr. Lionel Beale, F.R.S., Physician to King's College Hospital.

In cases where the blood which passes through the kidney is unhealthy, and especially when it is loaded with noxious matter, as takes place in the majority of cases of long-continued beer or spirit drinking, the secreting power of the renal cells is gradually impaired. They lose their healthy appearance, and become much shrivelled, and at last suffer disintegration; while, in consequence of the germs having been destroyed at an early period of growth, their place is not occupied by a new generation. A complicated series of morbid changes in other structures of the kidney gradually ensues; and, in consequence of the blood being rendered still more depraved by the

accumulation in it of matters which ought to be removed by the kidney, other organs are gradually involved.

In the kidney, it will be observed that the coats of the smaller arteries become much thickened, the capillaries shrink, while their walls become thicker and often granular. The quantity of blood distributed to the organ diminishes; and many of the capillaries, being no longer required, shrink and cease to transmit blood. The diameter of the secreting tubes decreases, while the basement membrane is thickened and more impervious. The whole organ becomes hard, and at the same time small and shrunken. This decrease in size takes place principally at the expense of the cortical or secreting portion of the kidney, as would be supposed. Such serious changes in the structure of an organ so important as the kidney necessarily affect the system generally, but these we must not now stop to consider. The Malpighian bodies also waste. The remains of many may be seen without a capillary in them being pervious; and not a few of those which still exist are found to be so altered that they can hardly be recognized as Malpighian bodies at all. The greater part of the blood sent to the kidney passes into the pyramids by the *vasa recta*, and soon re-enters the veins, a small quantity being distributed to those tubes and Malpighian bodies nearest the pyramids. The diminished amount of urea, &c., present in the urine, is probably separated in this latter situation; while a certain quantity of water, with a little albumen and the material of which the casts are formed, also escape in this situation, as well as from the straight part of the tubes.

Long before the disease has arrived at this stage, the urine will be found to contain a very small amount of solid matter, which consists principally of salts and extractives, with a very little urea.

By many pathologists these changes are explained by the effusion of inflammatory lymph, and subsequent thickening, condensation and contraction of the so-called *matrix;* but it seems to me that all the appearances observed may be much more simply accounted for on the view that they depend upon depraved nutrition and wasting, than by resorting to the hypothesis of the inflammation of a structure whose existence has not been satisfactorily demonstrated, and which, if it does exist, according to its warmest advocates, only serves as a supporting tissue to the more essential elements of the gland-structure. It is very hard to see why such a tissue, which takes no active part in the changes going on in the gland, should be the starting-point of all the serious morbid alterations which occur. The idea, I believe, has arisen from a supposed analogy between cirrhosis of the liver and the so-called chronic inflammatory disease of the kidney. Cirrhosis was considered to depend upon inflammation, thickening, and subsequent contraction of another supporting fibrous tissue (Glisson's capsule), which was supposed to surround the lobules of the liver, and by its contraction to press upon the vessels ("On Cirrhosis of the Liver," 'Archives of Medicine, vol. i., p. 118). For the origin of these morbid changes, we must look to the altered actions going on in the secreting structure, and not to inflammation of tissues of doubtful existence, which take no part in the nutritive operations or gland-functions. The conclusions to which I have arrived from my own observations, with reference to the nature of the so-called matrix in the healthy kidney, and the changes taking place in disease, are at variance with those usually entertained both in this country and on the continent. The discussion of this question involves the whole subject of areolar tissue and its corpuscles, and I therefore propose to defer it until this has been fully entered into in another place. For an admirable statement of the opinions generally held, with many original observations, I must refer you to a work by Arnold Beer, just published in Berlin ('Die Binde-Substanz der Menschlichen Niere'). The drawings accompanying this work appear to me rather rough. The engraver perhaps has misinterpreted some of the author's representations.

In other cases, the epithelium undergoes a very peculiar change, to which much attention has been given of late years. Fatty matter accumulates in the cells of the uriniferous tubes. The intertubular capillaries and those of

the Malpighian bodies are also affected in a similar manner, and little collections of minute oil-globules may often be seen at intervals in their walls. This change often commences in a few of the tubes, and gradually extends until the whole organ is affected. The kidney is in many instances much enlarged, while its colour has become very pale. Fatty degeneration, in many instances, is not confined to a single tissue or organ, but almost every part of the body is more or less involved.

*Bright's Disease.*—This term has been applied to all *morbid conditions of the kidney associated with albuminous urine.* Of late years many important characters have been made out, by which we are enabled to distinguish several diseases of the kidney essentially different from each other—different in their origin, in their progress, and often in the results to which they lead. Dr. Johnson has accurately described several of these morbid changes, and his researches have been confirmed by other pathologists. However, some physicians still insist that the different conditions above alluded to are merely different stages of one and the same morbid process. Let me ask you to examine carefully the small contracted kidney so commonly found in the bodies of old drunkards, with its rough puckered surface and diminished cortical portion, and contrast it with the large, smooth, and pale kidney, in a state of fatty degeneration, which is not unfrequently met with in young people not more than twenty years of age. The causes of these diseases are different; the conditions under which they occur are different; and, although the result is fatal in both, death occurs in a very different way. Their chemical characters are different; their microscopical characters indicate the occurrence of changes which are totally distinct. Again, the treatment required in the early stages of these diseases, when alone any benefit is likely to be derived from treatment, is different.

The divisions and nomenclature adopted by Dr. Johnson are the following: *Acute desquamative nephritis; Chronic desquamative nephritis; Waxy degeneration of the kidney; Non-desquamative disease of the kidney; Fatty degeneration of the kidney; Suppurative nephritis.* Dr. Johnson still supports the same classification, and opposes the theory held by some pathologists with reference to the *oneness* of Bright's disease. He has recently written a paper on this subject, which will be found in vol. xlii. of the 'Medico-Chirurgical Transactions.' Dr. Johnson says, with regard to the oft debated question, if large kidneys, at a subsequent stage of the morbid changes, contract, "The *rule* is that a large Bright's kidney remains large to the end and does not become a small one; and on the other hand, a contracted Bright's kidney does not pass through a previous stage of enlargement.—*British Med. Journal, July* 28, 1860, *p.* 576.

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## 48.—ON THE TREATMENT OF ACUTE AND CHRONIC ALBUMINURIA.

By Dr. George Corfe.

During the last outbreak of cholera in London—namely, in 1854—many of its victims who recovered from the direful scourge were left the subjects of more or less permanent uræmia. Some of these cases proved fatal, and the mode of death differed but little from uræmic poisoning in the ordinary forms of morbus Brightii or albuminuria. The progressive and fatal steps of the disease were, insidious coma, total and permanent suppression of urine, a small pupil and a hazy cornea, a peculiar flabbiness on handling the fleshy parts of the body, and well-marked fumes of muriate of ammonia were formed, as a mirror, moistened with hydrochloric acid, was made to receive the exhaled breath of the patient. This latter pathological feature is induced by the non-eliminated urea becoming converted in its elements to ammonia when it arrives at the pulmonary cells for exhalation, and is oftentimes a most valuable indication of the existence of uræmic poisoning. The frequent occurrence of albuminuria in hospital practice since 1854 has induced me to

keep a record of many of the most interesting forms of this disease; and in watching the progress of the affection, I have been led to group certain distinct phenomena, which will enable us to form an accurate basis, and serve as a diagnostic mark between the acute and chronic stages of this intractable malady. Such a desideratum appears the more necessary when we reflect how different the treatment should necessarily be in each form, and that by a judicious management of the acute, the direful ravages of a chronic stage are thereby obviated.

It may be stated, then, as a pretty general rule, that where the urine is scanty, the specific gravity variable from 1010 to 1020, the morning secretion (urina sanguinis) lighter than that of the afternoon (urina chyli), the albumen copious both by nitric acid and by heat, the tube casts abundant, the urine smoky and frothy, with or without oil-corpuscles, though the amount of dropsy be small, yet the disease is, under these circumstances, in its early and acute form.

On the other hand, a chronic stage may be readily diagnosed by the following physical signs:—The countenance has already become more or less cachectic; there is frequent nocturnal micturition; the albumen is scanty, perhaps altogether absent; the specific gravity rarely exceeds 1010, and is persistent in weight; its acidity is so faint that it can scarcely be exhibited, and there is a strong tendency in it to pass into an alkalescent state under the smallest doses of alkaline remedies; the colour is of the palest straw. The skin assumes a faint but distinct shining appearance, as though a thin layer of collodion or gum had been painted over its surface. There are frequent attacks of frontal headache; tinnitus aurium, lumbago, and general prostration of the system are also sequences of the acute form of the disease. It is by no means a rule that dropsy should supervene in such a change of structure as that which the kidneys undergo in the chronic form of albuminuria; indeed, this symptom is more frequently absent, but usually presen in the majority of acute cases of the disease.

The pathological appearances of the two forms, the tendency to inflammation of the serous membranes, the frequent fatal attacks of renal epilepsy, other cerebral disturbances from uræmic poisoning, have been pointed out by others, so that I need not dwell upon these phases of the disease. My special object is to give to the profession some of the results of a successful treatment of the acute and of the chronic form. Amongst several hundred cases which have passed under my notice in hospital practice, I cannot find from the notes on them that there was any connexion between this disease and scrofula. Dr. Bright considers the occurrence of tuberculosis with albuminuria to be "decidedly rare;" and on this account, perhaps, one may prefer the more frequent adoption of an antiphlogistic and powerful drastic treatment in the early stages, which would be inadmissible if experience and pathology led us to suspect a complication of tuberculosis with the disease.

In treating the acute stage, my experience justifies me in urging strongly the adoption of one of the two following lines of practice:—If the patient has had no previous attack, and has lived temperately, and was otherwise in good condition before the invasion of the disease, then I would recommend a small bleeding, so as to affect the pulse under the finger; and, after two hours, the exhibition of half an ounce of fresh-made infusion of digitalis every hour for three doses. Both the bleeding and the diuretic should be administered early in the day. After a pause of twenty-four or thirty-six hours, according to the condition of the vascular system, the same dose should be repeated, and even to a third and fourth repetition, followed on each occasion by copious draughts of "nitre-drink," the form for making which is simply to disolve a drachm of the nitrate of potash in a quart of thin barley-water, and a little "foots" sugar may be added to remove the mawkish taste. I have witnessed the most extraordinary results from this line of treatment. If the disease does not yield—that is to say, if the invasion of dropsy increases, the albumen is yet copious, the specific gravity high, the tube casts numerous, and the urine scanty, instead of resorting to that mischievous

and unscientific mode of attacking the enemy by stimulating congested kidneys, already semi-inflamed, with saline diuretics (the evils of which practice by-the-by I have too frequently seen displayed in a few days in the pathological theatre,) then a most valuable agent is to be found in the determined exhibition of elaterium, accompanied with daily dry-cupping to the loins, and a sudatorium at night. The form for exhibiting this drastic is the following:—Fresh extract of elaterium, half a grain; powdered capsicum, two grains; gamboge, three grains; soft extract of jalap, four grains; make two pills. One of these pills should be taken at six o'clock in the morning; the second at eight; and if no powerful drastic effect follows by nine a. m., a third dose may be given. The intense vomiting and purgation which ensue, it must be acknowledged, are very trying to the patient; but he is amply rewarded for his suffering by a subsequent relief to all his distress, so that he has oftentimes begged for a repetition of the pills before it was thought advisable to put him to a second trial, even when such an heroic treatment has been met by heroic suffering. I am convinced that the value of this drug is only appreciated by a few physicians; and even where it has been employed, the exhaustion consequent upon its action has deterred many practitioners from persevering in its use. But it ought to be borne, that under such treatment the patient should be allowed a liberal quantity of animal food in the form chiefly of strong broths, with chops and steak together with a pint, at least, of stout or good ale, and vegetables, &c., daily.

I remember one instance amongst many which Dr. Watson was in the habit of mentioning to his class when speaking of the use of this drug in albuminuria. He was summoned to attend in consultation a case of the above disease. "When I arrived there," he would observe, "I found the man overwhelmed with anasarcous swelling, his dyspnœa was most urgent, and there appeared to be no prospect of relief. However, I ascertained from the practitioner that he had tried everything but elaterium. I urged its use, and prescribed some to be given immediately. Four or five years after this occurrence I was again in consultation with the same practitioner, when he asked me if I recollected the case of albuminuria, and what I gave him, adding that it made a complete cure of the man. I referred to my private notes, feeling assured that the chief drug was elaterium, and so it proved, and this was the form I administered it in:—One grain of extract of elaterium, dissolved in an ounce of spirit of nitric ether; a small spoonful to be taken every hour until the bowels have been well opened."

I have frequently used the same form with excellent benefit in children affected with the disease after scarlatina. As soon as the system has been thoroughly brought under the influence of the above remedy, immediate recourse should be had to another equally valuable agent—namely, the tincture of the sesquichloride of iron in half-drachm doses, and the most elegant and efficacious mode of exhibiting it is in a glassful of seltzer water. Allusion has already been made to the injudicious practice of giving stimulating saline diuretics in this stage of the disease, and it has repeatedly happened that hæmatine and red corpuscles have been voided, and finally suppression of urine has followed the use of such drugs, whereas a nearly and a discreet use of the remedies alluded to has been attended by a complete alleviation of the worst form of the disease. As the attack becomes more chronic in its nature, I have seen marked benefit follow the use of the syrup of iodide of iron in half-drachm and drachm doses, together with a generous animal diet, and a moderate use of good beer or porter, to the exclusion of all spirits. Drachm doses of cod-liver oil after each meal has proved a valuable adjuvant in the treatment of many severe cases.—*Lancet, May* 19, 1860, *p.* 489.

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## 49.—ON THE TESTS FOR DIABETIC SUGAR.

By Dr. Lionel Beale, F. R. S., Physician to King's College Hospital.

The presence of grape sugar in urine is readily ascertained by the appli-

cation of certain tests, and if moderate care be taken in the examination, the detection of this substance is not open to many fallacies.

*Moore's test* for grape-sugar consists in adding, to the urine suspected to contain it, about half its bulk of liquor potassæ. If sugar be present, the mixture becomes of a rich brown colour upon boiling, which increases in intensity if the boiling be prolonged. The brown colour of the solution is owing to the formation of mellassic or sacchulmic acid; glucic acid is also produced in the decomposition. This test, however, cannot be depended upon for detecting the presence of small quantities of sugar, because there are some other substances besides sugar which will cause the development of the colour in a slight degree.

*Trommer's test.*—Of all the tests which have yet been proposed, that originally suggested by Trommer, or some slight modification of it, will be found of the greatest practical value for showing the presence of sugar in diabetic urine, in clinical investigations. Trommer's test is applied as follows:—A small quantity of the urine, placed in a test tube, is to be treated with solution of potash, and a drop or two of a solution of sulphate of copper is to be dropped into the mixture. If sugar be present in any quantity, the precipitate at first formed will be redissolved, and the solution will be of a *dark blue* colour. If only traces of sugar are suspected to be present, one drop of the sulphate of copper solution will be sufficient. The dark blue solution is now to be heated to the boiling point, and if sugar be present, a pale *reddish brown precipitate of suboxide of copper* is immediately thrown down. Instead of boiling the mixture, it may be allowed to stand for some time, when a similar deposit will gradually subside. If the suboxide is only reduced after prolonged boiling, this cannot be taken as good evidence of the presence of sugar, for under these circumstances there are some other substances which will cause the reduction of the oxide of copper. Again, if the solution simply *change colour* by boiling, *without the occurrence of a distinct precipitate*, we must not infer that the change is necessarily due to the presence of sugar, for almost all specimens of urine exhibit this change. A flocculent precipitate of earthy phosphate, which always takes place, cannot be mistaken for the suboxide, as it is quite colourless, or of a pale greenish tinge. The reaction alone characteristic is the production of a brown or yellowish precipitate (varying in quantity according to the amount of sugar the urine contains), either after the mixture has stood for some time, or upon boiling it not longer than for a minute.

*Modifications of Trommer's test* have been proposed by Barreswil, and others, the most applicable, however, according to Lehmann, being that of Fehling (Lehman's 'Physiological Chemistry,' by Day, vol. i., p. 288. Cavendish Society). As this test is more easily applied than the sulphate of copper and potash, it may be well to mention its composition,—69 grains of sulphate of copper are to be dissolved in 345 grains of distilled water; to this solution a concentrated solution of 268 grains of tartrate of potash, and then a solution composed of 80 grains of carbonate of soda in an ounce of distilled water are to be added; the mixture may be poured into a 1000 grain measure and filled up with water.

Barreswil's solution is composed of the following constituents:—

| | |
|---|---|
| Cream of Tartar . . . | 96 grains |
| Crystalised carbonate of soda . . | 96 ,, |
| Sulphate of copper . . . | 32 ,, |
| Caustic potash . . . | 64 ,, |
| Water . . . . | 2 fluid ounces. |

In using these tests, it is only necessary to add about an equal bulk to the urine in a test tube, and then to boil the mixture. If sugar be present, the precipitate of suboxide occurs immediately. The application of this solution to the quantitative determination of sugar, will be considered under *volumetric analysis*. Trommer's, or one of the above mentioned modifications, will be found the most delicate test which can be used, when only small quantities of sugar are suspected to be present, and the tartrate of

copper solution is applied as easily as the liquor potassæ test, while the results obtained from it are far more to be depended upon. If albumen be present, the reduction of the oxide of copper does not take place, so that in using the copper test we must ascertain that this substance is absent. Ammonia also dissolves suboxide of copper.

It has been shown that allantoin, creatine, and creatinine, have the power of producing a precipitate of suboxide of copper, like grape-sugar, and more recently, M. Berlin has proved that uric acid possesses to some extent the same property.

Some time since I endeavoured to ascertain the cause of certain anomalous results, which were sometimes met with in employing this test; and as these to some extent explain the discrepancies of different authorities with reference to the presence or absence of sugar in the urine in certain cases, it is well to allude to them here. The following results were obtained.

1. The precipitate of suboxide of copper was readily dissolved by acetic, hydrochloric, and nitric acids. It was also dissolved by ammonia.

2. The precipitate was insoluble in a solution of chloride of sodium, but was readily dissolved by a weak solution of chloride of ammonium.

3. The addition of a few drops of chloride of ammonium previous to boiling, entirely prevented the precipitation of the suboxide, the mixture retaining its greenish colour. Upon adding some solution of potash, however, the precipitate of suboxide was produced, and ammoniacal fumes were given off at the same time. If a moderate quantity of solution of chloride of ammonium was present, the precipitate did not occur upon the addition of potash, even after very prolonged boiling.

4. If a drop of a very dilute solution of the chloride of ammonium was added to a pretty strong solution of sugar, and, after the addition of the tartrate, the mixture was boiled, no precipitate took place, but the solution became of a pale brown tint; the suboxide being immediately thrown down upon the addition of a few drops of a solution of potash, with the development of ammoniacal fumes. In the above cases in which no precipitate took place, it was ascertained that there was the usual excess of alkali present in the test solution.

5. A solution of oxalate of ammonia also prevented the precipitation of the suboxide, but a greater quantity of this salt than of the chloride of ammonia was required.

6. A neutral solution of urate of ammonia (artificially prepared) also prevented the reduction of the suboxide, and dissolved the precipitate if added to it. On carrying out this experiment further, it was found that the *precipitate of suboxide of copper was dissolved by urine containing an excess of urate of ammonia.*

7. A solution of grape-sugar in water was prepared, and by a preliminary experiment it was ascertained that, upon being boiled with the tartrate test, an abundant precipitation of suboxide took place.

To a portion of the precipitate of suboxide produced in this way, about a drachm of healthy urine, immediately after it was passed, and while yet warm, was added, and the reddish precipitate was instantly dissolved, forming a perfectly clear solution. Upon further boiling, a slight precipitate of phosphate took place. The suboxide, however, could not be precipitated by the further addition of potash and prolonged boiling.

8. Upon mixing a small quantity of grape-sugar with the same specimen of healthy urine, and boiling the mixture with the tartrate test, *no precipitate*, except a little phosphate, was produced. About half an ounce of the same mixture of urine and grape-sugar was placed in a test tube, mixed with six drops of yeast, and inverted over mercury. The whole was then placed in a temperature from 70° to 100° for about twelve hours, at the end of which time the tube was found quite filled with gas, and all the liquid was expelled into the vessel in which it had been placed. The specimen of urine with which the above experiments were tried, was allowed to stand in a still place; and when it had become quite cold, an abundant precipitate of urate of ammonia was found to be present.

9. A portion of the aqueous solution of grape-sugar was mixed with a strong solution of urate of ammonia (artificially prepared), and then a certain quantity of the tartrate solution was added, and the mixture boiled. The characteristic precipitate, or opalescence, was not produced, but the mixture became of a pale fawn colour. In a weak solution of urate of ammonia, the characteristic precipitate appeared after boiling the mixture for some minutes. *So that, although much sugar is present, the colour of the mixture may be merely changed to brown, and no precipitate whatever may take place.*

10. A solution of grape-sugar was treated with a drop of a dilute solution of chloride of ammonium, and boiled with the tartrate solution. The mixture became of a brown colour, but no precipitate occurred. Upon the addition of a few drops of solution of potash, the precipitate of suboxide was produced.

A solution of grape-sugar, treated with Trommer's test, according to the usual method, behaved in the same way, in the presence of chloride of ammonium, as when treated with the tartrate of copper solution; but in this case a greater quantity of the chloride was necessary, for when only traces were present, ammoniacal vapours were given off, and the precipitate of suboxide subsided, as before remarked.

From the results of the above experiments, the following conclusions with reference to the practical application of Trommer's test, and Fehling and Barreswil's solutions, and other modifications of the copper test, may be drawn:—

1. That if the urine contain chloride of ammonium (even in very small quantity), urate of ammonia, or other ammoniacal salts, the suboxide of copper would not be precipitatated if only a small quantity of sugar were present.

2. That unless there be a considerable quantity of one of the above salts present (in which case the blue colour will remain). the mixture will change to a brownish hue upon boiling, but no opalescence or *precipitate* of suboxide of copper will occur. When only a moderate amount of sugar is present, I have been unable to obtain a precipitate, under these circumstances, by the addition of potash to the solution, and prolonged boiling. By observation 8, it appears that a specimen of urine exhibiting this reaction may contain a large quantity of sugar, as ascertained by the yeast test.

3. That in many cases in which the precipitation of the suboxide is prevented by the presence of ammoniacal salts, the addition of potash to the solution, and subsequent boiling, will cause the production of a precipitate with the evolution of ammoniacal fumes. Hence care should always be taken that there is a considerable excess of free alkali present.

4. When only small quantities of sugar are present in the urine, and the precipitate of suboxide of copper is not decided, the fermentation test should be resorted to.

Upon treating different specimens of diabetic urine with Trommer's test, or its modifications, it has often been noticed that in one case the precipitate is produced as soon as the mixture reaches the boiling point, or even before; while in other instances it is necessary to keep it in active ebullition for some minutes, before any precipitate is produced. This circumstance receives explanation from the facts above detailed with reference to the presence of ammoniacal salts; and other anomalous results, which must have occurred to many in the habit of employing this test, become explained.

As a test for diabetic or grape-sugar, if proper precautions be observed, much greater dependence can be placed on this test than by simply boiling with liquor potassæ. Specimens of urine in which sugar is suspected to be present, and no decided precipitate of suboxide (which must be carefully distinguished from phosphate) occurs, should be carefully fermented with yeast before any conclusion is arrived at.

*On Testing for Sugar when only Traces are Present.*—In fluids which are suspected to contain only mere traces of sugar, it is necessary to separate

some of the other constituents before applying the test. The plan recommended by M. Leconte is the following:—Excess of acetate of lead is added, and the precipitate separated by filtration. The solution is concentrated by evaporation, treated with ammonia, and again filtered. The copper test is then applied. The objections to applying the reduction test to solutions containing ammonia has been already discussed. It is better to employ carbonate of potash, or soda, instead of ammonia. The excess of lead salt may also be removed from the filtered solution by passing sulphuretted hydrogen. The precipitate of sulphuret of lead is removed by filtration, and the liquid, after evaporation to a small bulk, may be tested.

Another plan recommended by M. Leconte is to treat the urine with acetic acid, and evaporate it to about the fifth of its bulk: it is then treated with alcohol, and after filtration from the salts, &c., the alcoholic solution is evaporated and tested. This plan is free from the objection that ammonia may cause the destruction of the sugar where only traces are present.

*The Yeast Test.*—This is one of the most satisfactory tests for the presence of sugar, and if employed with proper care can hardly fail in its results. Two test tubes, of the same form, and of equal size, are to be taken. One is nearly filled with water, and into the other a corresponding quantity of the urine is to be poured. An equal amount of yeast is now to be added to the liquids in the tubes, and after pouring in just sufficient fluid to fill the tubes, the thumb is to be carefully placed over the opening, and the tube inverted in a small cup of mercury. The best plan, however, is to cut out a little india-rubber pad, slightly larger than the upper extremity of the tube. When the tubes have been filled up to the brim with a little water, the pad is allowed to float on the surface, next a little cup or beaker is inverted, and carefully placed over the end of the tube. The india-rubber being pressed against the open end, the fluid is prevented from escaping. The whole may be inverted, and a little mercury having been poured into the beaker, the india-rubber may be removed, with forceps, without any escape of the fluid. The tubes may be supported in position by a wire stand. Both tubes are then to be exposed, for a few hours, to a temperature of from 80° to 90°, and the comparative size of the bubble of gas in the upper part of each may then be noted. If an appreciable quantity of sugar be present, the bubble of gas in the tube containing the urine will be many times larger than in the tube which contains the yeast and water. In the latter tube the bubble of gas merely arises from the small quantities of air previously mixed with the yeast, becoming disengaged, and floating to the surface. Fermentation, when carefully performed, is positive evidence of the presence of sugar, although it does not indicate the kind of sugar present.

*Maumené's Test.*—A little woolen rag, as merino, is cut into strips, and soaked for four or five minutes in a solution of perchloride of tin (one part of the perchloride to two parts of water.) The slips are then dried over the water bath. A drop of the urine suspected to contain sugar is allowed to fall on a small slip of the prepared merino, which is then dried, and exposed to the dull red heat of a spirit lamp. If a trace of sugar be present, a black spot is produced.

*Bismuth Test.*—Bottger has lately proposed a new test for sugar. This consists in adding first of all potash, then a small quantity of subnitrate of bismuth; lastly the mixture is boiled. If sugar be present, the oxide is reduced to metallic bismuth, which is precipitated in the form of a black powder. It has been asserted that sulphuret of bismuth is formed, but this seems not to be the case. Brücke shows that this test is more delicate than Trommer's (or the modification of it by Fehling); and he finds that the black precipitate is produced to some extent in specimens of healthy urine. The bismuth test may also be applied thus—A solution of carbonate of soda (crystallised carbonate 1 part, water 3 parts) is prepared, and a certain quantity added to an equal amount of the urine. A little basic nitrate of bismuth is then added, and the mixture heated to the boiling point. If sugar be present, a black precipitate is produced.—*British Med. Journal, May* 19, 1860, *p.* 374.

## 50.—ON THE METHOD OF DETECTING AND ESTIMATING THE QUANTITY OF SUGAR IN DIABETIC URINE.

By Dr. JAMES M'GHIE, Glasgow.

A considerable variety of methods have been given by different writers on urinary diseases, for testing diabetic urine, and estimating the quantity of sugar contained in any given specimen. The sense of taste is perhaps the readiest means of ascertaining that saccharine matter is present in urine, and evaporation gives an approximate estimation of the quantity. Trommer's test is now most commonly employed, and the improvements made upon it by Barreswil and Fehling, enable us to employ it not merely for the qualitative, but also for the quantitative determination of sugar in the urine. The fermentation test, also, is one fitted to give satisfactory results.

Having been engaged in 1849 in the examination of diabetic urine, I had frequently tried all the tests usually employed, and among others that of Pittenkofer, with bile and sulphuric acid. At that time there was no notice of Runge's test in Dr. Golding Bird's work, though it appeared in a subsequent edition. Runge only contemplated the production of a blackish colour by applying dilute sulphuric acid to diabetic urine: while Pittenkofer employed it along with bile to produce a violet colour. Neither of these methods afford satisfactory results. "Grape sugar," says Thudichum, "is not influenced by dilute acids, and not blackened by concentrated sulphuric acid. If grape sugar dried at a temperature of 100°, is mixed with one and a half times its weight of concentrated sulphuric acid, it dissolves, and forms with the acid a combination which is an acid itself, and has been called sulphosaccharic acid." However just this observation may be in reference to dried grape sugar, it does not apply to that form of sugar found in diabetic urine. When concentrated sulphuric acid is added to an equal volume of diabetic urine, and heat applied to ebullition of the mixture, the following changes occur: First, the fluid becomes blackish, and soon passes into a state of inky blackness. As soon as ebullition commences the mixture begins to froth up, and continues to be covered with a layer of froth so long as the boiling continues. This layer of froth also remains after the mixture is cooled. In no other kind of urine except the diabetic, are these appearances manifested. After the boiling has been continued for a short time, besides the intense black colour, when the fluid is closely examined there will be found a copious evolution of carbonaceous particles. The high specific gravity of the fluid in which these fine black particles are suspended, prevents their being deposited as a precipitate till it is largely diluted with water. In ordinary samples of diabetic urine a copious precipitate is speedily deposited on the addition of water. The precipitate should now be thoroughly washed on a weighed filter, till all the acid is removed, and nothing but the pure carbon remains on the filter. After being carefully dried and weighed, the excess of weight of the filter containing the carbon over the weight of the filter without the carbon, will give the amount of carbon contained in the sugar; and when this quantity is found it is easy to calculate the weight of the sugar. The dried precipitate on the filter will be found in the form of numerous small pieces of jet-black carbon, which may be collected, and, by combustion, will be dissipated in the form of gas, without leaving any solid residue.

It may be objected to this test, that there are other kinds of urine which, when boiled with sulphuric acid, develop appearances somewhat analogous, if not altogether identical with those produced by diabetic urine. It is true that albuminous urine, when boiled with strong sulphuric acid, gives a deep rich brown colour, approaching to black; but it presents none of the peculiar phenomena which we have noticed as accompanying diabetic urine when treated in a similar manner. The colour is different; it does not froth; and when diluted with water, there is no deposit of black carbonaceous matter, but the deep brown colour merely become less deep in shade.

Any probable fallacy as between albuminous and diabetic urine, will, if the following method is adopted, be completely dissipated. Let the urine be first acidulated with $NO_5$, and then boiled, when, if albumen is present, it will become coagulated, and may be removed by filtration, then proceed with the test as described above.

I have seen this test fail to develop the characteristic phenomena when applied after the urine has been kept a sufficient length of time to admit of decomposition taking place. A sample of urine which has already been found by Trommer's or some other test, to contain sugar, may, after it has stood some time, be tested by sulphuric acid, and will probably fail to produce any of the characteristic reactions. It will be found in all such cases that decomposition has begun, and not only this, but every other test will fail. The precaution ought to be taken to have the urine operated upon as fresh and recent as possible, when embarrassing failures of this kind will be avoided.

The *modus operandi* of the sulphuric acid in its action on grape sugar is extremely simple and well defined. The sugar consists of $C_{12}, O_{12}, H_{12}$; that is, twelve atoms of carbon are combined with twelve atoms of HO or water. Strong sulphuric acid has sufficiently strong affinity for water to decompose the sugar, and remove from it all the elements of HO. and leave the insoluble carbon disseminated in fine particles through the mixture; and whatever action it may have upon the other constituents of the urine, it does not give rise to any other insoluble compound, if the necessary precautions have been taken at the outset, to remove the mucus by filtration, and the albumen, if there is any present, in the manner indicated above. In some specimens of high-coloured and concentrated urine, when there is a considerable quantity of animal extractive and colouring matter, the action of the sulphuric acid may develop a small amount of blackish particles, which, when treated in the same way as the carbon of diabetic sugar, are readily distinguished from it. It may be affirmed, without fear of contradiction, that there is no other kind of urine which comports itself in the presence of sulphuric acid in a manner analogous to diabetic urine. Grape sugar in the solid state is dissolved in $SO_3$, but in solution in the urine is decomposed by it. When treated with caustic potash and heat, the smell of burnt sugar is evolved, the solution becomes brown in colour, and glucic and molassic acids are produced. In the crystallized state it contains, besides the constitutional elements of water, two equivalents of water of crystallization, which are dispelled by a temperature of 212°. When the temperature is raised to 284°, three equivalents of constitutional water are dispelled, and caramel is produced; but it is only when in a state of solution that sulphuric acid removes all the constitutional elements of HO. Of the ordinary tests Prommer's is preferred by some, Cappezuoli's and the fermentation test by others. The sulphuric-acid test is one which, if deemed worthy of a trial, will, in point of simplicity in manipulation and certainty in its results, be found second to none.—*Glasgow Med. Journal, April* 1860, *p.* 42.

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## 51.—ON THE DETERMINATION OF THE PROPORTION OF SOLIDS IN THE URINE OF HEALTH AND DISEASE.

By Dr. William Seller, F.R.S.E., Edinburgh.

Of late, my attention has been drawn to the modes of estimating the proportion of solid matter in the urine. There are still attendant on the reduction of the urine to dryness, great practical difficulties; and, respecting the proper rule for solving the problem by reference to the density of the urine, there are conflicting views among the best authorities.

I propose to offer a few observations bearing on each of these two methods; and first, of that which rests on the density.

It will be remembered that the three best-known proposals for finding the *proportion* of solids in the urine from the density are, to multiply the num-

ber denoting the excess of the density above 1000, 1st, by the number 2; 2dly, by a number less than 2 (1.65); 3dly, by a number greater than 2 (2.33).

This mode of proceeding rests on the circumstance, that when soluble matter is added to water, the solvent commonly, yet not always, undergoes an expansion.

If there be no expansion, there is no need of a multiplier—the excess of the density above 1000 signifies the amount of the soluble matter present. If the expansion expressed in water-grain measures be found to be equal to the excess of the density above 1000, represented by weight in grains, then the multiplier 2 will give the amount of soluble matter contained in the solution. For example, in the 1000 grain specific-gravity bottle, let 50 grains of soluble matter, the mean expansion caused by which is 25 water-grain measures, be dissolved in 975 grains of distilled water, the bottle will be exactly filled, and the specific gravity, which here corresponds with the actual weight, will be 1025. But 25, the excess of the density or specific gravity above 1000, multiplied by 2, gives the whole amount of solid matter, viz., 50 grains. Parallel to this is every case in which the expansion is equal to the excess of density above 1000. And thus, that form of the rule under consideration which makes 2 the multiplier, proceeds on the assumption that the number denoting, in water-grain measures, the mean expansion caused in water by the solid matter of the urine is equal to half the amount of that matter expressed by weight in grains.

When, again, 1.65 is used as the multiplier, the method proceeds on the assumption that the expansion is to the excess of density above 1000, very nearly as 2 to 3, or is equal to two-fifths of the number denoting the weight of the soluble matter present.

Lastly, when the multiplier is made 2.33, the assumption exists that the expansion is to the excess of the density above 1000, nearly as 4 to 3, or is almost equal to four-sevenths of the number denoting the whole amount of solid matter present.

The whole of these statements is indisputable, provided it be admitted that a fixed weight of soluble matter of one kind creates the same amount of expansion during solution, whatever be the proportion of water. And though this proposition cannot be assumed as true without exception, yet it is certain that exceptions take place only to a small extent, chiefly when the point of saturation is approached.

It should be observed, as flowing from what has been affirmed above, that the number expressing the weight in grains of a salt present in solution, is made up of the excess of density above 1000, together with the number denoting the expansion in water-grain measures. Thus, when the specific-gravity bottle is filled with a solution weighing 1020 grains, the quantity of water present, if there has been any expansion at all, must be less than 1000 grains by the number of water-grain measures denoting the expansion; that is, the whole quantity, 1020 grains, is made up of the quantity of soluble matter, together with 1000 grains of water minus the number expressive of the expansion.

It should be remarked also, that, owing to the expansion being so uniformly constant for the same weight of a salt, whatever be the proportion of water, we reduce the specific gravity of a solution—for example, from 1050 to 1025—not by adding a volume of water equal to that which the solution is estimated to contain, but by adding a volume of water equal to that of the solution itself. Thus, 1000 grain-measures of urine of specific gravity 1030 may be found to contain no more than 970 grains of water; yet its specific gravity is reduced to 1015, not by adding other 970 grains of water, but by adding to it an equal volume, or 1000 grains, of water.

It should also be remarked, as flowing similarly from what has been affirmed above, that, for example, if four waters be poured upon a salt, and then four waters more, and so on in succession for a number of times, the solution obtained will not present such a uniformly decreasing specific gravity,—that such a scale of decrease is obtained only when the successive

quantities of water added are each equal to the first quantity, together with the expansion which took place while in the first instance the salt was taken up. Thus, if 250 grains of nitrate of potassa are dissolved in 1000 grains of water, the expansion amounts to nearly 90 water-grain measures; so that the quantities of water to be added in succession to produce such a uniformly decreasing scale of density are, not 1000 grains, but 1090 grains each.

So much, then, for the principles on which the necessity for a multiplier rests.

We have next to consider the practical application of these principles to the case of the urine.

Of the practical rules before referred to, that most in favour in this country at present is the one proposed by Dr. Christison, not for the urine in general, but for the special highly dense urine of diabetes—that, namely, by which 2·33 is made the multiplier. While many adopt this rule implicitly, like Dr. G. Bird, who has founded two extensive tables upon it, others qualify it by the rule wherein the number 2 is made the multiplier, which they recommend when the density of the urine is lower in the scale, while they take Dr. Christison's rule only when the specimen of urine under examination is of high density. The third rule, that in which 1·65 is made the multiplier, was proposed by the eminent French observer, Becquerel. This last rule, however, has, of late years, thrown entirely out of English books on the authority of Dr. Day, who, in an elaborate paper, has laid down Dr. Christison's rule as that most applicable to all kinds of cases.

The evidence on which each of these rules has been supported by their respective partisans, is the actual amount of solid matter obtained from numerous specimens of urine of known density. The objection to this evidence is the discrepancy in the results described by different observers,—which discrepancy is manifestly dependent on the difficulty of getting the solid residue perfectly free from water.

I am not aware that any one has hitherto attempted to try the merits of these several rules by the particular properties of the known constituents of the urine. This kind of inquiry, however, does not appear unworthy of attention. The plan of such an investigation I shall endeavour to suggest and follow out, as far as materials exist for the purpose.

The solid matter of the urine in health admits of a threefold division, namely, into—1, urea; 2, organic matters other than urea; and 3, inorganic saline substances. The proportions under these several heads, in many standard analyses of the urine in health, approach to an equality. Thus, in the analysis given by Miller in his *Chemistry*, the proportion of urea in 1000 parts of urine of specific gravity 1020, is 14·23; that of organic matters other than urea is 15·56; and that of inorganic saline substances is 13·35; while the proportion of water amounts to 956·80. Thus it may be allowed to us to assume, as a convenient diagram of the constitution of the urine, that the proportion in each of these divisions is equal, say 15 of each in the thousand parts. Such a diagram will stand thus;—Urea, 15; organic matters other than urea, 15; saline substances, 15; water, 1000, less the amount of expansion; while the specific gravity will hardly exceed 1020. It is obvious that, were the medium expansion created in water by such a proportion of each of those divisions ascertained, a considerable step would be gained towards determining which of the multipliers before referred to should in general be preferred.

It is only in the case of the saline substances commonly believed to be present in the urine, that any evidence has hitherto existed with regard to the amount of expansion produced during their solution. When a substance can be obtained in a state of purity, nothing is easier than, within moderate limits of accuracy, to determine its property in this respect; for no more is necessary than to put a certain quantity of the substance into the thousand-grain specific-gravity bottle, and to fill it up with distilled water, then weigh the whole. It is manifest that the difference between the entire weight and the weight of the soluble substance put in is equal to the quantity of water

present, and that the difference between the quantity of water and 1000 is the amount of expansion in water-grain measures. In this way I find that 30 grains of pure urea cause an expansion equal to 22 water-grain measures.

With respect to the organic substances other than urea, the small proportion of each, together with the difficulty of procuring most of them in a state of purity, has prevented me from making any direct observations as to their effect on the expansion of the solvent during solution. But it is evident that, in an exact analysis of the urine supplying the means of determining the effect of the urea and of the several inorganic saline substances on the expansion of the solvent, the means are furnished of inferring the expansion created by these organic substances in the aggregate.

Unfortunately, few recent analyses of the urine are of a kind to assist the mode of investigation here suggested; because it is not common, of late, to attempt to fix the exact proportions in which the several saline substances are present. The well-known analysis of Berzelius, now so time-honoured, is almost the only one I can find suitable to my present purpose. It is certain that no analysis has yet appeared fit to take the place which it has held so long. The only considerable defect in it is, that the specific gravity of the specimen of urine from which it was taken is not stated. It is less material that the organic substances other than urea and uric acid are not named in accordance with present views.

The proportion of solids to the water in this analysis is remarkably high, being 67 to 933 on 1000 parts by weight, or, in round numbers, 70 grains in 1000 water-grain measures of urine. If the specific gravity be sought by the converse of the rule so often referred to—that is, by dividing the quantity of solids present in 1000 water-grain measures by one of the three numbers, 1·65, 2, or 3·33—the answers will be respectively 1043, 1035, and 1030. The first number, 1043, is certainly considerably above any specific gravity which we are accustomed to meet with during health in this country. Nevertheless the case may be different in Sweden. As to 1030 and 1035, they are by no means uncommon as characterizing the urine of particular parts of the day, chiefly when the urine is very slowly secreted. The chief peculiarity, besides, in this analysis is the large proportion of urea. It has, however, been remarked, that the slow secretion of the urine with a small proportion of water is the circumstance most favourable to the elimination of an unusually large proportion of urea. And it is probably this same circumstance which has given rise to the prevailing idea, that it is the large proportion of urea which principally contributes to a high specific gravity of the urine. The truth, however, is, that when the urine has a high specific gravity, in health, there is an abundance of urea; but it is not the abundance of urea which chiefly contributes to that high density, for even 70 grains of urea, contained in 1000 grain measures of its simple solution, give rise to a density no higher than 1019. There is thus no reason to doubt that this analysis correctly represents a genuine specimen of healthy urine.

First, then, of the inorganic saline substances in this analysis. These salts are the sulphate of potassa, the sulphate of soda, the phosphate of soda, the phosphate of ammonia, the chloride of sodium, the hydrochlorate of ammonia, earthy matters with a trace of fluoride of calcium and siliceous earth. The two last particulars being omitted, the sum of these salts in the analysis is 17·41, and the sum of the expansion which they create in water during their solution, calculated from data obtained, for the most part, from authentic chemical sources, is 7·26 water-grain measures. Thus, a solution of these salts in 992·74 grains of water would yield 1000 water-grain measures of a fluid having a specific gravity of 1009·15. If this fluid were a true type of the urine, the two lower multipliers would give a more correct indication of the amount of solids present than the highest; for 9·15 multiplied into 2·33 give 21·31—an excess of nearly 4 grains above the quantity of salts present, viz., 17·41.

But urea, which in the analysis under consideration is present in large proportion, produces a very different effect on the urine from these salts. It was stated above, that 30 grains of urea create an expansion during solution

equal to 22 water-grain measures. Thirty grains of urea being the quantity stated in the analysis in 1000 grains of urine; there is more than 30 grains in 1000 water-grain measures of urine; and, therefore, the expansion due to it is correspondingly greater than 22 water-grain measures; but that small, difference may be left out of view. If, then, the fixed salts (17·45 grains) and the urea (30 grains), together 47·45 grains, are dissolved in water, the sum of their expansion is (22 × 7·26) 29·26; so that 970·74 grains of water will suffice to produce a solution 1000 water-grain measures in volume. The specific gravity of this solution is 1018·19; and even the highest multiplier, 2·33 fails to bring out so large a quantity as the solids present are equal to; for 2·33 × 18·19 are only equal to 42·38, the multiplier required being 2·6, since 18·19 × 2·6 = 47·29.

We have next to consider the effect of the organic matters other than urea. Here the uncertainty of the specific gravity creates a great difficulty. Thus, were we sure that the specific gravity is 1030, the aggregate of the expansion produced by the three divisions of the solids would be known, since it is the whole amount of the solids diminished by the excess of the density above 1000; or it is 70 — 30 = 40. Hence it would at once appear that the expansion created by 18·46 organic substances other than urea amounts to 10·74, or the difference between 40 and the expansion already seen to belong to the sum of the urea and the inorganic salts, namely, 29·26.

Since, however, this uncertainty as to the specific gravity of the specimen of urine under consideration prevents any trustworthy inference as to the effect of the organic matters of the urine other than urea in the expansion of the solvent, recourse may be had to some other analysis in quest of such information.

In Miller's analysis, the amount of fixed salts, 23·35 gives rise to an expansion of no more than one-third $\frac{23.35}{3}$, or 4·45, while 14·23 parts of urea create an expansion of 10·43,—the joint expansion, therefore, effected by the fixed salts and the urea is 14·88. The whole quantity of solid matter in this specimen of urine is 43·2, and the specific gravity is 1020. The total expansion is discovered by taking the excess of the density above 1000, namely 20, from the whole quantity of the solids, 43·2, when there remains 23·20. But this last number being diminished by that denoting the joint expansion produced by the fixed salts and the urea, namely 14·88, there remain 8·32 as indicating the expansion attributable to the organic substances other than urea, which amount to 15·56. But 15·56 : 8·32 :: 18·46 : 9·86. Thus there is deduced from Miller's analysis a number, 9·86, expressive of the expansion due to the 18·46 of organic substances other than urea contained in the analysis of Berzelius,—a number differing in a very slight degree from that, namely 10·74, inferred from the assumption of 1030 as the specific gravity of that specimen of urine.

We may regard it as proved, then, that the true specific gravity of the specimen of urine to which the analysis of Berzelius applies is 1030. And it should be observed, also, that the highest multiplier, namely 2·33, gives a nearly exact result as to the whole quantity of solids present; for 30 × 2·33 = 69·90, or, as nearly as possible, 70,—the round number assumed above as denoting the quantity of solids present in 1000 water-grain measures of this specimen of urine.

It must be confessed, however, that little progress can be made towards determining by this method which multiplier deserves the preference, until many trustworthy analyses of the urine, as it exists under different circumstances compatible with health, can be procured on the exact plan adopted by Berzelius. In the meantime, some progress might be made, at no great cost of trouble, if those who have leisure were to accumulate information by determining the exact proportion of urea and of inorganic salts in a number of specimens of urine. It is easy to determine the proportion of urea by the method of Liebig, to which I endeavored to draw attention in a recent number of this journal; and it is a work of no great difficulty, to those who have time on their hands, to determine the amount of inorganic salts. Thus, if the urine be reduced by heat to the consistence of syrup, and pure nitric

acid be added to the extent of 4 or 5 per cent. on the original quantity of the urine; if the evaporation be continued till the whole will solidify on cooling; and, lastly, if portions of the mass be projected into a red-hot platinum capsule, the fixed salts will be obtained in the form of a white mass. It is true that, after we have ascertained the proportions of urea and of the fixed salts, we cannot discover the proportion of the organic matters other than urea unless we know the whole amount of solid matter present. But of an easy method for this purpose I am going to speak before closing this paper.

Before leaving this subject I will only add a single illustration, by way of showing the point from which any further investigation must set out. Let us recur, then, to the diagram of the constitution of the urine in which the solid matter was regarded as falling under three divisions, in each of which the proportion was held to be 15, while the amount of the solvent was taken at 1000, less the total expansion. Of the amount of expansion produced by these several divisions, there is no certainty except in the case of that consisting of urea. With respect to the expansion produced by the other two divisions, though a figure can be produced by calculation for each, yet, owing to the insufficiency of the data, these numbers must be regarded as merely provisional. For every 15 parts of urea dissolved, it is certain that the expansion is nearly 11; for every 15 parts of organic substances other than urea, the provisional number denoting the expansion is 9; and for every 15 parts of inorganic salts, the provisional number denoting the expansion is 5.

Let us first attend to the result when the formula is used without change on these numbers. If 15 parts of each of the three divisions of constituents be together dissolved in 975 parts of water, the aggregate of the expansions is 25, so that the solution occupies 1000 water-grain measures. The total amount of solid matter is 45, from which the specific gravity may be deduced by subtracting from it the number denoting the entire expansion, $45-25=20$, which is the excess of the density above 1000. Or the specific gravity in this case is discovered by estimating the total weight of the solution, thus:—Water, 975; solids, 45—sum of both, 1020. Here the excess of the density above 1000 is 20; which number, with the highest multiplier, gives a product somewhat in excess of the amount of the solid matter present, $20 \times 2{\cdot}33 = 45{\cdot}60$.

If 6 parts of urea and 15 parts of each of the other two divisions be dissolved, the aggregate of the expansions, viz., 4·3, 9, and 5, amounts to 18·3; which number, taken from the entire sum of the solid matter, namely, 36, gives 17·7 for the specific gravity. Here, then, the number 2, as multiplier, gives very nearly the actual quantity of solid matter present. Again, if 34 parts of inorganic salts, 8 parts of urea, and 4 parts of organic matter other than the urea, be dissolved, the total expansion is nearly 18 (nearly 11, 4·7, and 3), which taken from the whole amount, 46, leaves 28 for the specific gravity. But 28 gives, with the leastmultiplier, 1·65, very nearly the exact amount of oslids present, viz.; 46·48. If, then, the above diagram of the general constitution of the urine be found to come near the truth, and if the numbers adopted be discovered, on further investigation, to express nearly the average expansion created by the organic matters other than urea and the inorganic salts, the highest multiplier, 2·33, proposed by Dr. Christison and extended by Dr. Day, is that which decidedly deserves the preference.

The conclusion to which many will be disposed to come, from what has been advanced in this paper, will probably be, that the settlement of the point at issue is not of sufficient utility to deserve so much trouble. It may be so; but the inquiry itself, independently of the result, cannot but lead to the accumulation of useful details on a subject of growing importance in practice.

For example, I do not find that attention has been anywhere drawn to the remarkable effect of urea in creating expansion of the solvent during its solution. The sugar of grapes, commonly regarded as identical with the sugar of diabetes, has, I find, a similar property—the sugar of grapes, dissolved to

the extent of 50 grains, gives an expansion equal to 30 water-grain measures. Cane-sugar has nearly the same effect.

With regard to the mode of determining the specific gravity, it appears to me that an urinometer with a properly constituted scale, the requisite attention being given to variations of temperature, is sufficient for all purposes connected with practical medicine. It should be seen to that the scale is properly graduated. I observe a very singular reason, given most unaccountably both by Golding Bird and Thudichum, for the inequality of the degrees of the scale, namely, that "the density of each stratum of fluid in which the instrument is immersed, of necessity increases with the depth from the surface." The degrees of the scale are necessarily unequal, because each is a sign of a new proportion; since the volume of the portion of the instrument immersed is in the inverse ratio of the density of the fluid into which it has been plunged. If any further reason be required, I should venture to say that the higher the urinometer stands out of the fluid, the greater is the pressure owing to the difference of weight in water and in air, with which it resists the buoyancy of the part immersed, so as to render the degrees shorter in proportion as these approach the lowest part of the stem.

I lately extemporised an urinometer, on the plan of which a convenient instrument might, I think, be made. I took a small pipette, and, having closed the lower aperture by the gas flame, I put a sufficient quantity of mercury into the interior to cause the instrument to sink perpendicularly below the level of the bulb. The pipette now floated, in pure water, perpendicularly with the water line, nearly midway between the bulb and the upper extremity. I then fixed a thin slice of common cork on the upper end, so as to form a small table on which weights might be laid. I also passed a wire with a slender point perpendicularly through the cork till it touched the surface of the water, to serve for an exact index to the height at which the instrument stood in the water. When this rude instrument was placed in a fluid denser than water, it rose, of course, throwing even a portion of the bulb above water; but, by placing weights on the little cork table, it was made to sink to the level at which the point of the wire grazed the surface of the water. Since by this means the same bulk of fluid is evidently always displaced, whatever be the density and whatever the temperature at the time, provided the weights on the little cork table are adjusted to keeping the point of the perpendicular wire just grazing the surface of the fluid, those weights will exactly indicate the difference between a given bulk of distilled water and the same bulk of any denser fluid. Whatever be the temperature at the time, the index for distilled water is shifted to answer for that temperature. If the bulk of fluid displaced by such an apparatus be not known, all that is requisite, for example, in the case of the urine, is to immerse the instrument in a fluid known to have a density of 1060, and to ascertain how many grains suffice to sink the point of the wire to the surface; and the product of this number divided by 60 will answer to the weight required for each degree between 1000 and 1060. Here there is no inequality of degrees. The sixtieth part of the difference between the weight of the volume of distilled water displaced, and the same volume of the standard fluid at 1060 equally displaced, corresponds to a degree. I find it convenient to use pieces of thin lead-wire in such experiments, owing to the facility with which these can be adjusted to any weight. Such an instrument, of course, is better suited to the study than to the sick-chamber.

With respect to the determination of the proportion of solid matter in the urine by evaporation to dryness, it has long been manifest that the difficulty of the ordinary methods place them entirely beyond the reach of medical men for practical use. A mode, however, occurred to me some time ago, which I have repeatedly put to trial. This mode is, I think, susceptible of very great accuracy, and at very little cost of time. It must be confessed, nevertheless, that without due precautions, it may lead into very considerable errors. It is founded on so simple an observation as that common filtering paper readily takes up a large proportion of urine, and, under favoura-

ble circumstances, dries in no long time. The only considerable source of error is the difficulty of bringing back the paper to exactly the same hygrometric state in which it was at the beginning of the experiment. The paper is to be carefully weighed before being moistened, and weighed again with the same care after it has become dry—the difference in weight is the amount of the solid matter in the quantity of urine employed. For 1000 grains of urine, or rather for 1000 water-grain measures of urine, I have commonly used three sheets of filtering paper, cut each into four pieces. The urine is poured into a well-dried basin, and each piece is moistened in succession, as far as it will bear without dripping; enough being left unwetted at its upper part to allow of a fold, by which it may be hung on a string or wire to dry. For this effect a few hours suffice. If the temperature and hygrometric state of the apartment can be kept uniform during the whole process, there is plainly no difficulty; if the drying be carried too far, there is an error of deficiency in the amount of solids inferred; if the drying be insufficient, or if the apartment has declined in temperature, or its hygrometric state has become greater, there is an error of excess. It should be remembered that the paper, though apparently dry, readily absorbs moisture, owing not only to its original tendency, but to the deliquescent character of the solid matters contained in the urine.—*Edinburgh Med. Journal, Aug.* 1860, *p.* 105.

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## 52.—ON THE FREQUENT OCCURRENCE OF PHOSPHATE OF LIME, IN THE CRYSTALLINE FORM, IN HUMAN URINE, AND ON ITS PATHOLOGICAL IMPORTANCE.

By Dr. Arthur Hill Hassall, London. Read before the Royal Society (Communicated by Dr. Sharpey, Sec. R.S.)

In 1854 I submitted to the Royal Society a paper, 'On the Frequent Occurrence of Indigo in Human Urine.' The communication, which was published in the 'Philosophical Transactions,' attracted considerable attention both at home and abroad. The singular fact of the frequent presence of indigo in the urine, first announced by me, has since been amply confirmed by a variety of observers. I have now to place before the Society some investigations in relation to the not uncommon occurrence in human urine of *phosphate of lime*, as a *deposit*, in a well-marked *crystalline* form.

When the earthy phosphates are treated of by writers, in connexion with the urine, they are usually described collectively, and it is seldom that each kind of phosphate is particularized, and yet there are several which may occur either separately or together. The phosphate of ammonia and magnesia, or triple phosphate, is often specified, but rarely is phosphate of lime separately mentioned, and phosphate of magnesia scarcely ever; and yet phosphate of lime is very frequently present as a deposit in urine, much more so, indeed, according to my experience, than the triple phosphate, excluding those cases of the occurrence of that ammoniacal phosphate arising from the decomposition of the urine subsequent to its escape from the kidneys. Even in these few cases in which phosphate of lime is specially mentioned, it is described *usually* as mixed up with the other phosphates, and *always* as occurring in the *amorphous or granular*, and never in the crystalline state; further, no peculiar importance is attached to it, as contrasted with the magnesian phosphate.

Even one of the most recent writers on the urine gives the following description of the physical characters of deposits of phosphate of lime in urine:—"Deposits of phosphate of lime," he states, "as usually occurring in the urine, and mixed with magnesia, are always white and amorphous, under the microscope appearing in granules, sometimes of a greenish tinge, which exert a refracting action upon light. *Crystallized deposits of this substance have not been observed.*"

I now propose to show, first, that there is a crystalline deposit, the crystals composing which will be described hereafter, which does really consist

of phosphate of lime; second, that it is of frequent occurrence in human urine; and third, that it is of greater pathological importance than the deposits of triple phosphate.

I would first remark that I have for years been acquainted with the fact of the occurrence of crystalline phosphate of lime in the urine, and I have referred to it in the Lancet of 1853, and also elsewhere. I should now remark, however. that the statement made by me as to the composition of the crystals, has hitherto been based upon their *qualitative* analysis only, and therefore was not so completely conclusive and satisfactory as could be desired. Until recently I had not made any *quantitative* analyses: these I have since been enabled to perform, and I now furnish the results of the chemical examination of four samples of the deposit.

*First Sample.*—Filtered from the urine of twenty-four hours; mixed, as ascertained in the first instance by means of the microscope, with a very minute quantity of *triple phosphate.*

| | |
|---|---|
| Bibasic phosphate of magnesia . . . | 0·15 |
| Bibasic phosphate of lime . . . | 1·85 |
| | 2·00 |

*Second Sample.*—Filtered from urine after the lapse of a day or two; mixed with a small quantity of *triple phosphate.*

| | |
|---|---|
| Bibasic phosphate of magnesia . . . | 0·47 |
| Bibasic phosphate of lime . . . | 6·18 |
| | 6·65 |

*Third Sample.*—From urine of twenty-four hours after the lapse of several days. Admixed with much triple phosphate, as shown first by the microscope, and afterwards by the chemical analysis.

| | |
|---|---|
| Bibasic phosphate of magnesia . . . | 4·30 |
| Bibasic phosphate of lime . . . | 5·41 |
| | 9·71 |

*Fourth Sample.*—Separated from six ounces of fresh urine. Deposit very pure.

| | |
|---|---|
| Bibasic phosphate of lime . . . | 1·96 |

No phosphate of magnesia.

Now the admixture of the phosphate of magnesia in the first three samples, was due solely to the fact, that the phosphate of lime deposited at first in the pure state, was allowed to remain in the urine until decomposition had commenced, and the phosphate of magnesia and ammonia had, in consequence, become formed. Deposits of phosphate of lime are sometimes contaminated from the same cause with carbonate and oxalate of lime.

These analyses are therefore conclusive as to the composition of this earthy phosphate. In order to show that no error has been committed in them, I here append the process adopted. That the deposit in question really consisted of a *phosphate*, was first repeatedly determined by the action of a solution of nitrate of silver; the crystals, when touched with this reagent, assumed a bright golden-yellow colour. After having been separated and washed in distilled water, the phosphate was ignited to free it from animal matter, urea, &c., and weighed. It was then dissolved in hydrochloric acid; ammonia was added until a permanent precipitate formed; this was re-dissolved by the addition of acetic acid. First the *lime* was precipitated from the solution by oxalate of ammonia, and afterwards the *magnesia* as follows:—Chloride of ammonium was added, then ammonia in slight excess, and lastly, phosphate of soda. The oxalate of lime formed was converted into carbonate of lime in the ordinary manner, and the phosphate of ammonia and magnesia into the pyro-phosphate of magnesia; these

were then weighed separately, and the amounts of the bibasic phosphate of lime were determined by the usual calculations. The results obtained corresponded very closely with the original weights of the ignited phosphates subjected to analysis. The analyses, therefore, show that the crystallized phosphate of lime is a tribasic phosphate, containing two atoms of lime, and, most probably, one of water.

*Form of the Crystal.*—The size, form, and arrangement of the crystals of phosphate of lime, as they occur in human urine, vary greatly, but the peculiarities are in all cases sufficiently characteristic to allow of the ready identification of this phosphate by means of the microscope. The crystals are either single or aggregated, most frequently the latter, forming glomeruli, or rosettes, more or less perfect. Sometimes they are small and needle-like, and then they frequently form, by their crossing and union at right angles, glomeruli or spherules. Sometimes the crystals are thin and flat, having oblique or pointed terminations. Very frequently, however, they are thick, and more or less wedged-shaped, and united by their narrow extremities so as to form more or less complete portions of a circle; the free larger ends of the crystals are usually somewhat oblique, and the more perfect crystals present a six-sided facette. I have never yet met with these crystals having both ends perfect, owing, I believe, to the tendency which they have to crystallize from a centre in rosettes. When these crystals are kept in the dry state for a long time, they not unfrequently break down and crumble into powder.

The late Dr. Golding Bird, in his work on "Urinary Deposits," has given a representation of some crystals which he has denominated "*penniform*," describing them as consisting of a variety of the magnesian phosphate; the crystals figured do, however, undoubtedly represent a modification of those of *phosphate of lime*. Although I have elsewhere pointed out this error, most recent writers on the urine still persist in describing these crystals as a variety of the phosphate of ammonia and magnesia.

*On the frequency of their occurrence.*—I find, as already stated, that phosphate of lime, in the form of crystals, is of much more frequent occurrence in human urine than the triple phosphate, excluding those cases of the presence of the latter phosphate which are due to the decomposition of the urea of the urine subsequent to its emission. I have met with deposits of crystallized phosphate of lime in some hundreds of urines, and in many different cases; it is therefore not a little remarkable, from the frequency of its occurrence, and the peculiarities presented by the crystals, that it should have been so long overlooked. The microscope, therefore, furnishes us, in most cases, with the ready means of detecting the presence of deposits of phosphate of lime, as of so many other urinary deposits.

*Characters of Urine depositing Crystallized Phosphate of Lime.*—The urine from which phosphate of lime is deposited is usually pale, but occasionally it is high-coloured; the quantity passed is large, and the calls to void it frequent, more or less uneasiness and smarting being occasioned by its passage, at the neck of the bladder and along the course of the urethra; its specific gravity varies greatly. Taking the whole quantity passed in twenty-four hours, it is usually below the average, nevertheless the animal matter and urea are absolutely in excess. It is generally feebly acid, often decidedly so when first voided, the greater part of the phosphate of lime becoming deposited while the urine still retains some degree of acidity; it, however, speedily becomes alkaline, owing probably to the excess of mucus contained in it. Sometimes the crystals of phosphate of lime are thrown down from the urine before its escape from the bladder; ordinarily, however, the urine is bright and clear when passed, and the crystals are not formed until some time after it has been voided. In collecting this phosphate for analysis, the object being to procure it in as pure a state as possible, and as free from phosphate of ammonia and magnesia, oxalate and carbonate of lime, it should be separated from the urine very soon after it has become deposited, and before decomposition has had time to set in.

*On the Pathological Importance of Deposits of Phosphate of Lime in Human*

*Urine.*—Of the pathological importance of excess of phosphate of lime in the urine not a doubt can be entertained, but certain reasons and facts may be advanced to show that deposits of that phosphate have a deeper pathological significance than those of the phosphate of ammonia and magnesia. The proof of this is the more necessary, since writers on the urine are in the habit of describing, as well as of treating, deposits of the earthy phosphates collectively, and without distinguishing between them: this course was natural enough so long as they were unacquainted with the fact that deposits of phosphate of lime, in the state of crystals, are of frequent occurrence, or so long as they mistook them for a variety of the ammonio-magnesian phosphate. One reason why we should be disposed to attach greater importance to the excess of the calcareous than the magnesian phosphate, is that most of the phosphoric acid of this last phosphate, and all the magnesia, are derived from *without*, being contained in the various articles consumed as food; while for the phosphate of lime, we have in the system—in the teeth and bones, and also in the nitrogenous tissues—sources containing some pounds weight of this phosphate.

That the osseous system is subject to disintegration is certain, and that the extent and rapidity of this differ remarkably in different cases is equally so. This is shown by the simple fact alone, of the early and rapid decay of the teeth in many persons. For this general reason therefore only, we should, *à priori*, be disposed to attach greater importance to the occurrence of deposits of phosphate of lime than those of phosphate of magnesia.

Other facts tending to confirm this view are,—first, that while deposits of phosphate of lime are frequently met with, those of phosphate of magnesia (not the ammonio-magnesian phosphate) are exceedingly rare; and seemed, that the calcareous is of more difficult solubility than the magnesian phosphate. This last circumstance explains probably why phosphate of lime falls as a deposit from acid urine, while phosphate of magnesia remains in solution.

The particular or special reasons for regarding deposits of phosphate of lime as of more moment than those of the triple phosphate, are derived from direct pathological observation. I have observed that when this deposit occurs, it is very apt to be persistent; and when it has disappeared, to return whenever the health is reduced from any cause. I have also noticed, that when it is persistent, it is usually associated with marked impairment of the health, and this often where organic disease does not exist. The prominent symptoms in one case of calcareous phosphatic deposit which I have had under observation for some years, were,—great disorder of the digestive organs, frequent and distressing headaches, occasional vomiting, debility, emaciation, great irritability of the nervous system, sexual powers weak, pulse slow and feeble, skin cold, urine in excess, of rather low specific gravity, acid when passed, but soon becoming alkaline, micturition frequent, with irritation at neck of bladder and in the course of the urethra; teeth much decayed. It should be stated that there is in this case a very slight tendency to paralysis of the right leg, as shown by an occasional sensation of coldness in the limb, and slight deficiency of power in it at times only. This symptom is, however, by no means a constant or necessary one in such cases.

If these views of the pathology of phosphate of lime be correct, we should expect to find an excess of that phosphate in the urine in great and rapid waste of tissue, during the rapid decay of the teeth, and in cases of mollities ossium. That there is an excess of the calcareous phosphate in the urine in these cases, is shown alike by observation and analysis.

It is obvious from this imperfect sketch, that much remains to be effected in regard to the pathology of phosphate of lime; but now that the frequency of its occurrence in human urine as a crystallized deposit is made known, its pathology, apart from that of the triple phosphate, will no doubt be specially considered.

It will be apparent from the following quotation, that the late Dr. Golding Bird regarded deposits of phosphate of lime of more consequence than those

of the triple phosphate:—"The pathological state of the system accompanying the appearance of deposits of phosphate of lime is analogous to that occurring with the triple phosphate; indeed, as has been already observed, they often, and in alkaline urine always, occur simultaneously. So far as my own experience has extended, when the deposit has consisted chiefly of the calcareous salt, the patients have appeared to present more marked evidence of exhaustion, and of the previous existence of some drain on the nervous system, than when the triple salt alone existed, unless its source is strictly local.

It should be remembered that these remarks of Dr. Bird refer to deposits of phosphate of lime in the *granular state*, and not to the crystalline deposits, with the occurrence of which he was unacquainted. I have already stated that, according to my experience, the granular calcareous phosphatic deposits are much more rare than the crystalline.

It follows from these observations and investigations:—

1st. That deposits of *crystallized* phosphate of lime are of frequent occurrence in human urine, much more so, indeed, than those of the amorphous or granular form of that phosphate.

2nd. That the crystals present well-marked and highly characteristic forms whereby the identification of this phosphate, by means of the microscope, is rendered easy and certain.

3rd. That there is good reason to believe that deposits of phosphate of lime are of greater pathological importance than those of the phosphate of ammonia and magnesia.—*Lancet, Aug.* 4, 1860, *p.* 111.

# SURGERY.

## AFFECTIONS OF THE BONES AND JOINTS, ETC.

### 53.—CASES OF EXCISION OF THE KNEE-JOINT.

[The following nine cases appear in the 'Lancet,' in its "Mirror of the Practice of Medicine and Surgery in the Hospitals of London." Probably statistics would now show a very fair amount of success in cases of excision of the knee-joint. The operation is undertaken with more confidence than formerly, and, in suitable cases, is gradually superseding amputation. The editor observes:]

To speak of particular points in connexion with excision of the knee, it seems to be questionable whether the operation should be resorted to in the young, because the affected limb, after the loss of its main joint, does not grow proportionately with other parts of the body. This has been found to be true in some cases; nevertheless, the shortened limb with a high-heeled shoe is preferred by the patients themselves to going about with a single leg. It would appear that there is scarcely a disease of the articulation, excluding of course anything in the shape of malignancy, for which excision may not be practised. It may be done for even extensive necrosis; indeed, we can call to mind other instances besides the subjoined wherein Mr. Fergusson adopted this mode of treatment with decided advantage; and we have the recent experiments of M. Ollier, of Lyons, to prove that if the periosteum be left entire in the vicinity of joints, it will reproduce bone.

At one time the patella was allowed to remain, because it added support and strengh to the knee; but subsequent experience has tended to show the advantage of its removal. Its retention is still a subject for consideration.

The great inducement to perform excision of the knee-joint at the present day is the fact that the per-centage of deaths consequent on this operation is much less than that attending amputation; a reference to the memoir of Mr. Butcher and Mr. Price will satisfactorily prove the truth of this. At Guy's Hospital, Mr. Bryant has shown, the per-centage of deaths in *pathological* amputation is 1 in 5·5, the same as in excision of the knee; whilst that in *chronic disease* is 1 in 7, being somewhat more favourable. The statistics of amputations elsewhere, taken generally, will not give such favourable results as these.

*Removal of Necrosed Bone, with subsequent Excision of the Knee-Joint in a lad aged 14; Recovery.* Under the care of PROF. FERGUSSON, at King's Collge Hospital.

E. H., aged fourteen, was admitted Dec. 27th, 1859. He stated that in May, 1858, he was being carried down some steps, when his knee came in contact with the iron railing; the joint was slightly painful at the time, but he thought little of it, and it improved by fomentations and rest. About six weeks after the accident, however, he experienced a good deal of sharp, shooting pain in the joint, with considerable swelling. He sought medical advice, but no remedies appeared to have a beneficial effect upon it. This state of things continued three months, when an abscess was discovered to exist in the joint, which was opened, and an immense quantity of matter evacuated. This gave him some temporary relief, but the knee had now become considerably flexed, obliging him to adopt crutches to enable him

to get about. He continued much in this state up to the time of admission.

The patient is a pale, delicate, strumous-looking boy. He lies on his left side: the thigh being flexed on the trunk, the leg on the thigh. There are several sinuses in the region of the joint leading to bone; one, on the inner and lower part of the thigh leads to an extensive bare surface of the femur. The patient has no brothers or sisters; father and mother healthy. No lung affection; other functions of the body natural.

Jan. 7. The patient having been narcotized, Mr. Fergusson proceeded to remove a piece of the shaft of the femur about six inches in length, by an incision of two inches, made at the lower end of the thigh. The removed portion was easily detached, and found to be necrosed.

8th. Has had a good night; complains of slight thirst. Limb rather uncomfortable; pulse 106.

10th. Wound suppurating healthily; the patient says the limb is not so painful as before the operation; appetite good.

Feb. 2nd. Has gone on well since last report; eats, drinks, and sleeps well; is anxious to have his leg put straight.

March, 3rd. To day the operation of excision of the knee-joint was performed by Mr. Fergusson, the patient being under the influence of chloroform. The H incision having been made in the integuments, sufficient of the ends of the femur and tibia was cut off to bring the limb in the straight position. The patella was removed. There was very little hemorrhage during the operation, no vessel requiring a ligature. A splint having been applied, the patient was sent to bed.

4th. Has had a restless night, the limb having been very painful; but the pain has been much mitigated by an opiate draught; pulse 120.

5th. Is much better to-day in health, but the leg continues to give great pain at intervals. To have three grains of sesqui carbonate of ammonia in an ounce of decoction of cinchona every four hours.

10th. Wound discharging healthy; no bad symptom; appetite good; bowels regular; all pain has disappeared.

20th. Is still going on well, the wound discharging a thin, serous fluid, mixed with pus; splint removed; the joint seems firm; splint reapplied.

May 1st. Gutta-percha splint applied to the back of the leg.

9th. Discharged to go into the country.

*Six Cases of Excision of the Knee-Joint performed at St. Thomas's Hospital during* 1859*: Recovery in Five.* Under the care of Mr. SOUTH, Mr. SOLLY, Mr. LE GROS CLARK, and Mr. SIMON.

The operation in the following cases was performed much in the same manner. Chloroform was given, and the joint was opened by an elliptical incision, which included the patella in the flap. The ends of the bones and the joint surface of the patella were removed by Butcher's saw, and the limb was put up in a slightly-bent iron back splint. Liberal diet and stimulants were given, and all the patients had cod-liver oil and some preparation of iron.

*Case* 1.—A strumous, delicate man, aged thirty-seven; had disease of the sternum and ulceration of the cartilages of the left knee-joint. He suffered much pain, his rest was disturbed, appetite bad, and his health generally broken. Two sinuses, connected with the joint, discharged a watery pus. Excision was performed by Mr. South, Nov. 5th, 1859. The cartilages were found extensively eroded, and the head of the tibia secondarily affected to a slight extent. At first, after the operation, his health improved; the wound united in part, and healthy suppuration was established: but towards the end of February diarrhœa of a very obstinate character set in, with night-sweats. By the end of March his condition was by no means satisfactory; there was no apparent union of the bones, his diarrhœa continued despite the use of various astringents, he was sick at times, and his appetite was capricious. The wound did not discharge excessively, and it looked healthy. From this time there was but little alteration in his condition, and certainly none for the better, and he went out, at his own particular request, on the

2nd of June. There was then no amount of bony union, the wound was discharging a tolerably healthy pus, and he did not suffer pain. He died a short time after leaving the hospital.

*Case* 2.—A very strumous-looking boy, aged six, with chronic ulceration of the cartilages of the right knee-joint, the tibia being dislocated backwards. He was hectic; suffered much pain; and there was a discharging sinus connected with the joint. Excision was performed by Mr. Solly on the 9th of November, 1859. The cartilages were extensively eroded and the ligaments destroyed. After the operation he improved at once, never having a bad symptom. The wound healed; he gained health and strength, and left the hospital with scarcely any perceptible shortening of the limb, being able to walk with ease and comfort.

*Case* 3.—A delicate, scrofulous boy, aged eight, with chronic ulceration of the cartilages of the knee-joint; free discharge from three sinuses. He was hectic, and suffered much pain. Excision was performed on the 5th of March, 1859, by Mr. Solly. There was a considerable erosion of the cartilages, and false membrane in large quantities; but no disease of bone. The wound united in great part by first intention; his health improved; he slept well; and his appetite was excellent. He left the hospital quite stout and well; the limb was only half an inch shorter than its fellow, and he could walk with ease and rapidity.

*Case* 4.—A strumous boy, aged twelve, with long-standing suppuration in the knee-joint and several discharging sinuses. He was much out of health, and suffered greatly. Excision was performed by Mr. Le Gros Clark on the 12th of March, 1859. The cartilages were found to be in great part removed, and there was some secondary disease of the head of the tibia. A considerable portion of the wound united by primary intention. He slept well, and took his food with relish. During the progress of the case he had a slight attack of erysipelas, and an abscess formed over the ankle. Despite these drawbacks, the result of the case was favorable; and when he left the hospital there was firm bony union, and only a small portion of the wound uncicatrized. The limb was but slightly shortened.

*Case* 5.—An emaciated, sickly girl, aged six, with scrofulous disease of the knee-joint. She had suffered much; slept badly; and had but little appetite. Excision performed Nov. 12th, 1859, by Mr. Simon. The cartilages were eroded, and there was softening of the end of the femur. For a few days after the operation her condition was very critical, but when suppuration was established she improved, slept better, and was more cheerful. She was sent from the hospital to the Margate Infirmary. There was firm bony union, and only a slight amount of ulceration in the line of the incision when she left St. Thomas's.

*Case* 6.—An unhealthy, pasty-looking man, aged twenty-three, with long-standing disease of the knee-joint. After his admission two incisions were made, and considerable quantities of pus escaped. He did not improve much, and excision was performed Nov. 19th, 1859, by Mr. Simon. There was extensive ulceration of the cartilages, and superficial caries, dependent upon a small sequestrum in the head of the tibia. He improved in health after the operation, and a good deal of the wound united by first intention. A counter opening was required, when suppuration occurred, to give exit to some retained pus. When he left the hospital there was firm union of the bones, he was able to get about, and was in very fair health.

*Two Cases of Excision of the Knee-joint performed at the Royal Free Hospital; Recovery.* Under the care of Mr. GANT.

*Case* 1.—John P., aged eighteen, was admitted on Oct. 19th, 1859. The patient is a strumous, unhealthy looking subject; a coal-porter. While following his occupation about four years and a half ago, a large piece of coal fell on his right knee, and occasioned pain for some days, after which he resumed his work, although the joint was much swollen, and still painful. He continued to work during the ensuing three months, the joint remaining swollen, and latterly the hamstring muscles contracted so as to draw the limb backwards to nearly a right angle with the femur. He then went under

treatment at the Westminster Dispensary for four months, during which period the muscles gradually drew the heel up to the buttock. He afterwards resorted to St. George's Hospital, where he experienced some relief, under the care of Mr. Hawkins, and was discharged at the end of three months. Thence he went to St. Mary's Hospital, and was a patient for more than eight weeks, under the care of Mr. Lane. Subsequently, at the Westminster Hospital, he remained for ten months, under the care of Mr. Guthrie, when he was discharged. After six months' suffering, he was again admitted into the same hospital, and continued under treatment for two months more. At length he sought relief at the Middlesex Hospital; but having remained for three months, without any improvement, he left the hospital. Eventually he came to the Royal Free Hospital, and was placed under the care of Mr. Gant on the 19th of October last.

The joint was found to be uniformly swollen, but not so much as to obscure its outline. A thin unhealthy discharge issued from two or three sinuses which led into the joint, and the patient was much emaciated. His general health having been renovated so far as the local disease and lengthened illness allowed, Mr. Gant excised the joint under chloroform on October 31st. By a transverse and slightly curved incision below the patella, and extending from one condyle of the femur to the other, the joint was fairly laid open. The patella was then removed, although in a sound condition, and a slice of the femur and tibia, both of which were partially bare of cartilage, were sawn off. The femur was most diseased, and a second slice was taken from it, until the rose-coloured bleeding bone was reached. All the diseased bone having been thus got rid of, the surface of the femur and tibia were seen to be well adjusted to each other, and the edges of the incision brought together with sutures of fine silver wire. No vessel required ligature. The limb was at once placed upon an extended Macintyre splint, of such a length as to allow of the femur and tibia to be in easy apposition without being much pressed together by the foot-board. Broad slips of adhesive plaster were then drawn around the limb, a splint applied above and below the knee, and a bandage from the foot upwards, and from the thigh downwards, leaving the knee uncovered, around which were placed strips of wet lint. No side splint was used.

No unfavourable or unusual symptom followed the operation. On the eighth day some hemorrhage occurred, which was arrested by the application of ice.

Dec. 17th. The splint was removed, and union found to have taken place; the limb was replaced; general health much improved.

Jan. 2nd. Union was more firm; health continues to improve; and his trembling agitation has subsided.

27th. Union more firm; limb replaced on a short posterior splint.

From this time the patient sat up in bed daily, but it was deemed advisable not to allow him to try the leg much. When the weather became warmer, he took gentle exercise, with his leg suspended in a sling. He left the hospital with a sound and useful limb, having been allowed to remain an in-patient until he could be admitted into the Margate Infirmary to thoroughly re-establish his health.

*Case* 2.—Elizabeth D., aged thirty-three, married, on September 29th, 1853, while walking down a gentle pescent, slipped over some rotten pears, and caught the heel of her boot in a grating, whereby the knee was wrenched inwards, causing severe pain. Considerable swelling ensued, which never subsided; but the articulation continued movable for three months, at which time, the patient having scrubbed two or three rooms, more intense pain attacked the joint, which soon became fixed. This crippled state continued without any improvement, although leeches and blisters were used from time to time after the injury. The general health remained unimpaired up to October, 1858, when she experienced severe pain, apparently rheumatic, extending throughout the whole limb. This subsided under treatment, but the practitioner to whom she had last applied candidly told her that the

case was incurable otherwise than by ampuation; and by his recommendation she came to the Royal Free Hospital on September 12th, 1859.

The joint was considerably and uniformly swollen. Three sinuses, one in the popliteal space, discharged a thin ichorous matter, and the limb was bent towards the buttock, but was movable with a grating noise, such as plainly bespoke destruction of the articular surfaces. The constitutional irritation was proportionate to the six years' suffering the patient had endured. She was not so much emaciated, but passed sleepless nights, trembled, and was always in tears.

On October 19th, chloroform having been administered, Mr. Gant excised the joint by making the same incision as in the preceding case. The head of the tibia was sawn off to the depth of half an inch; a thicker portion of the femur was taken away, and the cancellated structure scooped out for more than an inch, so as entirely to remove all diseased bone. The patella was also removed. The subsequent steps of the operation and adjustment of the splint were precisely similar to those adopted in the other case.

The general health improved rapidly after this operation. The patient gained flesh and spirits, and progressed without a single unfavorable symptom. At the end of eight weeks the splint was finally removed, and so firm was the anchylosis that no other was applied, the limb being merely bandaged from the toes upward to prevent any tendency to swelling on first moving about.

The patient could rest her whole weight on the operated limb, and she left the hospital in time to eat her Christmas dinner at home.—*Lancet, Aug.* 4, 1860, *p.* 107.

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## 54.—ON EXCISION OF THE HIP JOINT.

By JOHN ERICHSEN, Esq., Professor of Surgery and of Clinical Surgery in University College.

Under the term "coxalgia" a great variety of diseases of the bones in the vicinity of the hip-joint are included. And perhaps the advance of conservative surgery has produced no greater improvement in the treatment of disease than by inducing surgeons to make a more careful and exact diagnosis in cases of hip-joint disease than was formerly the case, when no distinction was drawn between the varieties depending upon the tissues affected. Now surgeons are, for the purposes of operation, in the habit of diagnosing between cases of disease affecting the soft parts only or the bones only; and further, of distinguishing which bone is primarily or principally attacked.

Under the one term "hip-joint disease" are included several forms, commencing in one or other of three primary pathological conditions.

First, in the *arthritic* form, the soft structures—viz., the ligaments, the synovial membranes, cartilages, &c.,—are alone primarily affected, usually with acute inflammation, presenting the symptoms of arthritis elsewhere, the disease usually continuing limited to these structures throughout. Its course frequently tends to abscess. Sometimes, however, the joint recovers without suppuration, but with more or less stiffness, or even complete anchylosis by fibrous adhesions of the opposing surfaces.

The second form—the *femoral*—is that in which the morbid action commences in the upper epiphysis of the femur. The progress of this variety of coxalgia is very insidious; its symptoms are by no means prominent; it generally occurs in young children, and is usually, I believe, of tubercular origin. In it you find the yellow, somewhat soft, friable matter filling up the cancelli of the head and neck of the femur, resembling, and indeed being identical with, tubercular deposit in other organs. This form of coxalgia is accompanied inevitably by the formation of abscess in the soft parts around the joint, commonly on the outside of the thigh, and in the gluteal region; and destruction eventually of the articular surfaces, with caries of the head of the bone, followed by its displacement, and leading to shortening and dis-

tortion of the limb, terminating very generally in the death of the patient from exhaustion and hectic.

The third form is the *acetabular*. Here the disease originates in the pelvic bones. When this, the pelvic aspect of the joint, is the part first affected, abscess invariably forms, sometimes inside the bone, in the pelvic cavity, but occasionally presents externally among the muscles, and points most commonly near the pubes above Poupart's ligament. In these cases, dislocation, though it is met with now and then, rarely takes place. Sometimes, however, the destruction of the acetabulum is so extensive that the head of the femur penetrates it, and passes into the pelvic cavity. Dislocation is most frequent when the disease has arisen in and has destroyed the head of the femur; so that the head being no longer present, no impediment is offered to the action of the muscles around the joint, and the bone at length slips from its position in the cavity of the acetabulum.

It is on that form of disease of the hip-joint which originates in, and is chiefly confined to, the head and neck of the femur, and to the root of the great trochanter, the conservative surgery has most influence. It is, therefore, of great importance to be able to diagnose this form from the other two.

*Distinctive Characters of the Three Forms of Hip-Disease.*—Now, before any question of operating can be entertained, and in only the worst forms and most advanced cases of hip-joint disease can such question be discussed, it is necessary to ascertain to which of the three forms of disease does the case under consideration belong—whether, in other words, it be *acetabular*, and, if so, what is the exact state of the pelvic bones, and which are affected; or *femoral*, and how far the destruction of the head and neck of the femur has proceeded; or whether it be *arthritic*, when operation is seldom allowable.

The determination of this question, which is by no means always easy or even possible, is often much facilitated when we are enabled to watch the case, or have a clear history of its progress previous to any examination. But when we have to deal with an advanced case, of which the account is not clear, and the patient, especially if he be a child, possibly unable to give a succinct history, we may generally, by attention to the following points, arrive at a pretty accurate opinion of the precise extent and seat of the osseous disease. In doing this, the points to be attended to are three in number: 1. The absence or presence of *dislocation*; 2. The existence, position, and extent of sinuses: 3. The condition of the exposed bone at the bottom of sinuses, as the pelvic bones usually undergo necrosis, not caries; the contrary being the case with the femur. It is by attention to those points that we may generally arrive at a pretty accurate diagnosis as to the precise state of the hip.

1. *Dislocation.*—In the arthritic form, this rarely occurs. Anchylosis, fibrous generally, now and then osseous, is the usual termination.

In the acetabular form, also, it is an unfrequent result, the head of the femur usually remaining unabsorbed; and, though the cartilaginous surfaces disappear, the bone remains in its cavity.

But in the femoral variety dislocation is common, the head of the femur being generally disintegrated; that is, the cancellous structure becomes soft, carious, and breaks up into small fragments, which gradually come away in the discharge; and the ligaments being at the same time destroyed, the remainder of the upper end of the bone slips out of the socket, and lies on the dorsum of the ilium, being dislocated backwards and upwards. The limb is thus shortened to the extent of two or three inches, adducted, and crossed in front of the other, its abduction being impossible. This is the usual course in the femoral variety of hip-joint diseases.

2. *Sinuses.*—An attentive examination of the position and direction of these is of great importance in forming an opinion as to the seat of the osseous disease. By passing down them a long probe, we may often ascertain the nature and position of the osseous mischief; but still the amount may be concealed by the fact that sound bone may intervene between the surface and the diseased bone, as when the inner aspect of the great trochanter is

affected, or sometimes the diseased or dead bone is covered in by a thick deposit of dense plastic matter; and so the real extent of the mischief in the bone remains obscure.

*Situation of Sinuses.*—There are three situations in which sinuses are met with, which vary according to their point of origin from the abscess, and the position of the diseased bone; hence it is of importance to note their site and direction. 1. When the sinus opens two or three inches below and a little in front of the great trochanter, about the insertion of the tensor vaginæ femoris muscle, the disease is almost invariably *femoral* in its origin. 2. When in the gluteal region, the sinus may indicate *femoral*, but not unfrequently is dependent on pelvic disease; the acetabulum, or a portion of the dorsum ilii, being the part involved. 3. The sinus may open in the pubic region, either above or below Poupart's ligament. A sinus in this situation is almost certainly diagnostic of disease of the pelvic bones, especially the pubic aspect of the acetabulum. When it appears *above* the ligament, it probably leads to intrapelvic abscess. On the other hand, when the abscess opens *below* Poupart's ligament, there is generally disease of the rami of the pubes or ischium.

The situation of the sinuses, as well as their direction, will thus be found to lead the surgeon to a more accurate diagnosis of the seat and extent of the osseous disase than he could otherwise form. And, indeed, it is only by attention to these circumstances that he is enabled in many cases to form an approximative opinion on these points; for the diseased bone is often so covered in by healthy osseous structure, and by plastic matter, that the probe cannot touch it; or the sinus may be so tortuous that a straight probe cannot follow its sinuosities.

3. But there is other important information obtainable from an attentive consideration of the situation of the sinuses. It is with reference to the probable nature of the osseous disease. In *femoral* coxalgia, this is almost invariably caries: hence those sinuses that indicate the existence of primary disease of the upper epiphysis of the femur proves this to be of a carious nature; whilst, on the other hand, sinuses occurring in the pubic region and by Poupart's ligament are almost invariably dependent on the presence of necrosed bone; necrosis being the form of disease that affects the acetabulum and pelvic bones.

*Cases requiring Operation.*—Diseases of the hip-joint may, so far as the question of operation is concerned, be divided into two great classes—those in which no suppuration takes place, however acute the inflammation may have been; and those in which abscess forms.

To the first class belongs the *arthritic* variety of the disease. In this form of coxalgia, the patient commonly recovers with a stiffened or even completely anchylosed, though useful and straight limb. In such cases, I believe that excision is never needed; at least, I have never had occasion to do it, nor have I ever seen a case that seemed to me to justify such a procedure.

The second class of cases—those in which abscess forms—are by far the most numerous. In the great majority of these, however, recovery will take place eventually, under properly conducted medico-surgical treatment. But the recovery in such cases is always so far incomplete that the limb is left much crippled, and often of but little utility. In such cases as these, after years of suffering and of confinement to bed, and after a hard struggle for existence, we find the unfortunate patient left eventually with a limb that is shortened to the extent of from two to four inches, wasted and adducted, with a projecting deformed hip seamed with cicatrices; the remains of the upper epiphysis of the femur being dislocated from the acetabulum, and adherent to the dorsum ilii by firm anchylosis. The limb is unable to support the body, and cannot be extended or the sole of the foot firmly planted on the ground; but it is to a certain extent useful in progression, the patient using it as a kind of paddle to push himself on with, as he limps on the point of the toes. In these cases, it is interesting to observe how Nature compensates for the loss of all abduction and rotary power in the hip

by giving an extremely increased degree of mobility to the lumbar vertebræ, so that the patient, in walking, swings the pelvis from these, and thus in a great degree makes up for the loss of the natural movements in the coxo-femoral articulation.

But, though recovery takes place eventually in the great majority of cases of coxalgia that have advanced to suppuration, yet in some, and not a few instances, the patient's constitution becomes unequal to the drain imposed upon it, and fatal hectic will eventually supervene. This fatal result is the direct consequence of the wasting and exhausting influence of the long continued discharge of pus from masses of carious or necrosed bone too extensive or too deeply seated to be eliminated by the natural actions of the part. It is in such cases as these that conservative surgery steps in, and endeavours to save the patient's life by the removal of the morbid cause that keeps up the discharge which is wasting it away. The object here is simply to save life by the removal of diseased bone. For the same reason—the preservation of life from hectic—that the surgeon amputates in an extreme case of suppurating disorganisation of the knee-joint, he excises in an extreme case of disorganisation of the osseous structures that enter into the formation of the hip-joint, amputation being here too formidable a proceeding to be undertaken; and yet the removal of the diseased bone, the irritation and suppuration from which is rapidly destroying the patient, being an imperative necessity.

The *femoral* coxalgia is that form of the disease that is most amenable to operation. In severe and extreme cases of this variety of hip-joint disease, the upper epiphysis of the femur will be found lying in a state of caries on the dorsum ilii, in a suppurating cavity, with sinuses leading down to it. The pelvic bones are sound; the acetabulum filled by fibroplastic matter of a reparative character, though possibly slightly roughened and necrosed at one lip. The soft structures in the gluteal region are thinned and wasted, the limb inevitably shortened and adducted. In such cases as these, the patient will probably perish, if left to the unaided efforts of nature; or if he recover, after years of suffering, it will be with a limb shortened, deformed, and but little useful. Whilst resection, being limited to the upper end of the femur, or at most to the roughened lip of the acetabulum, as well, is an easy operation, removes the cause of the wasting discharges and hectic, and in no way increases the already existing shortening, as it is limited to that portion of diseased bone which is already lying above and behind the acetabulum, and which, being carious, must disintegrate and crumble away in the discharges before a natural cure without operation could take place.

In the *acetabular* form of coxalgia, the ultimate result is, I believe, inevitably fatal, if the disease is allowed to run its own course unchecked by operation. Large portions of the pelvic bones in and around the acetabulum fall into a state of necrosis; that cavity becomes perforated; and the head of the femur, still lying in it, becomes denuded of its cartilaginous investment, roughened, and carious. Large abscesses, intra- as well as extra-pelvic, form, and the patient dies worn out by hectic, the natural efforts of the part being entirely unavailing to separate and to eliminate such large masses of necrosed bone, deeply lying and covered in as they are by the upper end of the femur.

Until a very recent period, surgeons feared to undertake the removal of large necrosed portions of the pelvic bones, and acetabular disease of the hip-joint was accordingly allowed to run its fatal course unchecked. Mr. Hancock was the first surgeon who undertook the removal of large portions of these bones, and since then the operation has been done several times by others. In no case has, I believe, more extensive disease been removed with a good result than in a girl of the name of Fallowfield, who was sent to me three years ago by my friend Mr. Tweed, and from whom I removed the upper end of the femur, the acetabulum, the ramus of the pubes and of the ischium, a portion of the tuber ischii, and part of the dorsum ilii. This patient, when admitted, was in the last stage of disease, exhausted by the constant discharge, and must inevitably have speedily sunk under the effects of

the disease, had no effort been made to remove the cause of the prostration. At the operation, she was so exhausted that it was necessary to have her on the operating-table for some hours before she was sufficiently restored to bear moving into bed. Nevertheless, by free use of stimulants and nourishing food, she made a rapid recovery: and a few months ago, the limb was straight, shortened about two inches; good movement existed at the hip; and she could walk with much ease. Constitutionally, she was in perfect health. In cases such as this, the result, if they are left to themselves, must inevitably be fatal. There is no danger of laying open the pelvic cavity during the removal of these masses of pelvic bone; for, as Mr. Hancock has shown, during the progress of the disease, the fasciæ. muscles, &c., lining the pelvis, become so thickened and infiltrated with plastic matter, that they form an effectual barrier which protects the pelvic cavity.

*Method of Operating.*—The patient lies on the sound side. If the disease is femoral, the gluteal region perforated with sinuses, and the soft parts thinned, the head of the bone lying dislocated on to the dorsum ilii, it suffices to pass a director down one of the chief sinuses leading to the carious bone, and to slit this up. If, however, the bone be more thickly covered, and is not easily reached with the probe, and if the sinuses open on the thigh at some considerable distance from the seat of disease, then a T-shaped incision should be made over the upper end of the femur, so as to expose it. The limb should now be forcibly adducted, rotated inwards and pushed upwards by an assistant, the soft structures separated by a probe-pointed knife from around the upper end of the bone, so that the whole amount of disease may come into view. The carious epiphysis is then cut off with a saw, the soft parts around being protected, if necessary, by means of retractors.

There is one practical question with regard to the amount of the upper end of the femur to be removed, which requires consideration; and that is, Should the bone be sawed through below the great trochanter; or through the neck only, leaving the trochanter? The practice should, I think, differ according to the nature of the disease. If this is femoral, it is best to take away the great trochanter, as the caries has generally reached its cancellous structure, or it may be infiltrated with tubercle. But if the disease be acetabular, it will suffice to remove the head only, leaving the trochanter, which is not affected in these cases. After removing the head of the femur, the upper end should be examined, and any carious parts gouged out. After the epiphysis of the femur has been removed, the acetabulum must be examined, and any rough or necrosed bone lying at its edge should be gouged away.

In the acetabular form of the disease, the early stages of the operation require to be conducted in the way just described; the incisions, however, being made more freely, but not carried so far forwards as to endanger the anterior crural, or so far back as to wound the sciatic nerve. After the removal of the head of the femur, all loose pieces of necrosed bone are to be taken out, and then, by means of ordinary cutting or gouge forceps, the acetabulum and other diseased osseous structures cut away piecemeal. In doing this, it will be found that the necrosed bone readily separates by means of the finger, or by the hand of a scalpel, from the subjacent soft structures; and the muscles and fasciæ lining the pelvic bones, being thickened and infiltrated with lymph, effectually protect the contained parts from all injury.

The result of excision of the hip-joint has to be considered from two points of view: 1. So far as the mortality immediately referrible to the operation is concerned; and 2. As to the utility of the limb that is left after its performance.

1. As to the fatality directly referrible to the operation itself. On this point it is difficult to speak with absolute accuracy at present, the statistics of all the operations not being before the profession; but, so far as my own experience is concerned, it is as follows. I have operated in seven cases. Of these, not one has proved fatal; three are now well and going about; two

I have lost sight of, after their recovery and discharge from the hospital; and two have died, one eleven months and the other two years after the operation, from constitutional disease unconnected with it. When we consider that all these cases were instances of advanced femoral or acetabular coxalgia, which would speedily have proved fatal if not subjected to operation, we may with justice look upon them as successful so far as the preservation of life was concerned. And, in this respect, excision of the head of the femur, or of the hip-joint, stands in a different position to similar operations practised on other joints. The surgeon excises the elbow, shoulder, or ankle, in order to restore a useful limb to the patient. Excision of these joints is a substitute for the loss of the limb by amputation—not, as in the case of the hip, for the loss of the life of the patient by a continuance of the hectic.

2. As to the utility of the limb left after excision of the head of the femur or the hip-joint. In estimating the utility of the limb left after excision of the hip-joint, we must compare it with the kind of member that would be left in the event of the patient surviving sufficiently long for a natural cure to result. It would be manifestly absurd to compare a limb which had become seriously crippled, diseased, and shortened, before any operation was undertaken, with one in which no morbid action had ever taken place. So also would it be unjust to compare it with the state of a limb left after a slight attack of coxalgia in which an operation could never have become necessary or been contemplated. But, on comparing the result of operated cases with that of those which recover spontaneously, after caries and destructive disintegration of the upper epiphysis of the femur has existed for years, we shall find that the balance is by no means against those in which excision has been done.—As I do not consider the operation necessary, in cases of *arthritic* coxalgia, I do not compare the result of these cases with that of those operated on, but confine myself entirely to those in which there has been destructive bone-disease. In such cases as these, when, after years of prolonged suffering, recovery is at last accomplished by natural means, what is the condition of the limb that is left? Why, more or less completely anchylosed at the hip, wasted, shortened to the extent of two or three inches, partially flexed upon the pelvis, adducted, with the knee possibly stiffened, semiflexed and advanced, the patient just able to put the toes to the ground, without the power of bearing upon or rotating the limb; but when he wishes to turn, twisting the whole pelvis by the aid of the greatly increased mobility of the lumbar spine.

After successful excision, the result is much more satisfactory, as you had an opportunity of witnessing in the boy on whom I had operated three years ago. The principal morbid appearance in the limb is its shortening to the extent usually of from two to three inches—to the extent, indeed, to which the preexisting disorganisation and dislocation of the head of the bone had previously reduced it. It is well nourished, straight, firm, and admits of easy and rapid progression. The anchylosis is fibrous, not osseous. The patient is then enabled to flex the thigh on the pelvis, and to adduct it: but, just as in the cases that have undergone a natural cure, the power of external rotation and of abduction are lost, the mobility of the lumbar spine compensating for the loss of these movements.—*British Med. Journal, May* 12, 1860, p. 351.

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## 55.—ON THE PATHOLOGY AND TREATMENT OF SCROFULOUS DISEASES OF THE SYNOVIAL MEMBRANE OF JOINTS.

By Richard Barwell, Esq., F. R. C. S., Assistant Surgeon to Charing Cross Hospital.

There is no doubt that even the most chronic, insidious, and painless form of this complaint, is at least in the first stages, an inflammation; and hence, the joint must at once be placed at absolute rest. Some form of counter-irritant should be employed, and proper internal remedies to ameliorate the constitutional condition. This is not the place to enter into the reasons that would cause the choice of one counter-irritant more than another, but it is

desirable to point out the disadvantages attending a certain plan too much in vogue. A patient applying with this disease is generally ordered a splint ---perhaps leeches or a blister; and after a time an issue is made, which is kept poulticed for a month or two, discharging all the while. Now it is evident that nothing can more conduce to the spread of the new tissue, whose encroaching growth constitutes the disease, than warmth and moisture, unless it be an indolent suppurating and granulating sore, such as an issue becomes a week or ten days after its application. If issues are thought desirable, they should be either kept open only for a short time after the separation of the slough, and another be made, or they should be touched again with the caustic potash, and so kept irritable. The former method has its disadvantage in the breadth of surface destroyed, the latter in the depth to which the sore penetrates, and both in the very great pain produced by the treatment. Some persons have believed, and do still believe, that the amount of benefit is proportionate to the pain. This is a mischievous idea, since it leads to a cruel line of practice. The notion, no doubt, was obtained from the observation, that issues benefit mostly at first, and that in a short time their action becomes null, or absolutely injurious, since the constant discharge weakens the tissues, and tends to produce cell generation all around.

A much better, and a much less painful plan than an issue, is the actual cautery; it is one which I would wish to see much more generally used; but its application requires some practice, skill, and knowledge, since an unskilful use is apt to produce too great a destruction of skin, too deep a furrow, or other evils instead of benefits. If the means be judiciously employed in properly chosen cases, its action is marvellous, and now that chloroform prevents the immediate pain, the objections which caused it to be disused in England have disappeared.

The iron which I employ for this purpose is known as Rusts: it is simply a three-sided prism, mounted by one of its oblong facets to a curved handle. The edges are slightly blunted. An iron of this shape has bulk enough to retain the heat a sufficient length of time, and yet presents a small edge. Two irons ought to be used, as a white, not merely a dull-red, heat is desirable, and thus a change of instruments is necessary. While the patient is being placed under the influence of chloroform, the joint to be cauterized should be protected by strips of plaster, or of moistened lint, a good inch, or an inch and a half broad, leaving narrow intervals between each, where the lines of firing are to run. When anæsthesia is fully established, a white-hot iron is to be slowly, and with gentle pressure, passed three or four times along each space, until the skin be turned into a yellow-brown horny substance. Let it be particularly observed that the rays ought not to be nearer than an inch; the skin must not be divided by the iron; the heat must be sufficient to char the skin. This mode of using the iron was called by Percy ('Pyrotechnie Chirurgicale,') "cauterization transcurrente." We might name it linear cauterization. Rust, of Vienna, and Bonnet, of Lyons, employed it with great success. My own experience warrants me in speaking of it very highly. In the first place, it gives no pain whatever at the time of application, and when the effect of the chloroform has disappeared, still no pain from the cautery can be felt. Compare this with the torture produced by the potassa fusa, under which the patient will writhe for hours after the application. But not merely is the hot iron painless; it absolutely relieves pain. I applied it in a case of hip-joint disease, in which the patient was suffering so much that he dared not move, his face was anxious, and his nights all but sleepless. After he had recovered from the chloroform, he was laughing, talking, moving about in his bed, and wanting to get up. In a case of knee-joint affection, the same change from pain to painlessness followed the use of the iron, and the patient is now able to walk. The narrow charred lines made by the cautery begin to separate in from five days to a week, and come away between the fourteenth and twenty-first day. They should then be healed as quickly as possible.

The value of pressure in strumous synovitis cannot be too highly estimated, if the proper cases be chosen for its use; such, for instance, as those

in which the swelling is soft and sluggish, in which a bursting pain is felt, and in which a sensation of cold is experienced. Dr. Scott, of Bromley, first used this plan with considerable success; he applied mercurials to the joints, and over them strapping plaster. The value of the treatment lies in the pressure. Mercurials are of little or no help; in strumous cases I believe they are even injurious. The *modus operandi* of this agent I believe to be, simply preventing the cells, through want of room, from generating afresh, and, therefore, constraining their organizing force to act in the production of tissue from the cells already formed, or in the absorption of those unfit for this purpose. I know no means at all comparable in their power of resolving strumous synovitis to the use of the actual cautery followed by pressure. If, for instance, on the knee-joint the lines have been drawn parallel to the axis of the limb, they will, on healing, contract very considerably like all scars of burns: and if to the pressure thus produced, be added that of strapping plaster, an almost irresistible power of resolution will have been gained. Indeed, unless the case be too far advanced, or too much supported by a vitiated constitution, it will yield to this treatment in a few weeks. The following case shows the value of this method:—

Jane S., aged six, a pale, strumous girl, has been suffering for twenty months from pain and swelling of the left knee, and was admitted into Charing Hospital under my care, (through the kindness of Mr. Hancock,) Dec. 9th, 1859. The knee is a good deal bent, the joint being round and shapeless; the child cries when it is touched, and when any attempt is made to move it.

Dec 21st. I drew four striæ of linear cauterization, one on either side of the patella, and one an inch beyond each of these, and got the limb into a straight position. On recovering from the chloroform the child expressed no sense of pain, but sat up, and began to play with toys and other children in the ward.

On the third day the skin between the cauterized lines looked red and inflamed. On the fifth, some separation at the edge of the slough had occurred. The horny portion gradually separated, and on the 1st January, 1860, the lines left were simply clear lines of ulcer, which were dressed with zinc ointment, discharged very little, and gradually healed. The knee was much diminished in size; morever, it could be pressed without producing symptoms of pain. It was, however, still swollen, and on the 23rd January, when the scar was quite sufficiently healed, the knee was tightly strapped, and a roller applied from the foot.

30th. Strapping applied again, and towards the end of the week the child was dressed, and allowed to get about by aid of the sides of the bed and of a chair.

March 6th. The child is able to go about so well that the mother insists on taking her out; promises to bring her to me amongst my out-patients. This she has hitherto not done.

Other cases showing the value of pressure might easily be given. I recorded three in the Lancet for Dec. 20th, 1859. Several others proving the extreme value of this combination have occurred to me.

We now, however, come to a much-neglected question—one which is, perhaps, scarcely comprehended in regard to the treatment of strumous synovitis—viz., is the principle of perfect rest to be exchanged for that of limited motion at any period of the disease? If so, at what period? I say that this question is either not understood, or is ignored in England altogether; while, in Lyons, M. Bonnet applied some practical suggestions to the answer.

I must, at the risk even of occupying much space, explain the principle whereon the value of passive motion, shampooing, &c., is based. When the cells, which have formed so abundantly in and around the synovial membrane, cease their generative activity, they remain for an indefinite period more or less passive. We know that the gelatinous condition thus produced may last for years without change, and that, being left undisturbed, the mass will, after a time, become the seat of abscess, cause destruction in

surrounding parts, produce caries, &c.; or, in more favourable cases, it will begin to assume a fibroid, and ultimately unite the two bones by anchylosis. Now, I have no hesitation in saying that, in the large majority of instances, the passive condition may be taken advantage of to induce a much more perfect, frequently an almost complete, cure of the malady; and that, by allowing it to continue, a much larger mass of disease is formed, much more tissue (periarticular, cartilaginous, or osseous) is implicated, and, therefore, even if the joint ultimately heal over, the cure is far from being as perfect as it might be made; and if degeneration set in, the ravages are unnecessarily wide-spread. Moreover, be it remembered, that if even a healthy joint be kept very long immovable, fatty degeneration of the cartilages is apt to come on, and even a sluggish inflammation of the synovial membrane. It must not be, however, for one moment imagined, that I would advocate movement of the joint at all stages of the disease. On the contrary, such treatment can only be beneficial during the above-named passive condition, and when all inflammatory signs have disappeared. Hence it is important to form a correct judgment as to this condition. It is well known that each joint, when inflamed, presents a certain spot in which pain and tenderness are much more marked than elsewhere; this is in every joint a fixed point—on the hip, behind the great trochanter; in the knee, the edge of the inner condyle, inside the ligamentum patellæ; in the elbow, at the back, over the head of the radius, and so on. Now, if the surgeon find that pressure upon these points produces no pain, he may begin to move the limb with perfect confidence and safety; and, as far as my experience goes in this treatment (already pretty considerable), with very great benefit. The movement must not, at the beginning, be violent or long continued; a slight passive motion, and that rigidly in the normal direction, a certain amount of rubbing and kneading with the hand, and then reapplication of the splint, are all that can be undertaken; and it may be a long time before it will be safe to abandon the patient's limb to his own management. I have also found it a useful plan when it can be managed, slightly to change the angle of the splint at each reapplication.

The subject of angle at which the joint would be flexed, would naturally lead us to consider another subject—viz., the value, as an actual remedy for inflammation, of changing any vicious position in which the joint may lie. There is no room for doubting the diminution of the inflammation and of the pain after this has been done; and amongst my cases are one or two which were clearly benefitted by this means. But this subject is too wide, and space too limited, for its discussion here.

The three methods more particularly named in this communication—viz., actual cautery, pressure, and passive motion, are neither original nor new; but the first and last more particularly are not used so frequently, nor with such discrimination and power as they might be. Much benefit would result from substituting these means for the negative, if not injurious, treatment to which these strumous cases are too often subject.—*Lancet, June* 30. 1860, *p.* 640.

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## 56.—LOOSE CARTILAGE IN THE KNEE-JOINT—REMOVAL.

By W. Fergusson, Esq., F.R.S., Surgeon to King's College Hospital.

There is some difference of opinion amongst surgeons as to the means to be adopted for getting rid of these bodies. Some prefer to remove them at once, whilst others leave them in the areolar tissues. Experience goes to prove that success more usually attends the latter proceeding, and that sometimes most serious consequences have ensued from the former, involving loss of both life and limb. We recollect seeing an hospital patient submitted to Syme's operation for the passage of a loose body from the knee to the subcutaneous areolar tissue, but the surgeon could not resist the desire of removing it altogether. The consequence was that the most violent inflammation set in, in a healthy constitution and robust frame. It went on

to suppuration, and the leg had to be removed at the thigh, the patient barely escaping with his life. This is by no means a solitary instance of the bad results of direct removal of the loose cartilage, and hence that proceeding has invested almost every case with uncertainty and fear as to the result of any operative measures for relief. Mr. Syme's and Dr. Goyrand's operation, together with various modifications of it, taking the precaution to leave the loose body beneath the skin, at any rate for a time, we have now seen performed on several occasions, without any bad consequences whatever. Some surgeons endeavour to obtain adhesion at the spot recommended for its detrusion by exciting some inflammation by puncturing the synovial membrane subcutaneously with a lithotome. An example of cure by this method, under Mr. Birkett's care at Guy's Hospital, is recorded in a former "Mirror," ('Lancet,' vol. i., 1856, p. 75.) We have heard Mr. Hilton remark at the same hospital that he has treated one case by subcutaneous section with success, and two without any operation, the bodies being kept stationary, and ultimately, he believes, absorbed.

We had the opportunity of seeing another case treated during the past summer at King's College Hospital, in which the loose cartilage had been present probably for ten years, but most certainly for three. Mr. Fergusson preferred cutting it out direct from the joint by means of a valvular incision. In some observations made on the occasion, he stated that he had cut a loose body directly from the knee-joint several times, and the result had always been successful. He adverted to Mr. Syme's subcutaneous operation, but preferred his own method. Fortunately, the present case has turned out tolerably well, but the patient ran very great risks from the extreme severity of the inflammation which followed, synovial fluid and pus flowing from the joint for some days; recovery has ensued with a stiff limb. We have seen recovery take place under precisely similar circumstances, but with firm anchylosis of the joint after a long and tedious illness.

We are indebted to Mr. W. Wickham, house-surgeon of the hospital, for the notes of the following case:—

Thomas H., aged thirty-four, admitted into the above hospital on April 26th, with a loose cartilage in the knee-joiut. He stated, on admission, that ten years ago, whilst walking, he was suddenly seized with severe pain in the left knee-joint. It lasted for a few minutes, and then as suddenly left him. This occurred on several occasions for a month or two, when it disappeared. He was not troubled again with it till three years ago. He could now feel the loose cartilage at the inner side of the joint. About six months ago the joint became inflamed, for which leeches and blisters were employed, and he was obliged to keep his bed for a fortnight; and, on getting up, he found great difficulty in walking, and the joint was very stiff. He had another similar attack of inflammation in the joint three months since. On admission, the man's general health was very good; the left knee was rather larger than the right. On manipulation, the loose cartilage could readily be felt on the inner side of the patella. The patient walked with a stiff knee to avoid pain consequent on the loose cartilage getting between the ends of the bones.

May 12th. The patient being placed under the influence of chloroform, Mr. Fergusson removed the loose cartilage, which was about the size and shape of a broad bean. The cartilage having been first brought to the inner side of the patella, and the leg straightened, a valvular incision was made over it, and, after some difficulty, it was removed by means of a scoop. Some synovial fluid escaped during the operation. The wound in the skin was closed, and the limb placed in a M'Intyre splint. The patient was then removed to bed, and the limb swung.

13th. The patient passed a very bad night; complains of great pain in the joint, which is much swollen and inflamed. Ordered sixteen leeches to the joint, and a pill containing three grains of calomel and half a grain of opium every four hours.

15th. He is greatly relieved since the application of the leeches. Or-

dered poppy fomentations to the joint, and a saline draught with the pills every four hours.

17th. The pain in the joint has returned, and the swelling increased. Twelve more leeches were applied, with much benefit.

18th. He is much better; the wound is now discharging a thin fluid, consisting partly of synovia and partly of pus; the pain is much less, and the swelling has diminished. Poultices were applied to the joint.

29th. The splint was this morning re-applied, with very little inconvenience to the patient, and he did not complain of any pain on applying firm pressure over the joint.

June 21st. The splint was left off to-day, and the whole limb firmly bandaged. The wound in the joint is healed. The swelling in the knee-joint has greatly subsided; manipulation causes no pain. With the assistance of crutches he is able to walk about the ward.

He was discharged to go into the country on July 12th. At this time there was still some swelling of the joint, with very little motion; he could, however, slightly bend the knee without causing pain.—*Lancet, Oct.* 13, 1860, *p.* 358.

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## 57.—CASE OF DISLOCATION OF THE ULNA FORWARDS AT THE ELBOW, WITHOUT FRACTURE OF THE OLECRANON PROCESS.

By Edwin Canton, Esq., F.R.C.S., Surgeon to the Charing Cross Hospital.

[The occurrence of dislocation of the ulna forwards at the elbow-joint, without a simultaneous fracture of the olecranon process, is so rare, that most authors have denied the possibility of its occurrence; others make no mention of this form of injury. One case of this accident is related by Colson in 1835. Mr. Canton says:]

The following case came under my care in the Charing Cross Hospital:—F. P., aged 40, a somewhat short, slim-built, but muscular man, while driving in a light cart at the rate of seven or eight miles an hour, was thrown out, and instinctively extended his right hand to prevent injury to his head. The weight of the body, however, caused sudden and forcible flexion of the elbow, and at the same time the forearm became twisted in under the chest. On rising, it was found that the elbow was considerably swollen, and the power of moving it entirely lost. When admitted into hospital, the forearm was forcibly flexed, and the hand supinated. The swelling, ecchymosis, and tension around the elbow were so great that it was with difficulty any of the more salient anatomical peculiarities of this part could be recognised,—everything appeared, in every way, so disarranged. The skin covering the inner condyle was stretched to the utmost, and here, over a space about the size of a sixpence, it was to such a degree injured, that a compound state seemed to be momentarily threatened. The antero-posterior and lateral diameters of the joint were increased in extent, and the general swelling was so great as to present a circumference far beyond the normal size of this region. Externally and somewhat anteriorly the cup-like cavity of the radius could be indistinctly distinguished: internally, the condyle was unduly prominent; anteriorly no particular point for diagnosis could be determined on, on account of the state of forcible flexion and great tumefaction there; posteriorly also the swelling was very considerable, but, below it, there existed a depression favouring the view that the ulna was broken immediately below its olecranon process. No median gutter, with lateral elevations to bound it, could be felt.

Attempts were made to ratify the mal-adjustment, but without success; the efforts, however, could not be longer continued, for, it was obvious that the injury already sustained by the soft parts was so extensive,—the obstacles to be overcome so resistent, and the great likelihood incurred of rendering the case one of the compound kind, forbade further trial, and it was agreed, in consultation, to place the limb at rest on a splint, and to keep

the parts cool with an arnica lotion. Within the course, however, of forty-eight hours the tumefaction became still greater; a large slough was forming on the inner side of the joint, and high constitutional irritation having set in, I was obliged to amputate the limb at a sufficient distance above the articulation.

*Dissection.*—A very careful examination of the elbow was made, under my superintendence, by my pupil, Mr. Edgar Brown, with the following results:—

*Bones.*—The ulna was dislocated forwards, so that the upper surface of its olecranon process became placed in front of the capitellum humeri, and had thus assumed the position naturally occupied by the head of the radius during flexion of the forearm. The radius was supinated and maintained *in situ naturale*—as regards the ulna—by the coronary and interosseous ligaments being intact.

*Ligaments.*—Of the anterior ligament, the only part remaining at all perfect was a shreddy portion about the centre; all the rest of it had been torn through. The posterior, and both lateral ligaments were completely divided. The coronary and oblique ligaments were injured.

*Muscles.*—The triceps extensor was detached from all its points of insertion. The supinator radii longus was uninterfered with at its origin; but the two radial extensors of the carpus beneath it were torn away from the surfaces whence they spring. All the muscles which arise from the external condyle—with the exception of the supinator radii brevis, and anconæus—were detached from this process. The only muscle that was torn through at its origin from the internal condyle was the flexor carpi ulnaris,—the olecranon and ulnar portions of it, however, continued intact. No mischief whatever had happened to any other of the pronators and flexors. The biceps and brachialis anticus were put greatly on the stretch.

*Blood-vessels.*—Though much shifting of their position had necessarily taken place, no vessel of large size had been injured; the sacrifice, however, of smaller ones must have been great—judging from the large amount of blood with which all the soft textures were infiltrated.

*Nerves.*—The ulnar nerve was torn across where it passed behind the inner condyle. The sheath of the median was distended, and its substance permeated with blood. The other nerves uninjured.—*Dublin Quarterly Journal, Aug.* 1860, *p.* 27.

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## 58.—ON THE TREATMENT OF LATERAL CURVATURE OF THE SPINE.

By Holmes Coote, Esq., F.R.C.S., Assistant Surgeon to the Royal Hospital of St. Bartholomew and to the Royal Orthopædic Hospital.

[After considering the causes and pathology of lateral curvature, showing that after a certain period important changes take place in the bodies and articulating processes of the vertebræ, Mr. Coote proceeds to consider the treatment of the disease. He observes first that for the cure to be effectual it must be done before these changes have taken place.]

The disease commences as one of general weakness of ligaments, just as is seen in cases of knock-knee; but in a brief space of time the opposed osseous surfaces become adapted, and cannot change their form. Of what use then can be cold ablutions, the shower bath, salt and water sponging, or the internal administration of tonics. All the salt and water in the universe will not set straight a distorted back; nor will steel, or other tonic remedies, replace a twisted vertebræ. By mechanical means alone can we hope to check and correct this serious and progressive deformity! This method should be resorted to, not only in the more severe, but in the earlier cases, when hopes may still be held out of even complete and permanent recovery.

Tavernier's belt has been superseded by Tamplin's apparatus, of which a representation is given in the accompanying wood-cut. After many alterations, it is the instrument now commonly used, at the Orthopœdic Hospital. It consists of a steel padded pelvic belt; of two crutches capable of elevation to reach the axillæ, and to take off the weight of the head and upper extremities; of a webbing band to pass in front of the abdomen, where, moderately tightly laced, it holds the instrument forwards, and of one or two pads on steel supports, worked with cog-wheels, to pass the displaced vertebræ steadily, but slowly back. Patience on the part of the patient, care and skill on the part of the surgeon are necessary, that the apparatus may fit without rubbing, and the weight, which is considerable, may be so equally diffused as to be borne without inconvenience.

It acts under the management of the surgeon, by slow, steady, gradually increasing, yet unremitting pressure, before which the strongest ligaments will yield. The pressure should therefore never be so great as to cause pain, nor to irritate the skin; it must be continued night as well as day, for what progressive improvement could be expected, if an extension of twelve hours were succeeded by a relaxation of a similar space of time; some cases require to be seen twice, others three times a week or oftener; for constant adaptation of the pads, or readjustment of the entire apparatus is necessary during the treatment. These points, however, may be readily learnt by the surgeon, who bears in mind the fundamental rule of patience, steadiness, and gentleness. When the spine has been brought again to the straight line, it can be so held by stays not differing in appearances materially from those usually worn.

The constitutional treatment must be guided by general principles. The patient may take exercise as usual, but should rest on an easy sofa, when tired. She may also with prudence, continue her education, so that the duration of the treatment becomes a matter of less regret to the parents.—*London Med. Review, July*, 1868, *p.* 13.

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## 59.—ON THE DRESSING OF STUMPS AFTER AMPUTATION.

By F. Symonds Esq., F.R.C.S., Surgeon to the Radcliffe Infirmary, Oxford.

The chances of quick union and a good stump undoubtedly depend, in a great measure, upon attention to a number of minute details in the dressing. Though the importance of such details have been generally acknowledged, they are too minute and various to have had much space allotted to their consideration in works on surgery. The following observations on certain points connected with the dressing of stumps have no pretension whatever to originality. They are simply a statement of views, which both reason and experience have induced myself, and I believe many others, to adopt.

*Sutures.*—If we divide sutures into two great classes, viz., Metallic and non-Metallic, the majority of surgeons of the present day—in amputation wounds, and most others where sutures are applicable—will probably be found to give preference to the former. In perfectly healthy tissues, union will generally take place so readily that the kind of suture to be used is not so much a matter for consideration; but when from any cause union is likely to be tardy, then it is a great desideratum to have one which may be kept in

the wound for a comparatively long period, at the same time causing the minimum of irritation and lesion of the tissues through which it is passed. This seems to be the very advantage which the metallic suture possesses over one of any organic metal.

I must beg to acknowledge the experiments—so conclusive as to the relative merits of these two classes of suture—made some time ago under the direction of Professor Simpson, of Edinburgh (vide 'Medical Times and Gazette,' January 1, 1859.)

Assuming, therefore, the general inferiority of the organic suture, I beg to offer some observations on the relative merits of the two chief kinds of metallic suture—viz., the common wire and the needle (or twisted) suture.

With a view to test their relative value, repeated experiments were made by myself and Mr. Gray our house-surgeon. In different patients, with different descriptions of wounds the twisted suture was used side by side with the wire suture, and the result in almost every case was this—that, at the end of a given time, union was more advanced and more satisfactory at the points where the twisted suture was employed, than at the other points. These experiments appear to me to carry some weight; for, when two kinds of suture, inserted simultaneously, are left side by side in the same wound for an equal length of time, there can be little room for fallacy in one's estimate of their relative efficiency.

The superiority of the needle to the wire is probably due to its greater firmness, enabling it to hold the edges of a wound in more exact apposition. The superiority of wire to thread on the other hand, must be attributed to its inertness from non-absorption of surrounding fluids.

An objection sometimes urged against the twisted suture is this—that often (in consequence of blood and lymph drying around the points of entrance and exit) its removal requires an amount of force which causes pain to the patient, as well as injury to the newly-formed adhesions. This evil may be prevented by a very simple precaution—well oiling the thread before it is twisted over the needle, and oiling it again (with a camel's-hair brush) once a day as long as the needle remains in. The needle may then be removed without any pain, or any injurious traction on the tissues recently united.

A very excellent kind of needle for the twisted suture is the common lance-pointed hare-lip needle, made by Weiss. It does not oxidize; it cuts easily, and is easily cut. I have suggested to Messrs. Weiss, as a matter of economy, to have them made at least double their present length, since about a quarter of each one (the flattened extremity) has to be wasted.

*Bandaging.*—In ordinary cases is it advisable to bandage the stump? I think not. The practice of bandaging the stump immediately after amputation seems to be grounded on the following suppositions: That it prevents retraction and involuntary twitching of the divided muscles, and (according to some) the entrance of air or deleterious fluids into the veins. Now let us examine what truth there is in each of these hypotheses.

1. As a matter of fact does bandaging prevent muscular retraction? My own observation convinces me that it does not. I doubt whether it can even retard retraction. Consider the case of an amputation of the leg or thigh. The immediate and primary retraction of the divided muscles takes place probably in the course of a very few hours. In the operation the whole thickness of the muscles has been cut across, therefore the whole substance of the remaining muscle contracts. Now any one, who knows any thing of the enormous strength of a single bunch of muscular fibres, will bear in mind how great must be the collective retractile force of the several muscles of a limb acting simultaneously. I am strongly inclined to think it is far too powerful to be counteracted by any amount of bandaging.

Again, even if it were possible to effect this retraction, would it be desirable? Surely not; for until the retraction has taken place, and the muscles have become quiescent, I do not see how Nature's process of repair can commence.

I have spoken of the immediate, primary retraction of the muscles. There

is a subsequent, secondary retraction, which begins generally about the second or third week after the amputation, and lasts a considerable time. It shows itself by an alteration to a variable extent, of the position of the cicatrix. In a thigh, for instance, the undue action of the flexors gradually pulls backwards the line of the cicatrix. Now, according to my experience, bandaging will no more prevent this secondary, than it will the primary retraction. The truth seems to be this,—that if a surgeon has left flap enough to allow for retraction, its occurence will be no mishap; if he has not left enough, no amount of bandaging will remedy his error.

2. As to the involuntary muscular twitching, causing the stump to start, this I have observed to occur just as much with a bandage as without one.

3. There is one more hypothesis to be considered, viz., that bandaging the stump may prevent the entrance of air or deleterious fluids into the veins from the wound.

This theory ignores the first principles of hydraulics and all that we know of the physiology of the veins, and the circulation. It assumes, first, that notwithstanding an atmospheric pressure of fifteen pounds to the square inch, the cut extremity of a vein may remain open, like a hollow tube, up to the nearest point where the current of blood begins. (We know that in certain localities a cut vein *will* remain open, its connexions being such as to prevent its collapse; but of these we are not now speaking.) Secondly, it assumes that the vein aforesaid has a marvellous power of suction, whereby it is able to imbibe fluid *from* the wound, *through* this tubular portion, *into* the current of blood beyond.

Now, if bandaging cannot answer any one of these ends, and has, further, the disadvantage of complicating the dressing, or concealing the stump from view, of getting constantly fouled by the discharge, of diminishing (when at all tight) the afflux of blood to a part which needs a very rich supply,—why bandage at all, unless œdema of the stump or some other complication render it necessary to do so?

In my own practice, and that of my colleague Mr. Hester, there have been at this Infirmary a considerable number of cases of amputation, in which primary union took place at all points, except those which gave exit to the ligatures, and good stumps resulted. In these the mode of dressing was as follows:—The wound having been secured with twisted sutures, a single layer of wet lint was placed loosely over the end of the stump, and kept constantly wetted for about a week; no other covering was applied. Then, as each suture was removed, a narrow strip of adhesive plaster was put to support the parts it had held in apposition, and changed as seldom as possible. No bandage or compress was used from first to last.

*Plaster.*—When it is desirable to support the edges of the wound with adhesive plaster, whatever may be the width of the strips towards their ends, I have found it better not to cut them more than half-an-inch broad at the part which is to be in contact with the wound. The advantages are (1), That the condition of the wound in the intervals between the strips is thereby constantly open to inspection; (2) There is the smallest possible area of contact between wound and plaster—a great advantage when the former happens to be irritable; (3) Free vent is left for discharge.

The position of the strips of plaster should be changed occasionally, otherwise the intervening parts from the unequal distribution of pressure, have a tendency to become unduly prominent. Attention to this will secure an evenly-rounded coutour to the stump; neglect of it will do the reverse, however well the stump may have been cut. This leads me, lastly, to notice how much it is in the power of the dresser to mould a stump, by making constant, steady pressure (whether by plaster or other means) on any point. If in any case, to use Mr. Paget's terse language, unremitting pressure will cause unr paired absorption, more especially has it a tendency to do this in a part where new tissue is in process of formation.—*Medical Times and Gazette, Sept.* 8, 1860, *p.* 230.

## 60.—ON TALIPES EQUINUS.

By Richard Wm. Tamplin, Esq., F. R. C. S., Surgeon to the Royal Orthopœdic Hospital.

[In the first part of his paper Mr. Tamplin makes the startling remark that out of the 20,000 cases of talipes equinus which have passed under his observation, not one was congenital. The most usual causes are irritation with disturbance of the nervous system, as from dentition, worms, and febrile affections; also from wounds, rheumatic affections, scrofulous disease of the bones of the leg. It also sometimes arises spontaneously, without any apparent cause.]

When talipes equinus arises from either of the three first-named causes, it is occasionally associated with paralysis or spasmodic contraction of the corresponding upper extremity, as shown in the statistical report. The majority of the cases, however, are those in which the muscles of the calf alone suffer by contraction. There are some in which the anterior tibial muscle is the only one paralysed. The balance of power being, under these circumstances, destroyed, the extensors appear to contract upon themselves, and thus produce the deformity.

Not unfrequently the first cause has been an attack of hydrocephalus, upon recovery from which the flexor muscles of the foot and toes are found to be perfectly paralysed or interrupted in their functions. I have in some cases had opportunities of witnessing the origin of the deformity. The history is as follows:—The parents of the child notice the frequent tripping and falling of the child from any slight impediment or irregularity on the surface trodden upon. They also notice a divergence of the toes, or, as they term it, increasing breadth of the front of the foot. If you request them to lead the child across the room in these cases, you will notice that the moment the opposite extremity to the one affected is carried forward beyond the perpendicular position of the body, the heel of the affected foot is perceptibly raised from the ground, and the toes unduly diverge from each other, by the increased pressure thrown upon the front part of the foot, and the inability to flex it, owing to the incipient contraction of the gastrocnemius muscle. If the leg be kept extended, it will be found, upon examination, that the foot can be forcibly flexed to its full extent; but, upon withdrawing the hand and setting the foot free, the gastrocnemius will immediately contract upon itself and draw up the heel. Upon making inquiry, you will generally find that the child is suffering from irritation consequent upon dentition, or from the presence of the small thread-worms.

In these cases, I have had the gums freely lanced; calomel, scammony, and jalap, administered for relief from the worms; stimulant embrocation applied throughout the whole length of the spinal column, and attention paid to the general health; applying to the foot tin splints, bent somewhat beyond a right angle, to counteract the tendency of the gastrocnemius to contract upon itself. I have in many cases followed up this plan of treatment for months; but, although the contraction was at its commencement the results have been unsatisfactory, except in a very few instances. Division of the tendo Achillis must be had recourse to, and then all tendency to contraction ceases, if propor precaution be used.

Some years since, Dr. Stromeyer, to whom we are indebted for the introduction of this new mode of treatment, visited this hospital, when a slight case of the kind just alluded to was placed on the table for operation. I consulted him upon it. He advised the use of the hot bath night and morning, together with general treatment, stating that he believed that would cure the child. This advice was followed, out of respect to so distinguished an authority, but with no satisfactory result. I afterwards divided the tendon, proceeded cautiously with the after treatment and succeeded in perfecting the cure.

These cases are instructive and important; for it appears that, although there is not decided and permanent contraction, yet the tendency to contract is permanent, and will inevitably produce complete equinus, if no efforts

are used to prevent it; and also that, as a rule, general means are insufficient. Within the last few weeks, I found it necessary to operate upon a child with this slight form of contraction, after having for six months tried general means without success. The temporary increased length given to the tendon by the operation in these cases, and the consequently weakened state of the gastrocnemius, although only continuing for a time—*i. e.*, until the tendon has resolved itself into the permanent condition—appears to admit of the anterior tibia exercising its full functions, thus restoring the balance of power between the flexors and extensors of the foot; at least, this is the only satisfactory inference I can draw.

*Irritation consequent upon a Puncture or other Wound in the Calf* produces at times an active and continuous contraction of the gastrocnemius muscle, which terminates in this deformity, without any apparent loss of power in the flexors of the foot, or any interference with the efforts of the will, so far as can be discovered. This does not show itself at the time the injury is inflicted, nor until the patient has to all appearance recovered from the immediate effects of the wound, attention being then drawn to it by the inability experienced to flex the foot. This also, if neglected, slowly and certainly terminates in complete talipes equinus, the inherent motion of the ankle-joint remaining unimpaired.

*Rheumatism* is the next of the causes mentioned as producing this deformity, as well as many other non-congenital deformities, as I shall have to mention. Contraction, when arising from this cause, is found to commence during the active stage of the disease, and seems to be produced by the instinctive efforts of the patient to keep the joint at rest. The gastrocnemius muscles are thus kept in a state of constant contraction, and the flexors of the feet, by the same efforts, are kept constantly relaxed, by which all motion is prevented, and the pain attending this affection is of course thereby mitigated or greatly relieved. The extensors become, therefore, permanently contracted, which is not discovered until the patient is able to stand or take exercise, when the heel or heels are found to be elevated, with total inability to flex the ankle-joint. In these cases, the integrity of the joint suffers by thickening of the synovial membrane, or greater or less alteration in structure, offering an obstinate resistance to the restoration of the natural position of the foot, even after the resistance of the contraction of the muscle has been overcome by the operation; and even after the foot is restored to its normal position, great difficulty is met with in the endeavour to restore motion, requiring the most unremitting perseverance and attention. The muscles or nerves themselves do not appear to suffer beyond the contracted state of the gastrocnemius, neither during the attack, nor at any period subsequent to it; for, after the removal of the contraction by operation, the voluntary powers over the muscles remain unimpaired. In fact, the deformity may be said to be produced by the exercise of those very powers, as we know the less a joint attacked with rheumatism is subjected to pressure, the less painful it is, and it is only by preserving the extended position of this articulation that this object can be attained, inasmuch as the astragalus is kept very nearly out of the articular cavity.

The next cause is *Scrofulous Disease in the Joint or its neighbourhood, or in the Muscles of the Calf or Tendons of the Leg*. Here, again, the instinctive efforts of the will are the remote cause, and the cicatrix or cicatrices the immediate cause. Many patients have applied with disease in the neighborhood of the ankle-joint, producing effusion into the joint by irritation or inflammation of the synovial membrane. The joint is kept by the patient perfectly passive in the extended position, hence, again, arises contraction.

In some instances, all the muscles surrounding the joint are kept in a state of active contraction, even with the very slight symptoms of disease: so much so, that upon examination you would be led to suppose that they were all permanently contracted, for you can neither flex nor extend the foot, which disguises the real nature of the case. This is also occasioned by the efforts of the patient to prevent motion of the joint in walking. A person recently consulted me respecting his son, a boy aged eight years, in whom

this condition of the muscles existed, with very slight swelling of the joint. Upon examination, no trace of motion could be discovered, from the extreme rigidity of all the muscles surrounding the joint. I ordered absolute rest, the leg to be kept in a horizontal posture on a couch—poultices and warm coverings to be applied to the limb. In five weeks, the muscles resumed their passive condition, rendering the cause perfectly evident, viz., disease in the joint.

It is important that this peculiarity in connection with the disease should be borne in mind, as at first sight you might be induced to divide the tendons, which would remove the rigidity of the muscles undoubtedly, but that effect would be but temporary; for as soon as the patient used the foot, the disease would again become a source of pain and irritation, and a second contraction inevitably result. It illustrates, however, one mode by which contraction of the muscles takes place, viz., that these constant instinctive efforts of the will, if not relieved early in the attack by local and general treatment, terminate in permanent contraction. Such a case occurred to me many years since, in a patient aged twenty years, who had suffered from childhood; and in this case I was compelled to divide every tendon surrounding the joint, with the exception of the flexors of the toes.

When scrofulous disease affects the joint itself, a permanently stiffened condition of the joint must be the result. You can, however, keep the foot at right angles during the time the restorative process is going on, and thus secure the position most favourable to the patient. I have had many cases (young patients) in which the most frightful disease existed, affecting the articulation, and in some the metacarpal bones, in which amputation had been recommended, all of which have yielded to treatment, and have been cured of the disease by rest, warmth, attention to the diet and general health, occupying, it is true, from two to three, and even four years. I mention this, in order that you may be induced to hesitate before you condemn the limbs of young patients to amputation, as is too commonly done; for, from an experience of years, I am convinced that it is an unnecessary step.—*British Med. Journal, July* 7, 1860, *p.* 515.

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61.—*A New Trephine.*—Dr. G. A. D. GALT, of Virginia, United States, has devised a new trephine, with the object of avoiding injury of the membranes or substance of the brain. The instrument consists of a truncated cone, with peripheral teeth arranged in a spiral direction, and oblique crown teeth. When it is applied, the peripheral teeth act as a cutting wedge, so long as counteracting pressure acts on the crown teeth; on removal of that pressure by the division of the cranial walls, its tendency is to act on the principle of a screw, but, owing to its conical form and the spiral direction of the peripheral teeth, the action ceases. Dr. Galt says that he has operated on the dead subject with it twenty times, and has never succeeded in wounding the membrane, although he endeavoured to do so. The editor of the 'New York Journal of Medicine,' in noticing the instrument, says that it has been examined by the members of the surgical section of the New York Academy of Medicine, and has, after repeated trials, been found fully to answer the design of the inventor. The instant the crown teeth cut through the internal table, all action ceases, and it is impossible to turn the crown.—*British Med. Journal, June* 16, 1860, *p.* 458.

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## ORGANS OF CIRCULATION.

### 62.—NEW MODE OF MAKING PRESSURE IN POPLITEAL ANEURISM.

By Dr. MOFFATT.

Dr. Moffatt thus describes an apparatus for making compression in a popliteal aneurism, which is a modification of one already employed by Dr

Fountain. "In a few minutes, with the aid of a carpenter, we erected a structure consisting of a stick of timber about four inches in thickness by eight in width, one end of which was secured to the top by an upright post of the same dimensions. This post was fastened firmly to the floor, and lashed to the iron cross-bar at the head of the bed. It was about six feet in height, and bevilled at the top to receive the stick first-named, these being firmly nailed together. The large piece of timber, about twelve feet in length, rested at the lower end upon a strong table placed at the foot of the bed, thus forming an inclined plane over the bedstead placed lengthwise beneath it. The patient was then placed upon the bed in the supine position, with his leg slightly flexed, somewhat everted, wrapped in thick layers of cotton, and placed in a long fracture box. A compress made of adhesive plaster wound tightly into a roll about an inch in length and three-eighths of an inch in diameter, was then placed over the femoral at the inferior angle of Scarpa's space. Upon this rested the lower end of a perpendicular piece of wood, about an inch square, the upper end of which was bevilled to meet the inclined plane before described. The pressure was commenced at eight in the morning. Its degree was regulated by drawing the upper end of the perpendicular down the inclined plane, to a greater or less extent as might be required. The hand of the operator was kept upon the stick, and thus secured an equable pressure, even though the patient moved his limb, as he sometimes did a very little. A second compress and upright were placed over the artery as it crosses the horizontal ramus of the pubes, and when the pain from pressure on one was too great to be borne comfortably, the other was used; and thus alternately pressure was kept up until five in the evening, when pulsation could no longer be felt in the tumour. Moderate compression was kept up for eight hours longer, after which the patient was kept for several days quietly on his bed. The great simplicity of this plan of compression, which is always available must be palpable to every one. That it is far easier for the patient, and unspeakably more so for the operator, cannot be questioned. It would also seem reasonable to infer, that the compression itself, made but at one point, is more uniform and certain than has yet been secured by any other method."—*American Med. Times.—Brit. and Foreign Med.-Chir. Review, Oct.*, 1860, *p.* 547.

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## 63.—ON THE TREATMENT OF ANEURISM FROM ANASTOMOSIS BY EXCISION.

By Oliver Pemberton, Esq., Surgeon to the General Hospital, Birmingham.

[The means of treatment selected in the hope of curing "aneurism by anastomosis" may be resolved into three well-marked divisions. These are:]

1. Endeavours to alter the intimate structures of the tumours, so as to render the further development of the arterial network well nigh impossible, and at the same time to promote its destruction and absorption.

2. The deligation of the principal arterial branches that are distributed within the tumour itself, or of those main trunks that immediately give off the branches that nourish it and pass about its locality.

3. The excision of the disease.

The first division comprehends pressure, the use of refrigerants, of caustic pastes, the injection of astringent fluids, and the introduction of setons and of needles with the view of exciting suppuration. Without, here, more particularly inquiring as to the value of compression, refrigerants, caustics, and the injection of astringent fluids, it may be affirmed that each one of these methods will prove of great utility in certain cases; these will, however, be found to be characterized generally by their comparative insignificance in size, and by their situation on the more solid textures of the frame. They appear to me altogether inadmissible in a case like that which I am about to relate, as their exhibition could only delay the attainment of the end in view. On the other hand, the introduction of setons, of numerous single

threads, of needles, and ligatures, with the view of exciting suppuration, or other means of treatment having similar objects to attain—all these will be eminently useful, even should they fall short of effecting a complete cure of the disease. The excitement of suppuration in all parts of one of these vascular growths cannot fail in changing completely the nature of its structure; so that should absorption of the now altered tissue be, from its size, a matter of improbability, at least one great danger—that of hemorrhage—is absent, and other steps to insure its removal can now be adopted in safety.

Of all the modes of treatment devised for the relief of this formidable disease, perhaps none have resulted in so great disappointment as that in which the ligature of either the main arterial trunks, or their branches supplying the tumour, has been accomplished. The return of blood into the disease in many instances occurred as certainly as the flow through the main channel was cut off, and where the only branch yielding a supply was apparently arrested, a new one, ere long, was found to have arisen near the same place. An exception, however, to this result, as will be seen, must be made in favour of those tumours situated in or near the orbit.

Of forty-five cases, in which the ligature was applied, either on the branches or the principal trunk, the following results were obtained:—

In ten of these cases the branches were ligatured. Of the patients who were submitted to this treatment not one was cured!

In thirty-five the principal trunk was tied. Of this number, fourteen were cured, ten died, nine were unsuccessful, and in two the result was imperfectly given. Further, in reference to situation, of these thirty-five, six cases were tumours in the orbit; of these, five were cured, and the sixth was reported as being imperfectly so.

It would appear, then, from these observations, that the ligature on the branches has altogether failed, and that where the principal trunks were tied, success was only remarkable when the tumours were within the orbit.

With such evidence of the danger and uncertainty attending this practice in the treatment of these diseases, under what circumstances, let us inquire, are we justified in resorting to it? In the first place, the tumour must be sufficiently formidable as to endanger the life of the patient, should its further increase be unarrested. Secondly, to check hemorrhage. Lastly, with the view of preparing the tumour to undergo, in comparative safety, the most certain of all remedies, (where its situation renders it practicable,) the excision of all the parts entering into its formation.

The excision of the structures constituting aneurism by anastomosis, seems to have impressed, in a very strong degree, the minds of those surgeons who first directed their attention to its relief. "You must not cut into it, but cut it out." This was the opinion held by John Bell, and he vigorously and successfully illustrated its value. J. L. Petit held the same doctrine, expressing it in the important caution that the incisions should be made in the sound tissues, where the bloodvessels were unaffected.

George Freer, who published his "Observations on Aneurism," in 1807, and who added a lustre to his connexion with the General Hospital in this town that has never been surpassed, by being the first to successfully tie the external iliac artery for the cure of femoral aneurism, also was impressed with the importance of the total removal of these growths with the knife. He records two cases, in which the disease was situated in the cheek, wherein the treatment by excision was attended by complete success.

More recently surgeons have been content to regard the operation by extirpation as the means of treatment that should be adopted when all others have failed. There are no records of late years, that I am aware of, of cases where this mode has been resorted to in the first instance; and yet there are many instances where such a course would have induced far less suffering, and would have placed even life itself in smaller peril. Thus Nélaton relates, that M. Mussey, for a relief of the fungus of the head, tied both common carotids, but was after all obliged to have recourse to excision, and cured his patient.

Dr. Warren's case also (American Journal of Medical Science, 1846) may be perused with advantage, as it shows that however necessary preliminary treatment may be ere incision can be ventured on in safety, it yet must come to it at last. The tumour, in this instance, was situated on the forehead, and was three inches in diameter: after repeated applications of needles and compressing ligatures to the arteries in the neigbourhood of the disease, together with the use of caustics, the cure had to be completed by excision.

This excision, or complete extirpation of the diseased structure, need not of necessity be carried out by the knife. Sir Benjamin (then Mr.) Brodie effectually accomplished it in the case of a tumor similarly situated, and which had resisted all previous efforts at cure, by means of ligatures so applied around steel needles, that complete strangulation was produced at its base. (Medico-Chirurgical Transactions, vol. xv., p. 177.) The situation, however, of the disease, must be the main guide to the selection of the mode by which it shall be thus entirely removed.

We are justified, then, in arriving at the following conclusions which may regulate our surgical treatment of these cases:

*a.* To cut out the parts containing the vascular structure as speedily as possible.

When the lips are the seat of the disease, this proceeding may, in some instances, be adopted, to the exclusion of all other modes.

*b.* If this excision cannot with safety be accomplished in the first instance, when the disease is otherwise situated, owing to the dangers of hemorrhage, or the prospect of too great disfigurement, the branches of arteries leading into the growth should be tied subcutaneously by needles and ligatures, whilst the intimate structure of the disease may be further altered by the introduction of numerous double threads, in the way I have described.

Should the situation be altogether unfavorable for the use of the knife, Sir Benjamin Brodie's plan of strangulation at the base should be adopted.

By whatever mode, however, the vascular character of the growth is modified, let this change only be desired in the hope that one or other of the means of extirpation I have mentioned may be carried out.—*Lancet, May* 26, 1860, *p.* 516.

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## 64.—CLINICAL REMARKS ON VARICES, &c., AND THE BAR-NEEDLE AND CLASP.

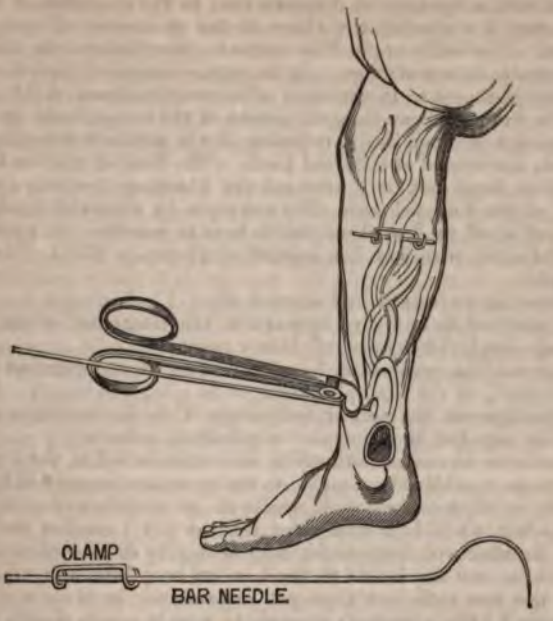
By JAMES STARTIN, Esq., F.R.C.S., Senior Surgeon to the Hospital for Diseases of the Skin.

[Shortly before Professor Simpson published his papers on acupressure, Mr. Startin, was experimenting as to the treatment of varicose veins. He used a tubular needle, through which, having passed it around and behind the vein, he introduced a strong wire ligature, and then, withdrawing the needle, left the wire in situ. Over this, a piece of gutta percha or metal was twisted so as to compress the vein and cause its obliteration. On the introduction of acupressure, Mr. Startin says,]

I immediately saw its adaptation to the end I was endeavoring to accomplish by the tubular needle, and forthwith constructed a needle from a No. 19 knitting-pin,* which I employed with such marked success as induced me to get the instruments which are before you, made by Messrs. Savigny & Co.

They consist of six or eight inches of No. 19, 20, and 21 steel wire, properly tempered, so as to bend and yet break short when required; one end being ground to a triangular point, and bent into a curved form of different sizes, to the extent of two or more inches of its pointed extremity, so that the needle represents a sickle with a long handle, the curved part being constructed to pass under and around the veins, the other straight portion or stem to form three or more bars. The point of this needle should project the sixth of an inch above the wire of the stem, and, as before said, the tem-

* *Any ordinary workman* can convert these needles into the bar-needle, the numbers to be employed *being Nos. 18, 19, 20,* and 21. They will afterwards require to be properly tempered.




per of the steel requires to be regulated so as to permit it to bend, and yet break short if pressed against any solid resisting body, as the pair of forceps also shown in the diagram. I have found some difficulty in obtaining the exact tempered steel required, so that were it not better to avoid a multiplicity of instruments, it might be preferable to direct the needle to be made soft enough to be divided at the required place by cutting nippers; if such cutting portion could not be added to the present forceps. These forceps, you will observe by the diagram, somewhat resemble the ordinary tooth-forceps, save that their descending blades are made to fit accurately by plain surfaces, excepting where two grooves cross them at acute angles, one of which grooves is to hold the needle for insertion beneath the vein—the other for withdrawing it when its point has passed the skin.

By reflecting on the shape of this needle, it will be seen that when its curvature has passed the vein, that its straight portion will compress the part to which it is applied with considerable force, which I have practically found quite sufficient to intercept the flow of blood through the vein and secure the obstruction of the vessel in all cases where much adipose tissue is not present, or where the vein is of medium size only; but, in the event of either of these occurrences, or indeed, in many instances, for additional security during the first ten or twelve hours, I have latterly used the second part of the contrivance, which I have called the Clasp. This consists of a piece of tinned iron-wire, varying in length and strength to suit the curvature of the needle and consequent length of the bar employed and left beneath the vein. One end of this clasp, as seen in the drawing, consists of a small circular ring or eye, and the other of a hook of corresponding size.

I have now, I fear not too intelligibly, described the Bar-needle and Clasp, a simple, yet, I believe, most efficient apparatus, which, if I mistake not, is destined to occupy as wide a sphere of utility in surgery as glycerine---an introduction of mine sixteen years ago—has held in therapeutics; for this needle is doubtless applicable to various other purposes than those indicated.

It may be mentioned that this instrument seems to be adapted to the treatment of aneurism by arterial compression; to the operation of the radical cure of hernia for closing the ring; to the treatment of many forms of nævus; and as a readily applied tourniquet, either before operations or after arterial wounds, or hemorrhage. In the latter instance I have once successfully employed it,—viz., in the case of a schoolmistress residing at Peckham, who is suffering from fungoid tumour of the breast, from which she lost a large amount of blood daily, reducing her to a hopeless condition; three bar-needles, introduced at different parts of the base of the breast, and the clasp applied, have not only arrested the bleeding, but have produced a sloughing of the diseased mass, thus conveying a hint which, when a more favourable case offers, I shall not fail to turn to account. I have also used the bar and clasp with success, instead of Professor Simpson's wire seton, in hydrocele.

Little more than a month has elapsed since I substituted this apparatus for the tubular needle and wire ligature in the treatment of varices, and I have already employed it in nearly thirty cases in public or private practice with a degree of success which has followed no other treatment with which I am acquainted, as the examples about to be cited will testify; but, to prevent disappointment, let me earnestly request those who adopt my suggestions, to take especial care that no complications exist in the cases selected; but that varicose ulcers and eruptions, and varicose veins, pure and simple, are alone to be considered amenable to the treatment recommended, and that any specific or other cachexy will render the procedure worse than useless. I must also beg it will be remembered that as yet I cannot decidedly pronounce on the effects of the bar and clasp upon very deep or chronic indurated ulcers, such as run the round of the London hospitals, and nearly environ the limb; nor has sufficient time yet elapsed, nor have my applications of the instrument been numerous enough to enable me to speak with much certainty, upon its ultimate effect upon varices, and very chronic varicose eruptions, with much cutaneous hypertrophy; such cases, however, will claim my early attention, and a record of the result in future clinical remarks.

The following is the method employed in operating with the apparatus above described:—The patient being placed in a good light, with his limb upon a chair, the bar-needle is to be seized by the forceps, near its curvature, taking care that its stem rests in the horizontal groove of their blades, its spare portion passing between the handles of the forceps so as to be grasped with them by the hand of the operator, as seen in the diagram. The point of the needle is then to be raised with the hand until it becomes nearly perpendicular to the spot at which it is to be inserted, when the needle is to be depressed so to occasion its curve to pass deeply behind the varicose veins and the smaller dilated plexus behind it, when a little pressure with the left hand on the place where the needle projects on the opposite side of the vein, will occasion its point to pass through the skin, when it is to be grasped by the forceps, and a little pushing with one hand and traction with the other, will bring the stem of the needle into the position it is to occupy, *i. e.*, within two inches or thereabouts of its blunt extremity, so as to allow half-an-inch or more to project on each side of the vein, as shown in the diagram. This portion of the needle now requires to be broken off, which is readily done if the steel be properly tempered, by taking the needle's stem between the blades of the forceps, half-an-inch or so from the point at which it first penetrated the skin, when pressure from the left to the right or from above downwards, made by moving the curved end of the needle, will break off the stem at the desired point, and leave the bar behind the vein with about half-an-inch projecting out of the skin on either side. The clasp is now to be applied, if required, (see diagram,) by passing its ringed extremity over one projecting side of the bar, carrying its middle portion, previously bent to fit, and yet gently compress the parts across the vein, when its hooked extremity may be passed behind the other projection of the bar, and the whole appara-

tus becomes firmly fixed like the pin in a brooch. By this procedure, of course, all circulation in the vein is arrested, and a coagulum forms on both sides of the bar, which may be rendered more or less permanent by retaining the bar a longer or shorter time.

In my first essays with the tubular needle, four, five, or six days were the periods adopted for keeping the ligature inserted; but laterally I have found fifty to sixty hours quite sufficient, whether in the case of an ulcer, eruption, or simple varix, though the last requires the longest insertion of the needle.

As the *modus operandi* of the bar-needle upon an ulcer, is to check very materially all secretion from its surface, so that it may be said to dry up, I have found the most suitable dressing, as it assists in this drying process, to be blotting-paper, which I fold into a thick compress and wet with water, and apply directly to the sore, the pad being large enough to cover all surrounding inflammation; this is secured by the domett bandage before mentioned, the bar-needle being covered with a piece of lint or adhesive plaster, and left between two of the turns of the bandage, which enables the clasp to be taken off the bar, without disturbing the dressings, should it cause pain after ten or twelve hours; and it is perhaps prudent, as a rule, always to attend to this point, as otherwise the wire may cut the skin, and make, while it cures, a wound, though this may be obviated by a pad or plaster under the clasp.

The blotting-paper dressing and the bar may remain forty, fifty, or sixty hours, when the needle must be taken out, and the ulcer dressed again as before described, taking care not to remove any portion of the paper which may adhere to the ulcer, as this forms an artificial scab, which favours the healing process.

If the case be a varix or varicose eruption only, all that will be necessary will be to apply the wet pad of blotting-paper over the site of the needle, and afterwards roll the limb with the domett bandage, which also may be put on wet with warm water, if pain or inflammation be present: and the eruption itself may be painted with the calamine and zinc lotion of the Hospital Pharmacopœia.

Any subsequent uneasiness in the limb or parts affected, may be moderated by passing a compress wet with tepid water around the bandage, and in very irritable patients the clasp may be required to be removed after a few hours. My own experience of this matter leads me to believe that as far we have yet gone, the treatment of varicose ulcer, pure and simple, is reduced from weeks into days by this operation.—*Medical Times and Gazette, May 26, 1860, p. 517,*

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## ALIMENTARY CANAL.

### 65.—A NEW METHOD OF OPERATING FOR HARE-LIP.

By Edward Woakes, jun., Esq. Luton. (Read before the South Midland Branch British Medical Association.)

[There are some inconvenience attending the ordinary operation for hare-lip; thus, the pins are liable to cause ulceration, and additional support is frequently required, to keep the edges of the wound together.]

The following plan has arisen out of a consideration of the desiderata which a successful operation for hare-lip demands. Briefly stated, these are—1, to support the opposed surfaces of the wound so thoroughly that any amount of contortion on the part of the patient will not interfere with the adhesive process: and 2, at the same time to secure so accurate an adjustment of the skin that when the cure is completed, there shall remain a minimum amount of cicatrix.

In the operation now proposed, these objects are accomplished in a very

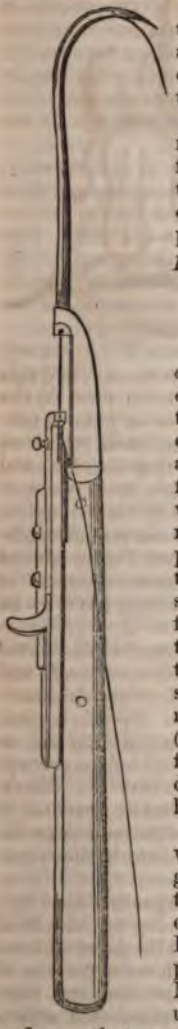
simple manner, by substituting silver wires and splints for the steel pins and silk in ordinary use. The wires being introduced, a splint of the size and shape of that represented in the drawing, is slipped along their projecting ends on each side by means of holes just large enough to admit the wires, and carried home to the cheek, at the spot where the wires were inserted. The ends of the two wires on each side are now twistedo utside the splint, which thus serves as a *point d'appui*, from which, at every additional twist, the approximation of the edges of the wound is made more perfect. The chief point to be observed is to insert the wires sufficiently far back to form a firm support to the wound; and this can be done far more safely with metallic wire than with steel pins, as the former, by adapting itself to the shape of the mouth, causes no painful tension, as do the pins when very deeply inserted. At the same time, so effectual is the support afforded by traction on the splints, that no additional aid for this purpose, such as adhesive straps, spring-trusses, &c., is requisite, even in the most formidable cases. By this immunity from tension, the tissues are enabled to display their well known tolerance of metallic substances, in consequence of which the wires may be retained for an almost indefinite period; whereas, from the oftentimes early commencement of ulceration when the less yielding pins are employed, these may have to be removed before union is complete.

The cut surfaces having been approximated as closely as may be considered advisable by twisting the wires, the line of incision remains still uncovered; and the second condition of the operation may now be accomplishedby the insertion of three or more delicate silk sutures very superficially. By this means, the most perfect apposition of the edges is obtained, and in consequence, as regards the final result, a very slight scar—in some cases all but imperceptible—remains. These sutures should be removed within twenty-four hours.

When the operation is complete, the appearance is much that represented in the accompanying drawing, where the actual size and shape of the splints are indicated. The following case may illustrate some undefined points in the operation.

*Case.*—W, M., aged 18 months, was the subject of congenital hare-lip; the cleft, which extended to just within the nasal orifice, being to the left of the median line. The usual incisions having been made while under chloroform, the wires were introduced about two-thirds of an inch from the line of incision, and made to pass out at a similar distance on the opposite side. This was done by attaching each wire to a piece of silk connected with a straight needle, sufficiently large to admit of the silk, with the wire attached, being drawn through after it. (This mode of introducing metallic wire answers perfectly well when the hollow needle for the purpose is not at hand, care being taken that, where the wire is bent down for the attachment of the silk, it is squeezed together with pincers that it may present as small an obstacle as possible to the passage of the wire at this its bulkiest part.) The ends of the wires to which the silk was united were then removed with scissors, and a splint passed alorg them, a twist being given to secure it. The other two were then similarly treated; and by alternately twisting the wires of opposite sides with a pair of ordinary pincers, the splints were first brought to lie upon the cheek, and then to approximate the edges of the wound. Three superficial sutures were now introduced, and the integument thus accurately adjusted. Adhesion took place rapidly, and in the course of twenty-four hours one of the superficial sutures was removed. The child,

which had not been weaned, sucked well, and appeared perfectly free from pain or uneasiness.



The following day, the other superficial sutures were taken away. On the fifth day, union appeared perfect, and the apparatus was removed; by snipping the wires of one side, and taking off the splint, that of the other with the wires attached was readily withdrawn.

The results of this and subsequent cases, as seen some months after the operation—the traces of the original deformity being less than in the most successful instances of the ordinary mode of operating—may I venture to think, claim for it the attention of those who are interested in plastic surgery.—*British Med. Journal, Aug.* 25, 1860, *p.* 666.

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66.—*On a Convenient Needle for the Ready Application of Metallic Sutures in the Operations of Cleft Palate, Vesico-Vaginal Fistulæ, &c.* By P. C. PRICE, Esq., Surgeon to the Great Northern Hospital.—Those surgeons who are engaged in the renovation of deformities arising from arrest of development—such, for instance, as the deficiency of the soft portions of the roof of the mouth in what is commonly known as cleft or split palate; or in the reparation of breaches of structure, as those resulting in permanent lesions of the female bladder, vagina, and rectum,—have often doubtless experienced the necessity of such mechanical instruments as would enable an easy performance of an operation. Improvements in such necessitous instruments have by degrees been added to the operator's *armamentarium;* but still every addition seems to show the further necessity that exists for yet more useful mechanical aids. Since the introduction of metallic sutures (so ably and forcibly recommended by Dr. Marion Sims) for the more efficient closure of the structures of the bladder, vagina, &c., the want of a suitable means of appliance has been more than ever needed.

The ordinary curved needle set in a handle, and drilled with a single hole near its point, is found inconvenient, for great difficulty is experienced in disengaging the metallic thread with which it is armed. To obviate this inconvenience, among other contrivances, a needle was invented by, I believe, a German, with a movable point, but still further perfection and simplicity was needed. A short while since, Mr. Startin, in the columns of this Journal, suggested the use of a tubular needle, which was a decided improvement, and one that was quickly recognised. Still, however, there seemed room for a more complete instrument to enable a free and ready application of the wire suture in cases rendered unusually difficult of management by reason of the position of the lesion, &c.

While lately witnessing one of Mr. Baker Brown's skilful operations for vesico-vaginal fistula, I was struck with the inconvenience that resulted from the method of applying the metallic suture (which, in this instance, required an extra pair of hands to arm the needle after its insertion through the soft tissues) by means of the ordinary grooved or tubular needle. To dispense with what I considered an impediment in these usually scientific and clever operations, I have devised the needle as shown above.

It consists of a tubular needle, such as at first suggested, I believe, by Mr. Startin, set in a wooden handle, but immediately connected therewith by a rod of metal, which is out of the plane common to both the needle and its

handle. Working on the upper surface of the handle is a steel slide with a forward and backward movement. At the front end of the slide is a catch-spring, regulated by a small screw-button. The arming of the needle is thus accomplished. The slide, having a moving range of an inch or an inch and a-quarter, is to be drawn as far on to the handle as it will go; one end of the metallic thread to be used is then inserted into the needle at what may be termed its breech, and pushed along the tube till it nearly reaches the point; the portion of the wire that projects at the breech is then placed in the catch-spring, and the needle is ready for use.

Having inserted the point, and as much of the needle as is necessary, through the lips of the structure to be closed, the wire is made to project beyond the point of the instrument (as shown by the dotted line in the engraving) by pushing forward with the thumb or forefinger the slide. The projected wire is now caught with a pair of forceps, and disengaged by pressing with the forefinger the button-head which regulates the catch-spring, so that it can be readily and quickly drawn from the needle, or the needle from it.

The instrument is depicted about two thirds its proper size, and was made for me by the well-known surgical mechanician, Mr. Matthews, of Portugal-street, Lincoln's-inn, and has been much approved of by Mr. Spencer Wells, and other surgeons who are in the habit of performing certain plastic operations on the perineum, &c. In cleft-palate, as well as in operations about the genito-urinary parts, this needle will be found very advantageous, and will at once render the passing of metallic threads through the curtains of the soft palate a less complicated proceeding. It has been my good fortune to have assisted for many years my friend Mr. Fergusson in nearly all his numerous operations for split palate; and although his master hand makes perfect in utility incomplete surgical instruments, still I have occasionally met with instances in which a more ready appliance for the application of the wire suture (which this distinguished surgeon now usually employs in his palate operations) would have been of much advantage.—*Med. Times and Gazette, June* 9, 1860, *p.* 574.

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## 67.—ON THE REDUCTION OF STRANGULATED HERNIA.

By Walter Jessop, Esq., Surgeon to the General Hospital and Dispensary, Cheltenham.

[Mr. Jessop publishes what he thinks a new method of reducing hernia. It is a *good* way, but not a *new* way. The case seemed almost hopeless: an operation was refused, and Mr. Jessop then adopted the following plan:]

Calling a male attendant into the room, I directed my patient still lying on his back, to the edge of the bed, and, with assistance, separated his legs, placing one over each shoulder of the attendant, who, facing the bed, stooped to receive them; and, in this position, by passing his hands round to the fore part of the thighs, was enabled to obtain a sufficient purchase to permit of his raising him on to his head and shoulders on the bed, thus throwing the intestines back upon the diaphragm, and to some extent necessarily making traction behind and directly *from* the seat of strangulation. After two or three minutes' manipulation of the abdominal parietes, I found the tumour become less tense, and drawing forwards the integuments round the point of rupture, I made lateral, upward and downward movements—jerking, as it were, occasionally the parts immediately contiguous to the stricture. This seemed to excite but little suffering; in fact, the patient, so far from uttering complaint, declared himself, after the first two or three minutes, decidedly relieved—that "the dead sickening weight that killed his groin," as he termed it, was better. Continuing these efforts, and varying them as they seemed to occasion distress, I presently felt a slight gurgling under my hand, and almost immediately had the satisfaction of finding the hernia reduced, and my patient comparatively in a state of safety.

The whole proceeding certainly did not occupy ten minutes. Slight peritoneal tenderness existed for some days, but the man eventually did well.

The *rationale* of the proposed plan is simple. A mass, large or small, of displaced intestine or omentum must assuredly be more readily withdrawn from its point of incarceration or strangulation by traction from behind than by the best-directed efforts of the taxis. Any one, for illustration, taking the trouble to put a fold or two of his handkerchief in a ring formed by his finger and thumb, and lightly strangulating it, will, on attempting to return it by pushing or kneading from before backwards, find infinitely greater difficulty in effecting his purpose than if he were to make traction from behind. In short, the employment of the taxis is at the best a clumsy and most uncertain mode of proceeding, and I shall venture in future to make it merely supplementary to the plan I now advocate.

"One swallow fails to make a summer," and it may be said that the practice of turning patients *a posteriori* upwards is opposed to all orthodox notions of propriety. Admit all this. Others, with greater opportunities, may happily be enabled to add to my single case; and granting that the position of the patient may be accused of positive inelegance, it may, at any rate, contrast favourably with our proceedings in lithotomy and in many other operations on the perineal regions.—*Lancet, Oct.* 29, 1860, p. 384.

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## 68.—ON THE USE OF CHLOROFORM IN STRANGULATED HERNIA.

By Thomas Bryant, Esq., F.R.C.S.E., Assistant Surgeon to Guy's Hospital.

[Mr. Bryant has the greatest confidence that in the majority of early cases of inguinal hernia, the hernia can be reduced when the patient has been thoroughly brought under the influence of chloroform. But to effect this, the patient must be rendered completely mastered by the anæsthetic, and a perfect coma, or even stertor, produced, if necessary.]

The following are a few of the cases which have passed under my notice, but they are accurate examples of all others.

A man, aged 41, was admitted under my care into Guy's hospital, on Aug. 3, 1857. He had been the subject of a right scrotal hernia for four years, and had never worn a truss. The hernia had been down twenty-four hours, and symptoms of strangulation had existed during the whole of that period. The taxis had been freely and efficiently applied without benefit, and I was consequently sent for to operate, as no other measures were deemed justifiable. Chloroform was administered, and everything prepared for operation; when, however, the patient had been completely brought under its influence, the taxis was re-applied, and without the slightest force the hernial contents returned into the abdominal cavity; a good recovery ensued.

A man, aged 29, the subject of a congenital hernia of the right scrotum, was admitted under my care Sept. 4, 1859. Symptoms had been present for two days, and were very violent. He had been operated upon nine years previously for hernia on the same side. A warm bath, opium, ice locally, and the taxis had been employed without the slightest benefit, and as an immediate operation was indicated, I was sent for. I found the man suffering severely, and the scrotal tumor was very large, tense, and painful; as it was most desirable to reduce the hernia without operation if possible, I applied the taxis, but without success; chloroform was accordingly administered preparative to operation, and when the patient had been thoroughly brought under its influence, the taxis was re-applied; not the slightest force was, however, employed; the hernia contents at once returned into the abdominal cavity, and a speedy convalescence followed.

Many cases like the two quoted might be given, all illustrating the same point, but I believe these are quite sufficient to indicate the benefits of chloroform in the reduction of a strangulated hernia.

I have before me twelve good examples illustrating the benefit of such a practice, all were, however, inguinal. The reduction of a strangulated fem-

oral hernia by the taxis is by no means common; that is, success cannot be anticipated to the same extent, as it may be in an inguinal hernia. In a paper which I published in the Guy's Hospital reports for 1856, I showed that femoral hernia required operation 25 per cent. more frequently than inguinal, and subsequent experience has convinced me of the force of such a truth. I also showed that in femoral hernia the taxis is more injurious, and is more frequently followed by gangrenous bowel, such points are worthy of remembrance, as a caution to the surgeon to employ his taxis with discretion and not too forcibly: the administration of chloroform in such cases is beneficial, but the caution as just given should be practically remembered, for force is not indicated, and must do harm, whether employed with or without chloroform.

Acknowledging it, then, as a fact that there are many cases of strangulated hernia which may be returned by the taxis, when the patient has been fully brought under the influence of chloroform; and remembering also that such a practice is demonstrated the most perfectly in those cases in which all other means had entirely failed, and when under ordinary circumstances an immediate operation would have been called for; it may be well to consider how far such a fact should influence our practice, and in what way we can turn such information to account.

A little consideration will soon lead us into the right path; for if it be true that in chloroform we have the most effectual means for producing relaxation of all the muscles of the body as well as of the parts involved in hernia; and if it be also acknowledged that by such means a patient is more speedily and more effectually brought into that relaxed and desirable condition than by any other measures which we have been in the habit of employing, the conclusion to my mind is tolerably obvious, that in all cases we should at once employ the most certain and most efficacious practice, and neither waste time by delay, nor prostrate the powers of our patient by the use of other remedies when we have before us one so incomparably their superior.

The warm bath is doubtless a valuable remedy, and the cases are numerous in which the taxis has succeeded under its influence, which had without entirely failed; but it is nevertheless depressing in its nature, and the depression lasts beyond the period in which such depression is necessary or desirable. The depression associated with a strangulated hernia is very lasting and often very great, and to encourage it by any artificial means cannot be regarded as good practice; and I feel confident that the depressing influence of a hot bath to patients labouring under such a disease as hernia, has often acted most prejudicially to the subsequent success of the case.

Opium, again is a remedy which cannot be too highly praised, and when given in full doses, and repeated, to bring the patient, who is the subject of a hernia, rapidly under its influence, its useful tendency has been too often manifested to allow us to depreciate its value; but if a surer and more expeditious remedy can be brought forward, and if it can be shown that by the use of chloroform the ends which we have been in the habit of seeking can be attained with greater certainty and without that loss of time which must necessarily take place by internal remedies, and without that lasting depressing influence which is exerted by the hot bath, I think few surgeons will dispute the conclusion, that the administration of chloroform should be primarily selected.

I will not consider the local applications of ice to the hernial tumour, as the cases we are discussing are those of strangulated hernia and not incarcerated, in which the symptoms are marked and not to be mistaken, and in which it is hoped that no surgeon will ever more allow valuable time to pass away by the use of such a precarious and uncertain measure.

When strangulation of the intestines has taken place, it cannot be too often repeated that the more speedily that strangulation is relieved the better; this truth is so recognized by the profession that it may appear too ~~ste~~ for repetition; but let me ask the experience of any hospital surgeon if is not exceptional for such a truth to be carried out. The cases which

appear in hospital practice are, as a rule, such as have been badly treated, and have been most unwarrantably neglected; four, five, six, seven, even ten or twelve days' strangulation is the rule at Guy's Hospital, and some surprise is generally expressed when a case of shorter duration has been admitted. The taxis, opium internally, and ice externally, have been often employed in no unmeasured proportions; and purgatives, even in these days of advanced knowledge, are not exceptional; and it is to prevent such malpractice that I believe a simpler practice would prove most valuable, as timid practitioners would not allow time to pass away by the use of remedies which although of value, are valuable only in select cases, and which as a rule are not to be recommended.

When, then, a strangulated hernia has taken place, what practice should be carried out?

If quite recent, and the symptoms are not severe, a warm bath, a full dose of opium, say two or three grains, and gentle taxis will generally succeed, and in such cases, and such alone, should they be employed.

In others when the hernia has existed for some hours or days, when the vomiting and other symptoms are so marked that there can be no doubt that strangulation of the bowel exists, and not incarceration, I would suggest that the immediate administration of chloroform should be resorted to; let the patient be brought completely under its influence and we may then expect, with considerable confidence, that its reduction by the taxis will rapidly follow, particularly if the hernia be inguinal; the force which is required under such an influence, is simply nominal, anything like power would prove injurious, and would be most certainly unwarrantable; the most moderate application of force is sufficient, if success is to be obtained; and if it should fail no extra power will be more beneficial; let then the taxis be of the most moderate description, let it be firmly and decidedly applied; but do not allow any thing like force to be exerted; if success is to follow its application, moderation will suffice; and if failure, force will not succeed.

If failure follows, let the operation be performed; there is no object to be gained by delay, but irreparable mischief must take place, and if the sac is not opened in such cases the operation is not one of danger.

The evils of the taxis, and of delay, as occasioned by carelessness, and the use of remedies, are of such a nature, and have made such an impression on my mind, that I firmly believe that it would be better for all cases of strangulated hernia to be treated at once by the administration of chloroform followed by the gentle taxis, and on this failing by the minor or other operation, than that any chance of delay should be occasioned by the use of remedies, or of injury by the taxis; and that if practitioners would discard all former measures, and adopt those which I have suggested, after experiencing their advantage, I feel sure that the cases of death from strangulated hernia would be but rare, and the cases of mal-treatment would be less numerous.

For if men would only be convinced that by the use of chloroform we have all the advantages of every plan of treatment which has been formerly employed, without any of their evils; that under its influence, if the taxis is to be successful, success will be attained, and free from the danger which attends its use without the anæsthetic,—as the required or permissible force should be but slight; and if they will only be impressed with the truth, that if such means fail, an immediate resort to operation is imperatively demanded, I feel certain that this improved practice would be followed by commensurate success, and that the mortality of our hospital cases would be materially diminished.

Since the introduction of chloroform the treatment of dislocations has been completely altered, and the means employed for their reduction are quite simple, when compared with those which were formerly resorted to, viz.: pulleys and personal strength.

By the use of chloroform in the treatment of hernia, an improvement equally important is now open to us, and I shall be much suprised if the re-

sult does not prove as favorable as I am sanguine enough to believe it will. In both classes of cases the evils of force are dispensed with, and its danger corrected; and in both the influence of science and of manipulative power are well exemplified. But in hernia there is this additional advantage; the delay occasioned by remedies will not be experienced; and I feel convinced that if surgeons will but recognise the value of chloroform as a means of reducing a strangulated hernia, the evils resulting from delay and forcible taxis will be done away with, and a more successful practice must be the result.—*London Med. Review, August*, 1860, *p.* 69.

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## 69.—ON ULCERATION OF THE LOWER EXTREMITY OF THE RECTUM; ITS VARIETIES, DIAGNOSIS, AND TREATMENT.

By James Rouse, Esq., Assistant Surgeon to the Westminster Ophthalmic Hospital, &c.

Notwithstanding the numerous works published on diseases of the rectum during the last few years, there appears still to be great difference of opinion as to the best mode of treatment; more particularly with regard to those ulcerations situated on the mucous membrane lining the sphincter ani, and in the fossa immediately above that muscle.

There are three forms of ulceration of the lower extremity of the rectum, which gave rise to very acute suffering; and, although they vary considerably in position, have nevertheless been described by most authors under the general head of fissure. It is proposed, in the present paper, to point out that three distinct forms of ulceration occur in this region, which, by ordinary investigation, may be distinguished from each other, and which require different modifications of treatment.

The most common form of ulcer found at the lower extremity of the rectum is that which is known as *fissure of the anus*. This disease does not seem confined to any particular period of life, though it rarely or ever exists until after puberty. It is more particularly common among persons who lead a sedentary life, and for the same reason it is rather more frequent in women than men. The fissure appears to be caused by a tearing of the mucous membrane lining the sphincter ani, by the passage either of hardened fæces, or of a foreign body contained therein. The following cases will, however, show that fissure of the anus may occasionally be the result of external violence:

*Case* 1.—A gentleman, aged 24, was riding a restive horse, when it suddenly bolted. He was thrown, with some violence, on the hind part of the saddle before he recovered his seat. He felt some pain about the anus at the time, and, on changing his shirt, he noticed a few drops of blood. For the next few days he experienced a slight burning pain during the evacuation of the bowels, and in about a week the characteristic pain of fissure was established. On an examination being made, a small crack was perceived on the posterior surface of the sphincter; it commenced about two lines within the anus, and extended upwards for about half an inch. Various local means were tried without benefit, and an operation to be hereafter described, was had recourse to with perfect success.

*Case* 2.—A captain in the navy fell off a ladder, and came to the ground on his buttocks, with considerable force. He did not observe any particular pain until he went to stool the following morning, when he experienced considerable smarting, and noticed that he had passed a small amount of florid blood. About a week after the accident, he applied for advice. He then, after every evacuation of the bowels, had pain, which lasted for several hours. On examination, an ulcer was found on the posterior surface of the lining membrane of the sphincter; the edges were not indurated, and the surface was florid. An ointment, containing mercury, was applied twice a day; and *in the course of a week* a cure was effected.

*Persons afflicted* with this disease, in describing the origin of their suffer-

ing, frequently state that while straining violently at stool they felt something give way, and on looking at their evacuations, they noticed a small quantity of blood. It has more than once occurred to me to be told by persons with fissure that the fæces were so hard that it was necessary to remove them with the fingers from the anus.

This crack or fissure is almost invariably situated on the posterior surface of the sphincter. I have seen upwards of a hundred cases, and in only six did the position vary; in three of these the fissure, was situated on the perineal surface of the muscle, and all occurred in women; in two it was situated on the left side; and in one on the right. It commences about three lines from the margin of the anus, and extends upwards in a straight line to the extent, usually, of half an inch, though sometimes as high as the superior margin of the sphincter. If the fissure be seen within a week or ten days of its occurrence, it presents the appearance of a bright red line with a sharply defined edge, and does not appear to extend through the thickness of the mucous membrane. A little later, if no treatment be adopted, one or two florid granulations may frequently be seen protruding above the margin; and it is during this stage that a small amount of blood is voided on going to stool. This appearance is very soon changed; the edges become everted, and more or less hard, and the surface of the ulcer itself looks excavated and pale, like any other indolent sore. The pain caused by this solution of continuity is at first trifling and only exists while the motion is passing; but it soon becomes most severe. It usually commences about half an hour after the bowels are relieved (the sensation up to that time being only uneasiness), and continues for five or six hours. As the disease progresses, the pain becomes more continuous and easily excited, and even walking or sneezing will bring it on. At this stage, the ulceration is found to have extended through the submucous cellular tissue into the fibres of the sphincter; there is a constant desire to pass urine, a serious addition to the other suffering, and this continues until relief is obtained by means of an operation.

This second form of ulceration is situated immediately in front of the os coccygis, and was first described by Sir B. Brodie, in a clinical lecture delivered at St. George's Hospital. This ulcer, which is almost invariably coexistent with an enlarged and varicose state of the veins about the rectum, does not, like the one just described, appear to be caused by a tearing of the mucous membrane, but by an injury done to it or to the submucous cellular tissue. It seems as if the forcible passage of the fæces (in these cases always very much hardened), and the pressure caused thereby, produced a small slough of the mucous membrane. Mr. Quain states, in his recent work 'On the Diseases of the Rectum' that he has noticed a case in which, "the disease having been of no long duration, and the suffering comparatively slight, the membrane appeared to be thinned from beneath." The ulcer, once formed, soon increases in size, and usually remains quite superficial for a considerable time; but at length, from the continual irritation, the edges become everted and hard. The surface, however, seldom becomes so indolent as in cases of ordinary fissure; and in this form of the disease the ulceration seldom if ever, implicates the fibres of the sphincter ani. The pain which, as in fissure, is caused by the evacuation of the bowels is most intense; there is usually very little spasm of the sphincter, but the patient complains of severe lancinating pain, which gradually subsides into a sensation of burning, which continues for three or four hours.

The third form of ulcer is situated in the fossa which exists between the external and internal sphincters; it is by far the most painful and serious affection of the three. It appears to be caused either by the lodgment of a small portion of hardened fæces, or by injury done to the mucous membrane in that situation by the passage of some foreign body, such as a fish-bone. Two cases are known to me where the presence of a polypus of the rectum (the extremity of which was pressed into this fossa every time the bowels were relieved) caused an ulcer in this position.

The ulcer, at first, is seldom *more* than the eighth of an inch in diameter, and it is *generally somewhat deeply* excavated. As the disease progresses,

the ulceration extends into the substance of the sphincter ani; so that, when the finger is passed into the rectum, the end of it sinks into a small cup-like cavity, the inferior part of which is formed at the expense of the superior margin of the sphincter. Except in cases of long standing, the edges are not indurated, and the surface almost invariably remains florid. In this disease a certain amount of pus and blood is passed at each relief of the bowels. If this ulcer be not cured by means of an operation, it leads to a most troublesome form of stricture of the bowels. The constant irritation, set up by the action of the bowels gives rise to inflammation of the submucous cellular tissue; this causes thickening and hardening, by which means the calibre of the outlet is seriously diminished.

The following case will illustrate this kind of termination:—

*Case* 3.—Mrs. S., aged 23, complained of very severe pain before, during, and after the relief of her bowels. She had consulteda s urgeon, who, on examination, found an ulcer immediately above the external sphincter. An incision was made through the ulcer into the tissue below; but this did not produce the slightest relief. Six months after the operation, she noticed that the discharge was much increased in amount, and she found more difficulty in passing her motions, which were small and flattened. A year subsequently to the operation, I saw her, and, on examination, discovered an ulcer of considerable size situated on the posterior surface of the rectum, and involving the superior margin of the sphincter, and such extensive thickening of the submucous tissues that the finger could not be passed through. Subsequently, by means of bougies, considerable benefit was obtained.

In these cases, the pain complained of is most severe, and there is more spasm of the sphincter than in simple fissure: in some of these cases the amount of spasm is so great that the muscle increases considerably in size. The pain appears to commence some little time before the bowels are relieved, probably this is caused by the pressure of the loaded bowels upon the ulcer.

*Diagnosis.*—The diagnosis of these cases is by no means difficult. The peculiarity of the pain complained of, the fact of its coming on either during, or soon after, the action of the bowels, and the ease with which these ulcers may be detected by the finger, when it can be introduced into the bowel, render a mistake almost impossible. There exist only two diseases with which these ulcers may be confounded: to wit, a syphilitic ulcer and spasmodic contraction of the sphincter. Neuralgia in the neighbourhood of the sphincter has such well-marked symptoms of its own, that it can scarcely be mistaken. With regard to the syphilitic ulcer, its characteristic appearance, the class of persons affected, the existence of syphilitic ulceration about the vagina, remove all doubts as to the nature of the complaint. The diagnosis between spasmodic contraction of the sphincter and fissure is rather more difficult; in fact, it is only by a most careful examination that the surgeon can determine whether an ulcer exists or not. There are, however, a few points of difference which it would be well to remember. In spasmodic contraction of the sphincter, the muscle very rapidly increases in size; the anal orifice becomes so contracted that even a gum catheter cannot he introduced without producing extreme suffering. This amount of spasm is most rare in ulceration, and it is the pressure caused by the finger on the ulcer itself that produces the pain. Again, in ulceration, it matters little in which form, sooner or later, there is always discharge of pus and blood; in spasmodic contraction this never occurs. Lastly, the patient having been placed under the influence of chloroform, a careful examination of the bowel can be made (which it is impossible to do without producing insensibility,) and, as in the following case, no ulcer is found to exist.

*Case* 4.—George ——, aged 45, a man of spare habit, sallow complexion, and depressed vital powers, complained of intense pain, which occurred during the time the bowels were acting, and for several hours after. The pain was not continuous, but came on in paroxysms every few minutes. The motions were very small and flattened; but there was no discharge or appearance of blood. On examination, the sphincter muscle appeared more developed than usual, and the anus was so contracted that it was impossi-

ble to introduce the finger. A speculum ani was employed, and the most careful examination failed to discover any ulcer. Under these circumstances, a small bougie, about six inches long, was introduced every other night. At first the pain caused was very great, and he was unable to retain it for more than three or four minutes; but he was soon able to bear it for a longer time. The size of the bougie was gradually increased, and he was ultimately cured.

*Case* 5.—A gentleman, aged 35, of spare habit and nervous temperament, had suffered with symptoms like those just described, for six months, and the pain had become so severe that he could not take exercise; he had tried various means to obtain relief without success. The most careful examination failed to discover any ulcer, but the sphincter was immensely hypertrophied. Bougies were employed for two weeks without producing the slightest relief, and the patient was so worn out and irritated by the pain he suffered, that he could not be induced to continue the use of them. It was therefore decided to divide the sphincter, and with the exception of the pain produced by the passage of the fæces through the wound, this patient never suffered any inconvenience afterwards.

I should not have insisted so strongly on the existence of this disease, but one of the most recent writers on diseases of the rectum doubts the existence of simple spasmodic constriction of the sphincter.

There is one other precaution necessary in these cases; and that is, to be quite certain that only one ulcer exists. It is not very unfrequent to find two; they may be either one above the other, or situated on opposite sides.

The treatment required for the ulcer in front of the os coccygis, and for fissure, varies according to the stage of the disease. If it be treated before it has become indolent, local applications, and attention to the state of the bowels, are all that is necessary. Grey oxide of mercury and spermaceti ointment (half a drachm to the ounce) or a scruple of calomel to an ounce of lard, with ablutions night and morning and after each relief of the bowels with yellow soap and water, will usually effect a cure. Great care must be taken in the choice of a laxative, the object being not to purge, but to render the fæces soft, so that as little stretching as possible of the ulcer should take place. Confection of senna or milk of sulphur generally produce the desired effect. A very common medicine in these cases is confection of pepper; this, combined with confection of senna, is very useful in cases of hemorrhoids, but it is apt in all cases of ulceration to produce considerable aggravation of the patient's suffering. When the ulcer has once become indolent, the best and only treatment (likely to prove beneficial) is by the knife. The operation is best performed in the following manner. The patient being placed on the right side, with the knees drawn up to the chin, the forefinger of the left hand is to be introduced into the rectum, and the knife passed up in front of it; the incision is then to be made, commencing a few lines above the superior margin of the ulcer, and to be carried through it down to the external skin, care being taken not to cut into the fibres of the sphincter, except in those cases where the disease has already involved that muscle. After the incision has been made, a small piece of oiled lint may be introduced into the wound. It is better not to allow any action of the bowels to take place for two or three days after the operation; this may be effected by giving small doses of opium or a milk diet.

The treatment required for the ulcer situated above the sphincter is division of the muscle. Local remedies never appear to afford the slightest benefit, but only tend to wear out the patience and spirits of the sufferer. The operation is to be performed in the same way as for fissure; but, instead of merely making an incision into the submucous tissue, the sphincter must be divided by one cut, the wound is then to be dressed in either with oiled lint or silk.

It is of course always prudent to try local means before proceeding to an operation; and the best application is the ointment of grey oxide of mercury, already mentioned. The most satisfactory method of applying the remedy

is by means of a suppository tube. The tube should first be lubricated outside, and then filled with the ointment; it is then to be passed into the bowel to the extent of an inch or an inch and a half, and the piston then pushed down; by this means the entire surface of the mucous membrane lining the sphincter is covered by the ointment.

Some surgeons recommend the application of nitrate of silver for these forms of ulceration; but it seldom proves very beneficial, and the pain it causes is quite as severe as that of the division. If it be attempted, a speculum should be introduced into the bowel; by this means, the ulcer is brought into view, its surface should be dried by a piece of sponge or lint, and the caustic freely applied.—*British Med. Journal, May* 12, 1860, *p.* 356.

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## ORGANS OF URINE AND GENERATION.

### 70.—ON INFLAMMATION AND ABSCESS OF THE PROSTATE GLAND.

By R. H. Meade, Esq., F.R.C.S., Senior Surgeon to the Bradford Infirmary.

[Mr. Meade has given us below several very interesting cases, not only to remind us of an operation performed by the late Mr. W. Colles, of Dublin, but to show that such operations as make a communication between the urethra and rectum, are not to be dreaded.]

*Case* 1.—Towards the end of the year 1854, I was consulted by Mr. H., a man about thirty years of age, on account of a troublesome stricture which had existed for several years, and was then causing considerable disorder of the general health. Upon examination I found a tight constriction in the membranous portion of the urethra, through which a small bougie passed with difficulty. I also noticed a cicatrix in the perineum, and learned that some time previously, he had had a fistulous opening in that situation, through which urine had escaped, but which had gradually healed up. At a short distance from the scar, the patient said he often felt a lump, which disappeared on pressure; and I here found a deep-seated, hard substance, about the size of a hazel-nut, which was evidently a small cavity, communicating with the urethra by a blind fistula. He said that when he made water tolerably freely he felt nothing of this; but when the symptoms of stricture were aggravated, the swelling became full, tense, and painful.

By the careful introduction of bougies during a period of many weeks, I succeeded in curing the stricture, so far that a metallic instrument of No. 10 or 11 size would pass freely into the bladder; and I taught the patient to pass one for himself, directing him to do so regularly once or twice in a fortnight, to prevent the return of the contraction of the canal.

Though he now made water freely, he still complained of very uneasy sensations about the neck of the bladder, in the perineum, and also in the rectum, where he described symptoms similar to those produced by stricture of the gut. The little swelling in the perineum also remained, and he occasionally noticed a purulent discharge along with the urine. To remove this blind fistula, which I hoped might have closed as the stricture was cured, I thought it best to cut into the tumour in the perineum, and form an external opening; on doing so I found, as I expected, a fistulous passage, along which I could pass a probe for some distance. After this little operation, urine escaped from the opening for a week or two, but the introduction of a probe coated with nitrate of silver, and passed deeply into the sinus, quickly caused its permanent closure.

Upon examination of the rectum with the finger, I found the passage of the bowel impeded by a strong membranous band stretched partly across its anterior surface, at a point opposite to the situation of the prostate gland; which organ appeared much thinned, so that a bougie when passed into the bladder could be felt more distinctly than usual in the rectum. Upon questioning the patient, I found that some time previously to his consulting me,

and at the same time that the first fistulous opening formed in the perineum, he had suffered from symptoms of inflammation and suppuration of the prostate; and matter had evidently formed and insinuated itself in various directions, finding one outlet in the perineum, and another into the rectum, after burrowing between the coats of the bowel, and producing inflammation and the formation of the membranous band which I have mentioned. Great portion of the glandular substance of the prostate seemed to have been destroyed, and from the uneasy feeling which still remained about the neck of the bladder, accompanied with occasional discharges of pus, and also from a boggy feeling which existed in the centre of the gland, it seemed very probable that some chronic fistulous passages still remained in the prostate and its neighbourhood.

Before adopting any further operative proceedings for his relief, I wished my patient to consult Mr. Teale, of Leeds; and he suggested that the prostatic portion of the urethra should be laid open into the rectum, an operation which had been recommended and adopted in somewhat similar cases by Mr. W. Colles, of Dublin, some years ago, and which also had been tried by Mr. Teale himself in several instances with success. My patient being anxious to undergo any operation that we could suggest for his relief, I at once recommended him to have this procedure carried out. I introduced a good-sized grooved staff of the ordinary curved shape into the bladder, the groove of which could be felt very plainly in the rectum. The patient lying on his side, and the staff being held firmly by my assistant, I then passed a slightly-curved sharp-pointed bistoury (the lower part of which was guarded by some lint) into the bowel upon my forefinger, and thrust the point of it through the centre of the prostate into the groove of the staff, above the membranous band which I have described. I then drew the knife downwards in the groove, divided this band, and laid open the prostatic portion of the urethra into the bowel for about half or three-quarters of an inch. There was very little bleeding from the wound, and no urine appeared to escape from it. The patient was convalescent in a few days, and the operation was completely successful, removing both the obstruction of the bowels and the irritation about the neck of the bladder.

I have seen Mr. H. several times during the last three years, and he continues well, with the exception of being obliged to pass a bougie occasionally. On examination of the rectum with the finger, the gut is quite free from obstruction.

*Case* 2.—John A., aged 41, was admitted into the Bradford Infirmary on November 15, 1859, with stricture of the urethra complicated with fistula in perineo, and recto-vesical, or rather recto-urethral fistula. His health was very much broken down, and for several month she had been in a very uncomfortable state; as, whenever he attempted to empty the bladder, the greater portion of the urine escaped by the rectum and through the opening in the perineum. By inquiry into the history of the case, it seemed that about a year before the patient's admission into the Infirmary, he had suffered from symptoms of abscess in or around the prostate, which were relieved by the matter finding its way into the rectum, and were succeeded by the escape of urine from the bowel. About three months subsequently to this, a gathering formed in the perineum, which was opened by a country surgeon, and was followed by a fistulous opening, through which urine also escaped. On the introduction of a bougie, a stricture was found in the membranous portion of the urethra, but not a very tight one, as it admitted a No. 7 or 8-sized instrument without much difficulty. On passing the finger into the rectum the prostate could be felt rather enlarged, but no softening nor bogginess of it could be found. The opening of communication between the bowel and the urethra could be easily felt: it was situated just below the apex of the prostate. The external aperture of the perineal fistula was within half-an-inch of the front margin of the anus, and a probe could be passed from it deeply up by the side of the gut.

After keeping the man in the hospital for a few weeks, for the purpose of improving his general health, and of dilating the stricture by the passage of

bougies, I performed the following operation on the 31st of December, 1859:—I introduced a curved and grooved staff into the bladder, and passing a slightly curved sharp-pointed bistoury on the left index finger into the rectum, I thrust the point of it through the lower part of the prostate into the groove of the staff, and cutting downwards laid open the prostatic and part of the membranous portions of the urethra, into the rectum, so as to make a free communication between them. I also divided the sphincter ani, passing a bistoury from the fistulous opening in the perineum through the mucous membrane into the gut, just above the sphincter, and then cutting out. Very little hemorrhage followed the operation. The urine principally escaped by the bowel for the first fortnight, after which it was passed in gradually increasing quantities by the urethra, while the fistulous passage and the wound in the rectum slowly closed. The progress of the case was retarded by the state of the stricture, which seemed to be aggravated by the operation; and it required the frequent use of bougies for several weeks before the contraction and irritability of the urethra could be overcome. By the expiration of three months, however, from the time of the operation, he was completely cured.

*Case* 3.—In May, 1855, Mr. B., a gentleman, about 35 years of age, of delicate constitution, who had been under my care several weeks for an attack of gonorrhœa, from which he had almost recovered, was seized, without any apparent cause, with symptoms of inflammation of the prostate gland. He first complained of pain and irritation at the neck of the bladder: but after a couple of days he referred his sufferings chiefly to the rectum; and on examination considerable enlargement and tenderness of the prostate could be felt by the finger in the bowel.

Complete rest, warm hip-baths, leeches both to the perineum and round the anus, were tried, but without producing much benefit; and shivering and other symptoms of suppuration made their appearance. The patient continued to refer most of his uneasy sensations to the rectum, and there was little or no impediment to the passage of the urine, though a frequent inclination to make water. I could feel no hardness nor deep-seated fluctuation in the perineum, but one part of the prostate appearing boggy to the touch when the finger was introduced into the bowel, I determined to make an incision into it at once from the rectum, and thus try to prevent the matter from insinuating itself into the tissues of the perineum, or in other directions. For this purpose I introduced a tubular speculum into the rectum, with a good-sized lateral aperture, which I placed opposite to the middle of the prostate gland. The mucous membrane could be seen projecting into the tube, and distinct fluctuation could now be felt by the point of the finger. I therefore made an incision with the sharpened edge of a gum lancet that had a rounded blade, and after cutting to some depth, about half-an-ounce of thick pus escaped into the speculum. Considerable hemorrhage followed, but it was soon stopped by retaining the speculum (which had a closed extremity) in the bowel, and plugging it with lint. After thus opening the abscess, all the symptoms rapidly subsided, and the patient was soon well. There never seemed to be any discharge of urine from the wound.

*Case* 4.—In September, 1859, I was called in to see a gentleman, 34 years of age, who had just returned home ill from Scotland. He had recently been married, and had been seized during his wedding trip, while in the highlands of Scotland, with severe pain in the rectum, accompanied with some obstruction of the bowels and difficulty in making water. He obtained slight relief from taking some castor-oil, and made his way as soon as possible to Glasgow, where he consulted a surgeon, who ordered some leeches to the anus, warm baths, opiate suppositaries, &c., and recommended him to travel home as quickly as possible. I found him suffering from a dull pain in the rectum, increased by the sitting posture, from difficulty, (but not pain) in emptying the bladder, and also from considerable sympathetic fever. On introducing my finger into the bowel I found a good deal of enlargement, and great tenderness of the prostate gland. He told me that previous to his marriage he

had laboured for some time under stricture of the urethra, from which, however, he had quite recovered; so I have no doubt that there had been subacute inflammatory enlargement of the prostate, such as is so frequently consequent upon long-standing stricture, and this had become aggravated by the excitement of the urinary organs consequent upon marriage, together with neglect of the bowels. I prescribed perfect rest in the recumbent posture, warm hip-baths frequently repeated, salines with hyoscyamus, &c. These measures relieved the pain and general symptoms, but after two days considerable hardness and tenderness appeared on one side of the anus, and the inside of the rectum was very sensitive to the pressure of the finger, a little below the situation of the prostate, showing that the inflammation had spread from the gland to the cellular tissue surrounding the intestine. The prostate itself had somewhat subsided in size, and I could detect no feeling of fluctuation. I now began to fear that matter had formed, and was insinuating itself in various directions; but on the following day my patient told me that during the night something had burst into the bowel, which had given him great relief, and on feeling an inclination to have an evacuation, he had parted with a small quantity of transparent sticky fluid, something like white of egg, but of a yellow colour, some of which he had saved to show me. With the finger I could now feel a depression and small opening in the mucous membrane of the bowel in the same situation (rather below the prostate) where I found a tender spot the day before. Small quantities of a similar fluid continued to escape for a day or two, and at the same time all the symptoms, both local and general, disappeared. The enlargement of the prostate also greatly subsided.

On looking over these cases, do they suggest any points of special interest to which I would call the reader's attention? In the first place, I may remark that Surgical authors generally seem to have had a great dread of the formation of either a natural or artificial communication between the urethra and rectum. In cases of abscess in the prostate, deep incision is recommended in the perineum, to prevent the matter from finding its way either into the urethra or rectum. I am anxious to show that this fear as regards the latter canal is in a great measure chimerical, and that it is often even advantageous to make a free opening into the prostate from the bowel, in case of suppuration in that organ, should fluctuation be perceptible, as the matter will be so much nearer the surface in that situation than in any other. The diffusion of matter into the surrounding tissues is thus likely to be prevented.

The idea of treating chronic suppuration in the substance of the prostate gland by puncture from the rectum is due to Mr. W. Colles, but he restricted the operation to a particular class of cases. In the 'Dublin Journal' for 1845 he says:—"There is a description of enlarged prostate in which surgery can render essential benefit to the sufferer, and that by a very simple operation. When we find a patient in advanced life complaining of unusual frequency of micturition, with more than ordinary straining, his urine depositing a good deal of muco-purulent sediment, and possibly a muco-purulent discharge from the urethra, we should make a very careful examination of the state of the prostate. If, under these circumstances, we introduce the finger into the rectum, and find the gland enlarged in either lobe, and, upon pressing on one particular spot, we feel the point of the finger sink, as if into a cavity; and particularly if we find this pressure causes the discharge, per urethram, of a quantity of this purulent fluid, to the amount varying from a few drops to a teaspoonful; here we may hope to render an essential service. The operation to which I allude is simply that of striking a lancet into this hollow soft spot, which will generally be found to contain some matter."

Besides those referred to by Mr. Colles, there are many other cases of neglected abscess in or around the prostate in which fistulous passages have formed in various directions, where a free opening between the prostatic urethra and rectum will be especially useful. In both the cases of this kind which I have related, the inflammation and suppuration of the prostate,

which laid the foundation for the complicated mischief that existed when I first saw the patients, arose from stricture of the urethra, and occurred in youngish men; and I wish to direct attention to the fact that enlargement, the result of chronic inflammation, very frequently occurs in the prostate in cases of old-standing stricture, and, when not complicated with abscess, is generally easily removed by the internal administration of iodide of potassium, a circumstance pointed out by Sir B. Brodie in his most valuable work upon the Diseases of the Urinary Organs.—*Med. Times and Gazette, Oct.* 20, 1860, *p.* 372.

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71.—*Report on the Condition of the Prostate in Old Age, founded on the Dissection of One Hundred Specimens in Individuals over Sixty Years of Age.* By Dr. JOHN COCKBURN MESSER, R. N.—In order to facilitate the consideration of the details of one hundred dissections of the prostate after the age of sixty, the author had arranged them into three classes—viz., 1. Those under four drachms weight. 2. Those between four drachms and six drachms weight. 3. Those over six drachms weight. By so doing, a broad division was at once made between those that were comparatively healthy—namely, the first and second classes; and those that were so altered as to be likely to affect the health of the patient, comprised in the third class.

1. In the first class there were twenty cases, giving—

| | Minimum. | Maximum. | Medium. |
|---|---|---|---|
| Age . . . | 60 | 94 | 76·2 |
| Weight . . | 4 drs. | 6 drs. | 4 drs. 57 grs. |

These cases for the most part, differed from the normal state only in point of size, and offered no obstruction to the flow of urine. The presence of small black concretions was very general in these as well as in all the other cases. In four cases there were slight appearances of the formation of circumscribed tumours. In one case abscess was found associated with stricture of the urethra. In one the posterior lobe showed a tendency to enlargement; but it was difficult to say whether the enlargement was more intimately connected with the prostate or with a fasciculus of the muscular coat of the bladder.

2. In the second class were forty-five cases, which might be considered normal in condition, giving—

| | Minimum. | Maximum. | Medium. |
|---|---|---|---|
| Age . . . | 60 | 94 | 76·2 |
| Weight . . . | 4 drs. | 6 drs. | 4 drs. 55 grs. |

None of these cases suffered from urinary obstruction connected with the prostate during life, although the bladder was often found fasciculated. In twelve of these, circumscribed tumours were observed, for the most part only slightly developed; in three, the posterior lobe was slightly enlarged; in one, abscess was present, the consequence of general paralysis.

3. In the third class were thirty-five cases, which gave—

| | Minimum. | Maximum. | Medium. |
|---|---|---|---|
| Age . . . | 60 | 87 | 75·2 |
| Weight . . | 6 drs. 15 grs. | 48 drs. | 15 drs. 2. grs. |

In seventeen of these, the enlargement affected both lateral and posterior lobes; in fourteen, the enlargement existed chiefly in both lateral lobes; in one, the enlargement affected only the left lateral and posterior lobes; in one, enlargement preponderated in the left lateral and posterior lobes; in one, enlargement preponderated in the left lateral lobe; in one, enlargement preponderated in the posterior lobe. Thus it appeared that 35 per cent. of all prostates after the age of sixty are abnormally large, 20 per cent. are abnormally small, and 45 per cent. are within the limits of the normal weight. *This enlargement is* principally caused by increase of the fibrous element of

the body; the glandular also being increased in amount, but not to the same degree. This new fibrous tissue is deposited in concentric layers, and so forms circumscribed tumours. The frequency of this fibrous deposit is shown by the fact that it was present in thirty-four out of thirty-five cases of enlargement, in twenty-seven of which it was found in the form of tumours; in seven there was no appearance of tumours. It also appeared that those glands in which the tumours were marked are liable to the greatest enlargement, as some thus affected were found to weigh thirty drachms, and even forty-eight drachms, while those in which the tumours did not appear never weighed more than seventeen drachms.

A comparison of the relative frequency of enlargement of the different parts of the gland showed that the lateral lobes are much more liable to be affected than the posterior; thirty-four of thirty-five cases were affected in their lateral lobes, while only nineteen of the same number were affected in the posterior lobe. It is rare to find the posterior lobe enlarged while the rest of the gland is normal; only one such case in thirty-five was found. Enlargement of the posterior lobe is the chief cause of obstruction to the flow of urine; but that may also be the consequence of hypertrophy of the lateral lobes, especially when it takes the form of tumours, and they project inwards upon the urethra.

It appears, from the nearly equal average age in all three classes, that the condition of the prostate does not materially affect the longevity of the individual. A slight difference does, however, exist in favour of those in whom the gland is most nearly normal, the average in these being 76·2, and in the enlarged 75·2.

The presence of abscess in the prostate produces enlargement to a greater or less extent, seldom, however to the same extent as fibrous deposit. The most frequent cause of abscess in the prostate appears to be obstruction to the flow of urine, either from stricture of the urethra, enlargement of the prostate, or the consequence of paralysis of the bladder. The frequency of abscess in the enlarged gland is in the proportion of five in thirty-five; in those between four drachms and six drachms, one in forty-five; in those under four drachms, one in twenty. The causes in these cases were—stricture of urethra in three cases; frequent retention in three cases; paralysis of bladder in one case. Tubercle is the only other abnormal deposit giving rise to enlargement of the prostate noticed in these cases, and that only in one case, which weighed twenty-four drachms. A similar deposit was observed, in the lungs, right kidney, and mucous coat of bladder in this subject.

While retention of urine, more or less complete, is the most important symptom and consequence of enlarged prostate, it is not found in every case. The proportion of men in advanced years suffering from the consequences of enlarged prostate is indeed small. Thus amongst 1,600 old men, with an average sick list of two hundred, not more than ten are under treatment for this disease, and half of these only occasionally. A much larger number must be affected with enlargement, as shown by *post mortem* examinations of the gland. In thirty-five cases of enlargement found after death, thirteen suffered no urinary symptoms during life, and two others only after the occurrence of serious lesions to the nervous system shortly before death. Although many of these cases were not greatly enlarged, some of them plainly showed that the prostate may be greatly altered, and yet the patient be free from urinary obstruction, as in one case, where the prostate weighed eight drachms thirty grains, with prominent enlargement of the posterior lobe; in another, which weighed nineteen drachms thirty grains, with general hypertrophy, and great encroachment on the urethra; in another, which weighed twenty-six drachms thirty grains, with the enlargement pricipally seated in the lateral lobes.

On considering the favourable circumstances for the formation of phosphatic calculi in cases of enlarged prostate, it is surprising that these concretions are not more *frequently found*. Of the thirty-five cases of the third class, *phosphatic calculi were found* in two, the largest weighing seven

drachms forty-five grains; in another, two uric acid calculi, of about thirty grains each, were found.—*British Medical Journal, May* 19, 1860, *p.* 383.

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## 72.—ON A NEW FORM OF CATHETER FOR DILATING STRICTURES.

By Buxton Shillitoe, Esq., F.R.C.S.

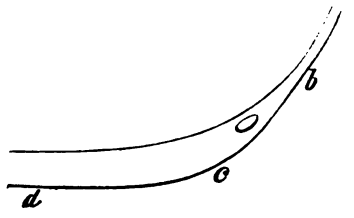
I have often tried, in severe cases of stricture, the old plan of retaining a catheter in the bladder, until I could pass a larger instrument; and I have usually found that if I could once pass a No. 1, or even No. ½, the difficulties of the case were overcome. There were, however, several objections which prevented my adopting this plan as often as I otherwise thought it advisable to do.

Firstly. The time that elapsed before a full-sized catheter could be passed (usually four or five days.)

Secondly. The necessity of keeping the patient in the recumbent position during the whole time.

Thirdly. The irritation and soreness induced by keeping the catheter in so long.

Fourthly. The frequent change of the instrument as the dilatation advanced, often produced so much pain as to cause the discontinuance of the treatment before the cure was complete.




Fifthly. The longer the time occupied in the dilatation, the greater the discharge, and the greater the difficulty in getting rid of it afterwards.

These objections, strengthened by the observation that I could frequently skip one or two numbers, even after the first six or eight hours, induced me to try a catheter having the form of a short cone, commencing about one inch and a-quarter from the end. This figure is a diagram of it. The extremity is made rather smaller than need be, and there should also be a hole near *a*, so that urine may flow through as soon as it has passed into the bladder.

The first part, from *a* to *b*, is of the size of a No. 1 catheter; it then gradually increases in the conical portion from *b* to *c*, up to the size of a No. 6 catheter, and this size is continued along the remainder of the tube. My second size commences at No, 6, and after the first inch and a-quarter increases, in the next inch up to 10 or 12.

My first trial of an instrument constructed on this principle was made on a gentleman who had suffered from stricture for the last twenty years. During this time he had submitted to internal division, and the stricture had once (eighteen months since) been dilated to No. 8, by tying in the catheter. This occupied four days. For the four previous months, a No. 1 or No. 2 gum-elastic bougie had been passed frequently, but whenever a No. 3 was tried, it was invariably followed by retention. This case, therefore, did not appear to be a very favourable one. I commenced on Thursday evening, at eleven o'clock, having previously ascertained that the seat of the stricture was near the bulbous portion of the urethra. I passed the catheter carefully through the stricture, and soon found the conical part firmly grasped by it; the urine, however, passed through the catheter. I then tied in the catheter, so as to exert a little pressure upon it, sufficient to prevent its slipping out again, and gave him a dose of opium. I visited my patient the next morning at nine, when I found that the catheter had passed up to the handle, so that the portion of the diameter of No. 6 must have gone through the stricture. I then withdrew it, and passed the one commencing

with No. 6; this I tied in firmly, and left it in until eight in the evening, when I was obliged to withdraw it, the patient being attacked by diarrhœa, to which he was very liable, and which was uncontrolled by astringents and opium. Previously to withdrawing it, I exerted a slight amount of pressure upon the instrument, and succeeded in passing it fairly through the stricture, so that in twenty-one hours I had succeeded in dilating it from No. 2 to No. 10. My patient did not suffer from retention or spasm, and but very slightly from soreness. I did not make any attempt to pass the instrument again for nine days. I then passed the larger sized catheter. Not the slightest pressure or force was used, and I succeeded in passing it through as far as No. 8 or 9; if I had used a very slight degree of force it would have gone through, but I did not like to run the risk of producing retention as the patient was compelled to transact business for some hours afterwards I expect that by passing the instrument about once a-week, and letting the patient sleep with it tied in, once every fortnight or three weeks, a cure will eventually be made; and I believe, that whenever strictures are capable of dilatation, this plan will be found to involve the least loss of time, the least pain, and the least inconvenience: and whether it be used by tying in the catheter, or by simply passing it, the conical form will be found to dilate more quickly and to be withdrawn more easily, on account of its shape. The part where the cone commences will vary with the seat of stricture, but any instrument maker can easily furnish the requisite catheter. Where this plan does not cure, it will at least be free from risk or danger, and will quickly convert an unmanageable stricture into a perfectly manageable one; or if time is of great importance, the tying in of the instrument for one night per week will dilate the stricture, without encroaching upon the next day.—*Medical Times and Gazette, July* 7, 1860, *p.* 5.

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73.—*New Operation for Phymosis.* By M. Ridreau.—The well-known operation for phymosis, practised by M. Ricord, leaves scarcely anything to be desired under ordinary circumstances, at least in the opinion of the majority. Some, however, object to the permanent exposure of the glans which so extensive a removal of the foreskin entails. To meet the views of surgeons holding this opinion, we quote from the 'Journal of Practical Medicine and Surgery,' the description of an operation, designed and successfully practised by M. Ridreau, a French military surgeon.

"Stretch the prepuce by drawing the mucous membrane forward, and the skin back, so as to lay bare the orifice of the foreskin; introduce a slender cylindrico-conic wooden rod into the aperture of the prepuce; perform a circular incision at about half a line from the mucous margin, dividing the skin only, which immediately shrinks backward on the glans; maintain the mucous lining upon the wooden rod, and remove circularly a sufficient quantity of it to give free play to the glans in the aperture resulting from the operation. Join the edges of the wound of the skin and of the mucous membrane by a few small needles and twisted suture. If a vessel bleeds, apply one of the sutures on that spot."

The wound heals in a few days with water dressing, and then the condition of the organ is perfectly normal, the glans being covered or exposed at will. Examination of the anatomy of the parts explains the success of this operation. The constriction is seated in the mucous membrane, and this is removed. Moreover, the skin of the penis unites with the mucous membrane, not by a diminution of its substance, but by accommodating itself by numerous wrinkles (in the usual manner of skin surrounding the sphincters) to the destined aperture; accordingly, the moment it is divided circularly, it may, without difficulty, be drawn back upon the penis. This operation possesses advantages peculiar to itself; a very limited portion of the textures is removed, a covering for the glans is retained; no deformity results; the cicatrix is linear, so imperceptible as to be mistaken for the natural junction of the skin and mucous membrane, and is entirely concealed when the prepuce

is drawn forward upon the glans; the portion of mucous membrane removed being replaced by integument.—*Dub. Hospital Gazette, Aug.* 1, 1860, *p.* 233.

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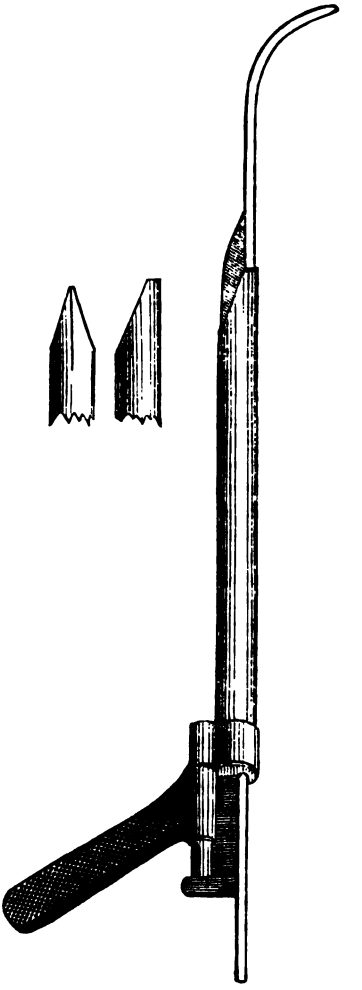
## 74.—TWO CASES OF STRICTURE OF THE URETHRA SUCCESSFULLY TREATED BY THE URETHROTOME DILATOR.

(Under the care of JOHN WOOD, Esq., King's College Hospital.)

Two cases of stricture of the urethra were operated upon by Mr. Wood, on the 16th of May, with his new instrument, the "urethrotome dilator." The first of these cases was that of a man of about forty, who had been suffering from stricture for many years, and had never had a larger instrument than a No. 4 passed. The stricture was situated three or four inches from the meatus, and admitted a No. 2 catheter pretty readily. The second case was that of a gold-digger lately returned from California, where he first became afflicted with stricture, the result of gonorrhœa. In this case the urethra was very irritable, the stricture being situated in the most common locality—namely, at the junction of the bulbous with the membranous portion of the urethra. A No. 4 catheter was passed by Mr. Wood, after much careful manipulation.



The instrument which Mr. Wood has but very lately brought under the notice of the profession combines the method of dilatation with that of the internal division of strictures, and consists—

1. Of a long steel staff, of about the calibre of a No. 2 catheter, grooved along its convexity, except for about two inches at its point, where it is curved to that extent.

2. Of a German silver canula, of the size of a No. 12. conical at its point, on the under surface of which there is a slit about half an inch in length. This canula is fitted with a convenient handle, fixed at an oblique angle.

3. Of a flat steel stilette, the point of which consists of a lancet-shaped cutting edge, about three-quarters of an inch long, working in the groove of the director by means of a thumb-plate and spiral spring.

The method of using this instrument seems to be simple and easy of application. The steel director is first passed through the stricture into the bladder, which fact may be ascertained by the urine trickling through the groove. The position of its point is always indicated by the groove, which is situated on the convexity of the instrument, and the short curve at its point allows it to be turned in any direction. The dilating canula is then slid over the director down to the stricture, which its conical point enters for a certain distance. Then the *stilette* is slid within the canula down *the groove of the* director, and on press-

ing the finger-plate at its extremity the cutting edge is protruded for about a quarter of an inch through the slit at the point of the canula, and still sliding in the groove in the director. It returns within the canula by the spiral spring in the handle extremity of the instrument. The dilating canula is now pushed on, and if it does not pass through the stricture, this may be scarified at its inferior, lateral, or even superior aspects, and the dilator pushed through it. The dilating and cutting portions of the instrument are now withdrawn, still leaving the original director. Over this again an elastic catheter is passed into the bladder, and the director withdrawn through it. In the two cases in which Mr. Wood operated, the instrument answered perfectly, the operation being performed without the aid of chloroform. In the first case there was no bleeding, and in the second only a few drops of blood escaped. Little pain was experienced by the patients, who walked home shortly afterwards, suffering little uneasiness.

Mr. Wood, in his remarks after the operation, made reference to the relative value of his instrument with those for the internal division of strictures from behind, in which a comparatively large-sized instrument must be passed through before scarification can be had recourse to. He said that the chief recommendations of the urethrotome dilator were—1. The safety with which it might be used. 2. That the cutting portion of the instrument would only cut the indurated and contracted tissues of the strictures, and only just sufficient for the passage of the dilating canula. 3. That all previous dilatation of the stricture became unnecessary. 4. That it affords a ready means of combining dilatation with limited section of the stricture.

May 19th. The second patient presented himself to-day, having suffered scarcely any inconvenience from the operation, since which the urine has been passed more freely, and in a larger stream. A No. 8, then a No. 9, and finally a No. 10 catheters were passed with ease.

The particulars of these two cases were furnished by Mr. Charles S. Mathews, assistant house-surgeon to the hospital.—*Lancet, July* 21, 1860, *p.* 58.

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## 75.—ON ALLARTON'S OPERATION FOR STONE IN THE BLADDER.

By Barnard Holt, Esq., F. R. C. S., Surgeon to the Westminster Hospital.

[This operation has often been declared to be improper, when the stone, or the prostate, are large. It would appear, however, that these two objections are not good ones, as the following cases show.]

J. R., a child aged eight years, was admitted into the Westminster Hospital under my care, April, 1860, suffering from stone in the bladder. The symptoms had existed for eight months prior to his admission, and consisted in difficulty and frequency of micturition, which required considerable straining to effect; occasional passage of bloody urine; and, but rarely, pain at the extremity of the penis. The boy having been placed under the influence of chloroform, a sound was passed to the neck of the bladder, where its further progress was arrested by some foreign body, which a slight amount of pressure sufficed to displace. The sound being thus fairly introduced, a stone was immediately detected, which, from the extent of surface traversed, appeared to be large.

I decided on performing Mr. Allarton's operation. On May 8th, while under chloroform, a staff as large as the urethra would admit was introduced, and the stone immediately detected. The child was now secured in the ordinary maner, and the forefinger of the left hand being passed into the rectum, the exact situation of the prostate was ascertained. A sharp-pointed and somewhat triangular knife was now thrust into the mesian line of the perinæum, with the back towards and about three lines in front of the anus, the finger in the rectum guiding the knife, and preventing its being wounded. An incision was now made directly upwards, and the groove of the staff cut into by *opening the urethra* immediately in front of the prostate; and

the knife was now run backwards and forwards to a slight extent so that it might be fairly divided, the tegumentary opening being enlarged as much as appeared necessary in its withdrawal. The forefinger was now removed from the rectum, and passed into the wound, with the nail in the groove of the staff, which thus formed an accurate guide for the director, which being made with a handle at right angles, was passed along the groove into the bladder. The assistant was now directed to pull the staff gently but firmly upwards towards the symphysis pubis, whilst I pulled the director gently downwards, and the forefinger of the left hand, being passed between these two instruments, was gently wormed between them with a twisting motion, dilating the parts as it proceeded until the neck of the bladder was reached, when both staff and director were withdrawn, and the dilatation was continued still further by rotating and moving the finger from side to side. The forceps were now introduced upon the finger, which was withdrawn as they passed into the bladder, so that to the present time no urine had escaped. The stone having been detected, the blades of the forceps were separated, when the gush of urine carried it between them, and with gentle traction it was removed. As was anticipated, it proved of considerable size, measuring two inches long and one inch wide, and weighing 240 grains; it was of the triple phosphate variety. The hemorrhage was of the most trifling description, and beyond the smarting attendant upon a wound he hardly suffered anything. *There was no involuntary escape of urine* after the operation, but it was passed at will, for the first week through the wound, but afterwards per urethram. Eight days from the operation he was walking about the ward, and in a fortnight was discharged cured.

The second case occurred in a spare, feeble man, aged sixty-five, who had suffered from stone for four years prior to his admission. The urethra admitted a full-sized sound, and the prostate was perfectly healthy. The bladder was, however, considerably contracted, not holding more than four ounces of urine; and the stone, which was readily detected, was large and dense. The same proceedings were adopted as have been already described; but the stone, proving even larger than was anticipated, eluded the blades of the forceps. A large pair were consequently had recourse to, and the stone removed. It was found to measure two inches and a quarter long by one inch and a half in breadth, and weighed 570 grains. It was of the oxalate of lime variety, coated with phosphates. The patient was comfortable during the evening, and passed his urine at will. He was, however, seized in the morning with a rigor, and died suddenly.

The post-mortem was made twenty-eight hours after death, and the bladder having been removed, the incision was found to be exactly in front of the prostate. The bladder was now cut into, and the mucous membrane corresponding to the neck was found to be congested and slightly torn, not, however, extending into the prostate gland, and much less than is met with in the lateral operation. The kidneys were in an advanced stage of cystic disease; the heart fatty; but the other organs were healthy.

The third case occurred in my private practice, in the person of a gentleman aged seventy-five, of spare habit, who had suffered from diseased prostate more or less for the last eight years, during which time he had occasional hemorrhage, sometimes slight, and sometimes sufficiently severe to call for the exhibition of gallic acid, the bleeding being at all times provoked by any extra walking or jolting exercise. Although there was no pain at the extremity of the penis after micturition, yet the symptoms to some extent, simulated those of stone, and the bladder was from time to time carefully examined both by myself and others, without, however, detecting any foreign body. Early in 1859, while at his seat in the country, he was attacked with inflammation of the bladder, characterized by rigors, great frequency of micturition, spasms of the most violent character, and occasional pain at the extremity of the penis, the urine depositing the usual amount of muco-purulent, tenacious deposit. Suitable remedies were had recourse to, and in the *course of three months* he recovered; the spasms entirely subsided, the urine *regained its healthy* character, but the difficulty of expulsion had so far in-

creased as to prevent his passing more than an ounce at a time. He was therefore directed to pass his own catheter three or four times in the day and night, which he accomplished without either difficulty or suffering. In the winter of 1859, being again in the country, he had another but more severe attack, which apparently arose from standing on damp ground, and it was evident, from a sudden discharge of pus and extreme tenderness of the prostate, that an abscess of that gland had formed and burst. His symptoms were now very distressing, the bladder much more irritable, and the pain at the extremity of the penis more decided; and being now unable to pass any urine naturally, he was compelled to rely entirely upon the catheter, which was passed every three hours. The bowels were daily relieved by enemata. The horizontal position was maintained, and all the known remedies had recourse to, belladonna, copaiba, and chlorodyne giving the greatest relief.

In the spring of the present year he was removed to town, the frequent and almost irresistible urgencies continuing, and the pain at the extremity of the penis being more acute. Opium, in the form of suppositories, was the only drug that now afforded the slightest relief.

On Sunday, June 15th, after violent straining at stool, the bladder became more than usually irritable; and being requested to pass his gum catheter, I immediately detected a calculus, rough, and so far as could be ascertained by such an instrument, not very large. A consultation with Mr. Arnott and Mr. Cæsar Hawkins was held: and as lithotrity offered no chances of relief, Allarton's operation was decided upon.

On June 27th he was placed under the influence of chloroform, and the operation, as already detailed, was performed. Not the slightest difficulty was experienced in reaching the neck of the bladder, but the size of the prostate prevented the interior being explored, or the stone being felt with the finger. The forceps were consequently substituted, and being buried to a considerable depth, a stone was caught and removed, proving large, smooth, and polished. Another was sought for and removed, which bore an accurate resemblance to the first. It was, therefore, evident there must be a third, which, after slight difficulty, was also included between the blades of the forceps, but being larger than either of the former ones, it required some little patience and gentle traction to remove it. The parts, however, yielded, and the third, somewhat roughened, stone was now abstracted. The first measured 1⅜ in. by 1½ in.; the second, 1⅜ in. by 1¾ in.; and the third, 1⅜ in. by 1½ in.: the weight of the three being 960 grains. The incision was insignificantly small; the hemorrhage very trifling; and my patient being now placed in bed, shortly recovered from the effects of the chloroform, and simply complained of the slight smarting of the wound. The neck of the bladder retained its contractility, and there was not any involuntary escape. The suppository was introduced as usual; and, excepting when the urine was withdrawn, he slept tranquilly. For the first three days he progressed in the most favorable manner; the intervals were increased, the catheter being introduced every four, instead of every three, hours; the spasms were less frequent and less intense, and he was nearly relieved from the pain he had previously experienced, when, on the fourth day he was seized with rigors, extreme and urgent pain in the region of the stomach, vomiting, and his pulse, which was exceedingly weak, rose to 130; skin hot; countenance anxious; and the urine presenting an appearance of bloody jelly. Opium and stimulants were administered, the bladder was injected with tepid water, and he rallied. He, however, continued exceedingly ill for three or four days, when the attack gradually subsided; the urine became more natural, but the spasms of the bladder recurred; and fearing the possibility of a calculus having descended from the kidney, he was again examined, under the influence of chloroform, without however discovering any foreign body; a roughness was detected with the gum-elastic catheter, but not with the sound, and in all probability depended upon a calcareous coating of some portion of the mucous membrane; indeed, *this was subsequently ascertained* to be the case, for upon introducing

a lithotrity spoon, a portion of gritty matter was removed, and eventually the corresponding mucous surface, coated with phosphates, came away in the eye of the catheter. Small particles of the same deposit have from time to time escaped. The frequency and spasms have now entirely subsided; he is enabled to pass per urethram from two to three ounces of urine; the bladder will retain from ten to twelve ounces; the wound has so far healed as to be hardly perceptible, and when necessary he can introduce his own catheter. The bowels which have hitherto required enemata of water, have acted naturally since taking the confection of senna; and an examination per anum proves the prostate to be diminished in size. He is daily gaining flesh and strength, and when the weather will permit drives out.

The present cases have been described simply as examples to prove that large calculi and an enlarged prostate form no bar to the operation, and, as far as they go, establish the facility with which a large stone can be removed without injury to the neck of the bladder. It is true that the mortality is about the average; but as in the fatal case there was probably sufficient cause in the state of the kidneys and heart superadded to the rigor, from which persons in advanced years frequently die, I cannot consider it as in any manner militating against the operation.

The case of the boy was interesting in showing how circumstances may alter and modify symptoms generally relied upon as characteristic of stone. The pain at the extremity of the penis was the exception, and not the rule, and evidently depended upon the stone generally occupying the same position and requiring to be displaced before the sound could be fairly introduced. The diagnosis was correct in reference to its size, which it must be admitted very far exceeded the average of such cases at such an age.

In the second case, the facility with which the stone was detected, and the ringing sound, afforded sufficient evidence of its size and density to preclude any advantage from lithotrity.

The third case must be considered as the most important of the three. The size of the prostate—which did not permit the finger to reach the bladder after the necessary incisions had been made, the long duration of the disease, the inability to pass any urine without the introduction of a catheter, the irritable state of the bladder, the emaciated condition of my patient, and the presence of three such very large calculi, were sufficient to test the operation to the utmost. I may mention that this gentleman had been subject to severe bilious attacks which very much resembled the one that supervened on the fourth day, and that the issue of the case proves the symptoms to have depended upon a deranged liver.

I have described the operation as it was performed; and although the precaution of introducing the finger between the director and the sound may not be absolutely necessary, yet I am convinced it simplifies the operation to such a degree that no surgeon of common intelligence need fear undertaking it. The extraction of the stone requires care and gentleness. A minute lost is a minute gained, if a large calculus can be removed through the neck of the bladder without injury; and I feel convinced this can always be effected if the necessary care is exercised. In almost all the descriptions of the ordinary forms of operating, the direction is, to have a large external wound. Such is not necessary; and if a stone can be removed, without bruising, through a moderate wound, it is certainly better than making a large one.

From all that I have yet seen of Mr. Allarton's operations, I consider it a most valuable one; the wound is less; the hemorrhage is less; the constitutional disturbance is less; there is no chance of infiltration of urine; and, as a matter of comfort to the patient, the ability to retain the urine is of the utmost value and importance.—*Lancet, Sept.* 8, 1860, *p.* 234.

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*76.—On Allarton's Lithotomy Operation.* By NATHANIEL WARD, Esq., *F.R.C.S., Assistant* Surgeon to the London Hospital.—[The lateral opera-

tion generally requires much preliminary study, and constant caution and care, in order to execute it with precision and without risk, the chief dangers being the slipping of the instrument from the groove of the staff, and the wounding the bladder or rectum.]

The comparatively greater safety, simplicity, and facility of the operation recommended by Mr. Allarton appear to me to constitute its peculiar merits. On the six occasions on which it has been performed at the London Hospital, either by my colleagues, Messrs. Critchett and Gowlland, or by myself, the steps recommended by Mr. Allarton have been followed with but a slight variation. The staff having been introduced into the *uninjected* bladder, the prostate was made out by the left index finger passed into the rectum, and its apex was steadied by it against the staff. A scalpel, with the blade half an inch longer than the length of the finger in the rectum, and having a straight cutting edge and curved back, was introduced with the cutting edge upwards into the perineal raphé, about half an inch in front of the anus, and passed steadily downwards and backwards until its point impinged in the groove of the staff, cutting through the lower wall of the backpart of the membranous portion of the urethra, just in front of the point of the index finger.

The knife was then carried forward about the eighth of an inch along the groove of the staff, and was then withdrawn by sweeping it upwards and forwards (as regards the operator), so as to make a median cutaneous incision about an inch in length. A common bone probe, set in a rough handle, was next passed along the groove of the staff into the bladder. The staff was then removed, and the left index finger, well greased, being passed along the *upper* surface of the probe to the prostatic urethra, arrived through this, after several graduated rotations, into the vesical cavity. The forceps were next introduced along the upper surface of the finger, and the calculi removed.

Thus it will be seen that the only points in which this lithotomy differed from Mr. Allarton's directions were—the use of the form of scalpel alluded to, and the incision of the membranous urethra by drawing the knife forwards and upwards, instead of passing it towards the prostate in the contrary direction. The cases were all successful, and the patients rapidly recovered.

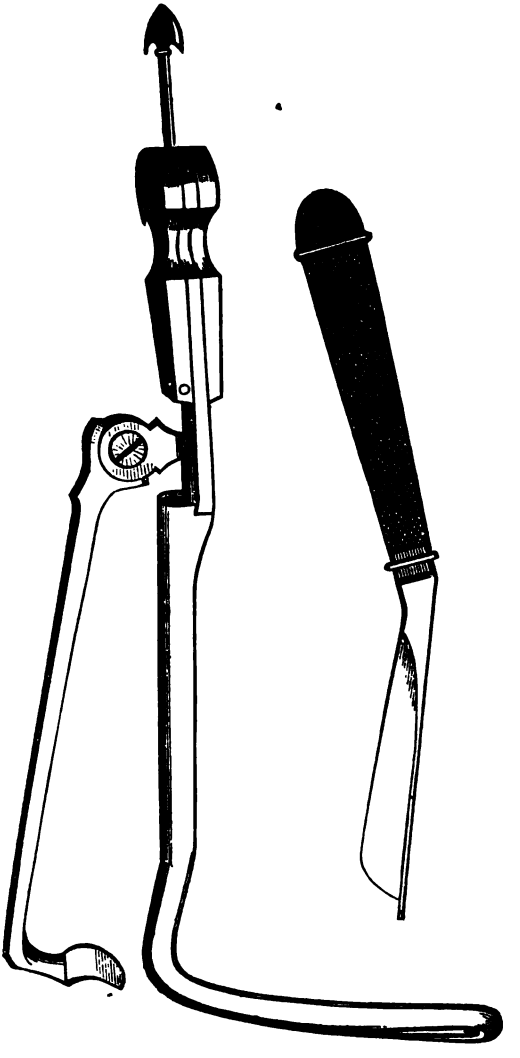
Independently of the many recommendatory points in this method of urethral lithotomy insisted on by Mr. Allarton, the *necessity of injecting the bladder* before cutting *does not exist*, inasmuch as the cavity of the viscus is not cut *into;* and the bladder is not in danger of being wounded, as the incision nearest to it is carried away from it towards the operator. But there is an important reason why *the bladder should not be injected*, and it is this:—The prostate gland is taken to be, so to speak, the land-mark of the operation. This organ in children is very small, and it requires a little education of the finger in order readily to detect it. If the bladder is tense in consequence of retained urine or injected fluid, the walls encroach on the immediate area of the gland, and the prostate is then not easy to be felt. On the contrary, when that viscus is not distended, the prostate stands out in comparatively prominent relief, and can easily be felt by the finger, and steadied against the staff.

I have little doubt that Allarton's operation will, in course of time, be generally adopted for the removal of small or average-volumed calculi from the bladder, and I cannot but think that since its introduction it would have received ere this a greater amount of sound surgical sanction had some lithotomists of this day, of the highest authority, deviated, in a spirit of inquiry, from a beaten track; and experimentally tested the applicability and efficiency of the modified Marian operation, the revival of which must be regarded as a useful addition to the resources of the healing art, and as a high compliment on the part of modern to ancient surgery.—*Lancet, June* 9, 1860, *p.* 568.

## 77.—ON LITHOTOMY.

By Professor N. R. Smith, of the University of Maryland.

[Instruments at all complicated only increase the difficulty of this important operation, which is becoming more and more simplified, especially since Allarton's operation has become popular in this country. Nevertheless, we think it right to let our readers see what other people do, and especially our cousins over the water, who are making rapid strides both in surgery and in *journalism* to overtake the mother country. Professor Smith, of Baltimore, has used this instrument with great success for thirty years, in more than 100 cases. He describes it as follows:]



*The staff is* a silver canula, with a slightly-spiral fenestrum cut out of its *dorsum, and* continued to near its point; along this the knife glides. You

see the cutting director attached to the handle of the instrument by a strong hinge. This has an angle about an inch from its lower extremity, and at its end is a cutting point, like that of a broad scalpel. On the inferior border of this angular portion is a groove, which receives this, back of the knife. When, as the hinge allows, you extend the director, it stands out at right angles to the shaft. When you depress it, you see the scalpel point enters the fenestrum of the staff at the dorsum, and cannot deviate from it. In the hollow of the staff is a short cylinder, concave towards the handle. To the centre of the concave end is attached a fine, flexible steel wire, which passes up through the handle of the instrument along a small canal. The cylinder glides freely along the canal of the staff, and drags the wire index where the knife encounters it in the staff.

I now proceed with the operation, the patient being placed in the usual attitude, and chloroform having been administered.

You see I introduce the staff with ease, and touch the stone with it. I carry the handle well forward and to the left side. I now bring down the hinged-cutting director, and with precision apply its point to the integuments on the left of the anus, and nearly on a level with its anterior margin. It is thus directed somewhat obliquely upward and forward, behind the bulb of the urethra, toward the membranous portion. I now press the director firmly down, and it cuts its way into the hollow of the staff, between the bulb of the urethra and the crust of the penis. It is firmly fixed, and cannot change its place.

I now apply the back of the gorget-knife, the handle of which has an angle with the blade, to the groove in the cutting director, and penetrate the hollow of the staff. I now press the body of the staff well to the right side, so as to make more room for the incision in the left perineum, and I cause the knife to glide along the spiral opening in the staff, directing its edge obliquely outward and downward. You see that it drags along with it the wire index, which causes no impediment. I have reached the extremity of the staff in the bladder. I now withdraw the knife, and, as I do so, dilate the outer portion of the wound.

You see me now introduce my finger before I withdraw the staff. I penetrate the bladder with it—the incision is sufficiently free. I now raise the cutting director, and withdraw the staff, retaining the index finger of the left hand in the wound.

Guided by the finger, I now introduce these forceps (comparatively small and short.) I seize the stone without difficulty, and am now attempting its extraction. I feel it break in the forceps, it being friable. I extract the principal portion. I feel often fragments with the finger. You see I have no difficulty in seizing them. I reach every portion of the interior of the bladder with the finger. I have now extracted the last fragment, and feel that the bladder is free. I shall not inject the bladder, because I explore it so freely with the finger, and feel that nothing remains. The operation is completed. You observe there is but little hemorrhage—none requiring any mechanical expedients to arrest it.

I shall introduce no canula, which I often use. I omit it in this case, because the opening is free and the perineum not deep, and because, in the recent case of Gov. Hicks, I found it to create irritation.

In Braithwaite's 'Retrospect,' vol. 39, you will find a description and plate of an instrument, the invention of which is claimed by Mr. Corbett, of the Glasgow Royal Infirmary, and which is identical with mine, except that the bend in the staff is angular and not abruptly curved as mine is. This deviation only impairs the usefulness of the instrument.

I published an account of my instrument in 1831, and, from that time to the present, have employed it in presence of the medical classes of the University of Maryland.

The advantages which I claim for this instrument are, that it greatly simplifies and abbreviates the operation—that it gives unity and continuity to the incision—that it obviates the danger of wounding the artery of the bulb,

and the danger of the slipping of the knife or gorget from the groove of the staff.—*Maryland and Virginia Medical Journal, May*, 1860, *p.* 345.

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## DISEASES OF THE SKIN.

### 78.—STATISTICAL ANALYSIS OF FORTY-TWO CASES OF RODENT ULCER.

By JONATHAN HUTCHINSON, Esq.

[This gentleman has published a valuable series of cases on this subject, which we have not space to give in detail. We will, however, give some of the most important results.]

The parts by far the most liable to be affected by the rodent ulcer are the eyelids and adjacent portions of cheek. Next to them the nose is its favorite site, and the third in the list are the cheeks. On one or other of these positions almost all the rodent ulcers occur. No case has yet been recorded in which this ulcer occurred in the integument of the extremities or on that of the trunk, if we except one case, in which the nipple was the part involved. It is a remarkable fact that in only one instance was the upper lip the primary seat, and that in no single case was the lower lip involved either primarily or by extension. This fact is of great importance, when it is borne in mind how frequently the lower lip is the seat of epithelial cancer. The single case in which, as just stated, the upper lip was affected, is not wholly exempt from doubt as to whether, indeed, it did not approach to the epithelial class of growths, since, unlike what occurs in a rodent ulcer, a lymphatic gland was enlarged.

*Questions of Treatment.*—The rodent ulcer is to be viewed, as far as our knowledge extends, as a local disease, which spreads by continuous growth. There is not a tittle of evidence in favor of the belief that it can be influenced by internal medication. The object which the surgeon must keep in view is to remove the whole of the diseased parts, and to procure a soft, supple cicatrix. If there be any tension on the cicatrix, the irritation thereby produced will be almost certain to induce a relapse of the disease. By means of deep escharotics, such as the chloride of zinc, complete destruction of the diseased parts may in many instances be ensured, and a very healthy and pliant cicatrix is usually obtained by this remedy. In many of the favorite seats of rodent ulcer,—the eyelids to wit,—the use of escharotics is very inconvenient; and in these, free excision, followed by the transplantation of healthy skin from the forehead, cheek, or temple, is the best measure.

*Aphorisms respecting the Rodent Ulcer.*—1. That there occurs not infrequently on one or other part of the face a form of ulceration which is characterized by an indurated edge, and by a tendency to spread to adjacent structures, without regard to difference of tissue; which is very slow in its progress; does not cause much pain; does not induce cachexia, and is never followed by enlarged glands or deposits in the viscera.

2. Sections of the indurated edge of this ulcer (or of the portions of new growth which are sometimes produced about it) do not exhibit the cell-structures met with in epithelial or scirrhous cancer. but only those of organising fibrous tissue.

3. This ulcer differs from lupus exedens in that it never occurs in the young, and never gets well spontaneously, while lupes exedens but rarely begins after the age of thirty, and usually tends after the lapse of time to cicatrise spontaneously. The two, also, further differ, in that lupus has a tuberculated, inflamed border, without any degree of induration; while the edges of the ulcer in question presents an extremely indurated ridge, without tubercles, and comparatively free from inflammatory congestion.

*4. The ulcer in* question differs from cancer in that there is but seldom *present any tendency* to the production of new material, that it never

causes the glands to enlarge nor induces morbid growths in the internal viscera.

5. Although it must be freely admitted that this disease is closely allied to cancer, and that in its inveteracy under treatment, and its tendency, if not removed, to spread deeply and extensively, it well deserves the designation of "locally malignant," yet it is inconvenient in practice to call it "cancer of the skin," since there are other forms of cutaneous cancer (the epithelial, scirrhous, melanotic, &c.) essentially different from it, and of a far higher degree of malignancy.

6. The term "a peculiar ulcer occurring in the eyelids," is too vague, and also involves an erroneous statement as to uniformity of location, an objection which, also, in addition to what has been stated above, applies to "cancer of the eyelids," since this ulcer is met with on many other parts besides the palpebræ.

7. To the designation of Rodent Ulcer given to this disease by Lebert, and adopted in this country by Paget (see Lectures on Surgical Pathology) no objection applies, excepting that it is more vague than desirable. Of those in use it is certainly the best, and should the disease become generally recognised by the profession under that name, the vagueness of its meaning will by custom soon cease.

8. The rodent ulcer is most commonly met with between the ages of 50 and 60, and is equally frequent in the two sexes.

9. It occurs but very rarely on any other region than the integument of the face, and is most common in the eyelids.

10. It is a singular and very significant fact that no case has yet been recorded in which the rodent ulcer attacked the lower lip, either primarily or by extension, while that part is well known to be a very frequent seat of epithelial cancer.

11. The *Diagnosis* of rodent ulcer is usually easy. An ulcer with a hard sinuous edge, situated on some part of the skin of the upper two-thirds of the face, of several, or, perhaps, many years' duration, almost painless, and occurring in a middle-eged or elderly person, of fair health, and without enlarged glands—such a sore is almost certain to be of the rodent type.

12. The *prognosis* of rodent ulcer varies with the stage of the disease and the treatment it is intended to pursue. If left to itself it will slowly but surely advance both in extent and depth, and will probably destroy the patient's life in the course of from ten to twenty-five years, death being eventually produced by the exhaustion consequent on suppuration, hemorrhages, pain, &c., and very probably aggravated by inability to take sufficient food owing to the diseased state of the mouth. If the case be seen in an early stage while complete removal either by knife or escharotics is practicable, a favourable opinion may be given as to the probable non-return of the disease. The younger the patient the more rapid will be the course of the disease, and *vice versa;* and the younger the patient the more nearly is the disease allied to cancer, and the more likely to recur after removal.

13. The only *treatment* which the rodent ulcer admits of is local, and the basis is that which obtains its freest removal with the least injury to the parts concerned. In some localities, and in some stages, escharotics, such as the chloride of zinc, may be advisable, but in most excision and transplantation of skin is the more certain and satisfactory.

14. A widely-diffused knowledge of the true pathology of rodent ulcer may be expected to result in considerable advantage to the sufferers from that disease, since it will encourage to the early and free adoption of local measures and to the employment of excision and transplantation even in some cases which, if considered cancerous, would certainly be beyond relief by surgical art.—*Medical Times and Gazette, Sept.* 29, 1860, *p.* 305.

## 79.—ON THE TREATMENT OF FAVUS.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary of Edinburgh, Lecturer on Clinical Medicine and on Practice of Physic.

[In the case which forms the text of the following remarks, the head was at first covered with yellow crusts, of long standing; exactly four days afterwards, there was not a vestige of a crust to be seen, nor even any broken surface, though the patches of absolute baldness and the stunted and diminished hairs in many places, showed clearly the deep hold the disease had taken. The change was entirely due to the successful poultices of linseed meal.]

After so much has been written about favus, and so many perfect cures have been recorded in periods varying from six weeks to several months, I am almost afraid to state my conviction, that the satisfactory result above mentioned, obtained in four days under linseed meal poultices, was quite as much entitled to the name of a cure as any that I have yet seen or heard of either in nature or in the records of medicine. To speak of a cure of this disease, with opportunities of observation extending over less than a year or two, is, in my opinion, evidence of nothing else than the most entire ignorance of its habits. I do not, however, doubt the cure of favus. Soap and hot water, with abundant scrubbing, the hair being kept short, will commonly keep the yellow crusts indefinitely in abeyance; as will also, perhaps more thoroughly and effectually, the simplest oil inunction. There seems no reason, therefore, to believe (though hospital physicians can but seldom hope to witness the result) that these simple means *long and perseveringly used*, will not effect the cure of a disease which owes its origin and perpetuation to nothing else than want of cleanliness.

Under ordinary circumstances, what takes place after an apparent cure of favus is this:—So long as the hair is kept shaved, and an alternation of oily applications with soap and water is maintained, the disease does not reappear; but on neglecting these precautions for a few weeks, yellow dots begin to crop up, and these rapidly extend so as to become distinct favus crusts, which in no long time, if uninterfered with, will cover the whole head. I have repeatedly kept cases under observation after the head had been completely cleared, in order to observe the first beginnings of the eruption after the suspension of the treatment; and I have also employed a great variety of medicated ointments and lotions, including sulphurous acid, iodine and sulphur ointments, empyreumatic oils, mecurial ointments, and mixed medications of various kinds. After most of these, I have seen the disease reappear about as quickly as under the simpler treatment by oil and soap. If there is any of them in which I have faith more than another, it is in empyreumatic oils, as the juniper tar oil or common pitch ointment. But the inveteracy of the disease evidently depends, not on the difficulty of removing its visible traces, but on the complete infiltration (so to speak) of the scalp with the sporules of the fungus in all old standing cases; and no treatment will be of the slightest avail towards a radical cure that is not deliberately and carefully pursued until a complete new growth of scarf skin has been obtained, perfectly free from all traces of the noxious germs. This must, of course, be the work of a considerable time; just as it is a work of time, and of unwearied attention to simple details, to rid a virgin soil of ragweeds and whins, or even of stones. No application of a specific can be expected to meet the one case, any more than the other.

One point, not always observed by those who have written on this subject, is, that favus is often, perhaps even in the majority of cases, implanted on the basis of a previous eruption; in other words, that the fungous crusts, or vegetable mould, are sown on a soil already the seat of impetigo, eczema, or some other variety of disease of the skin. Sometimes the original disease has died out when the favus first comes under treatment; at other times, it *still* persists and requires separate treatment. In the course of considerable *and varied experience* of true favus, however, I have not seen a single case

that did not at once yield to local treatment, to the extent I have indicated above; and I am very far from believing that any constitutional disorder has to do with the production of the fungus, further than that favus and other diseases may arise simultaneously, under exposure to the same causes of filth, neglect, and hygienic errors of every kind, in every variety of bodily constitution.—*Edin. Med. Journal, May* 1860, *p.* 1003.

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80.—*Glycerine in Skin Diseases.*—This substance has been justly recommended in various affections of the skin, and especially in those attended with desquamation. In that troublesome affection *pityriasis capitis*, in which the hairs become dry and fall off, during the abundant epidemic exfoliation, undiluted glycerine may be applied with excellent and durable effect. In *pityriasis rubra* and *pityriasis simplex*, a mixture, composed of equal parts of oil of almonds and glycerine, and one-half of oxide of zinc, has proved very useful.—*Dublin Hospital Gazette, May* 15, 1860, *p.* 158.

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81.—*Treatment of Eruptions around the Anus.*—By JOSEPH BELL, Esq., Gateshead.—Occasionally we see obstinate cutaneous ulceration surrounding the anus in children. Considerable tumefaction attends it betimes, and deep fissures are occasionally seen. This disorder is probably herpetic, and almost always can be cured with yellow wash. The proportions being from one grain to one grain and a-half of hydrarg. bichlorid, to one ounce aq. calcis. The part is to be frequently bathed with it, and should the lotion produce pain it is to be diluted with water, and when at rest a little lint soaked in the lotion is to be applied and left on the part; deobstruents being at the same time administered.—*Med. Times and Gazette, July* 7, 1860, *p.* 23.

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82.—*On the Therapeutical Methods of preventing Pitting of the Face in Confluent Small-Pox.*—By Dr. STOKES, Dublin.—During the last five years Dr. Stokes has employed gutta-percha and collodion in a considerable number of cases of confluent small-pox, for the purpose of preventing pitting of the face. In most of the cases the crust came off in large flakes or patches, composed of the dried exudations and the covering material, leaving the skin uninjured. This kind of treatment was most successful in cases of a typhoid character, but appeared to be not so well adapted to those presenting a more sthenic type. Dr. Stokes considers that the application of poultices over the face is the surest method of preventing disfigurement in small-pox. Their use should be commenced at the earliest period, and continued to an advanced stage of the disease. In most cases they may be applied even over the nose, so as to cover the nostrils. This plan should fulfil three important indications of treatment—namely, to exclude air, to moderate the local irritation, and to keep the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs. The best poultice is formed of linseed meal, which should be spread on a soft material, such as French wadding, and covered with gutta-percha paper or oiled silk. The conclusions to which Dr. Stokes arrives are the following: 1. That the chances of marking are much greater in the sthenic or inflammatory than in the asthenic or typhoid confluent small-pox. 2. That, considering the change in the character of disease observed during late years, we may explain the greater frequency of marking in former times. 3. That, in the typhoid forms of the disease the treatment of the surface by an artificial covering, such as gutta-percha or glycerine, will often prove satisfactory. 4. That, in the more active or non-typhoid forms the use of constant poulticing, and of every other method which will lessen local inflammation, seems to be the best mode of preventing disfigurement of the face.—*British and Foreign Medico-Chirurgical Review, July* 1860, *p.* 248.

## DISEASES OF THE EYE AND EAR.

### 83.—ON CORELYSIS.

By J. F. Streatfeild, Esq., Editor of the Ophthalmic Hospital Reports.

[A short notice appeared in the journals about three years ago, upon the subject of Corelysis. Mr. Streatfeild has now matured the operation, and we insert the most practical parts of his paper, which, in the original, would occupy too much space for this work. Mr. Streatfeild has been perfectly successful in freeing the iris whenever it is adherent at its pupillary margin, whether to the lens or cornea, or to a false membrane in the pupil itself. He now names his operation *Corelysis* (meaning Pupil freeing, from Κόρη and λύσιρ.

My paper will throughout appear to have a double bearing, as it refers to the two cases in which either the pupil is bound to the lens or cornea, or to a false membrane in its area, but they are alike and are here always considered together, for in either of them it is required to *free the pupil.*

As to the *prevention* of pupillary adhesions I have no other suggestion but that the old belief in belladonna and mercury should be cherished, that the former should never be neglected at any time in the treatment of iritis, and the latter should be generally administered in very frequent small doses continued for some time after the disappearance of the inflammation, with good food or perhaps some iron or other tonic, as well as the specific medicines. At the conclusion of the treatment of a case of iritis the absence of any adhesion should be ascertained, if the iris is not perfectly active, it may be because of a single band between it and the lens; and if at this time the atropine will not cause its separation, it is certainly, in my opinion, to be divided.

Until I commenced freeing the pupil in so many of the cases in which it is bound, I believe adhesions were disregarded, as far as any surgical or other remedial attempt is concerned, in any less serious cases than those of virtual blindness, that is to say, that unless there was very extensive posterior adhesion, the pupil was closed, or so in effect, the adhesions were allowed as of course. If belladonna was applied it was for the purpose of discovering an interval of the adhesions for the contemplated artificial pupil, whereby the iris was to be torn, cut or excised, the adhesions themselves being left, and a confessedly ill-placed pupil made by one of the old methods. Belladonna will in fact seldom effect the severance of pupil-adhesions, when a patient applies because of them, and not for the efficient inflammation, then no longer existing; it may do so, it should be tried, and the discovery of an interval of non-adhesion of the pupil is to me of as great importance for the operation on the membrane which obstructs the pupil as it is for making an artificial pupil, in these cases the most urgently requiring an operation, or in any case for corelysis. This new method can, therefore, be only compared with corectenia, corotomia, corectomia, and some others, all of which are really operations on the iris, not like corelysis on that which confines the pupil, or occupies its place. These adhesions then have been permitted, but if all useful vision is lost in any case, the iris has been cut through or a part of it removed and an artificial pupil, necessarily out of the axis of vision has been formed; these pupils are certainly not the best that could be imagined, and the operations on the iris, supposing them unnecessary in these cases, are also, it now seems to me, too severe, but for those of them in which the lens-capsule certainly cannot be cleared in the area of the pupil. If, for the sake of conservative surgery, the iris should not even always be operated on for occlusion of the pupil, there has been introduced an heroic operation, which I believe to be quite unjustifiable, for the deliberate extraction of the transparent lens in these cases; if, by corelysis, a cataract is discovered, it can be removed by way of the restored pupil, *but I believe* the lens is seldom opaque in cases of occlusion, and I *know that although* adhesions may exist between the false membrane oc-

cluding the pupil, and the lens-capsule, with which it is in apposition, they are probably only partial and much weaker than the capsule itself, at any rate in the cases presenting the external appearances to which I shall allude.

When corelysis, instead of doing nothing operative or making an artificial pupil, seems to be the alternative, it is because the ordinary medicinal agents used to prevent the adhesions or for their detachment, have been ineffectual; mercury has been tried, and no ciliary redness or other signs of latent inflammation exist, still the tentative use of belladonna may be made in any case, although it has probably been tried, and that when the adhesions more recently established would be much more likely to give way. I do not know for certain how recent must be the adhesions, which, by the use of belladonna, will give way, which will be thus separated, or on what other conditions of treatment, or the state of the patient's health, this will depend; old bands will be extended by its use, if they are narrow and isolated, and recent adhesions, *only, as far as I know*, may entirely disappear. (It is a common mistake to suppose that of course adhesions found to exist with iritis, have resulted from the present inflammation of the iris). When the adhesions are to a false membrane in the area of the pupil, I believe this membrane, however slight it may be, can only be dispersed by corelysis; no medicinal agent will remove it, if it exists after the inflammation has ended.

Atropine—or, applied to the skin, belladonna rubbed down with glycerine—should be used without delay in the cases, whether recent or determined, in which the pupil may be or has been involved; if employed when the pupil can dilate fully, the iris is so removed from the lens that it can hardly become bound to it by the opposite effect of the iritis, and then lymph having been effused between them, the midriatic effect will, perhaps, be enough to cause their separation, and if not the patient's vision is thereby probably improved for a time, and it is altogether essential to the surgeon for an exact knowledge of the state of the case. It is most important that the agent should be repeatedly employed; a solution of a quarter of a grain of sulphate of atropia to an ounce of water, will produce full dilatation in a healthy eye (but, in iritis, past or present, the effect is varied in the different cases), a strong atropine solution cannot cause more dilatation than this, but by it the effect is brought about more rapidly, it also probably lasts longer, and, when the iris is inflamed, perhaps it is the best to be employed (a shade worn by the patient may promote the dilatation or its continuance); and if these means have been ineffectual they should be persevered in, whilst inflammation exists, because the treatment otherwise adopted may give an opportunity of dilatation in time, but if there is no inflammation they may be hopelessly discontinued. That posterior adhesions not unfrequently become detatched is proved by the opthalmoscopic examinations, in which, with a pupil properly dilated, their remains are seen often as a circle of irregular pigmented dots, or one or two opposite the margin of the pupil when it has been contracted; these, and, after corelysis, the like dots are compatible with very good vision.

The basis of the new method of operating is expressed in the title first applied to it of "reinstating the pupil," it is founded on the wish to restore the normal state of the parts, to imitate it better than by artificial pupil, or to improve the vision of those patients with bands of adhesion after iritis, in whose cases they have been left, if not with impaired vision, with probability of a recurrence of the inflammation which had caused them. For this purpose the pupil must be freed, and, if it was practicable, could be only found by the attempt in the different cases in which it is bound. Could the bands of adhesion confining the pupil be broken through by operation without injury of any of the parts concerned; and could the pupil be released from any of the membranes, which after iritis have blocked it up, by their being dispersed by operation, and the natural aperture of the pupil be thus made free again to dilate and contract in any degree? This position I have practically endeavoured to take affirmatively, and now I hold it so. That *synechiæ* are very prejudicial to the future integrity of the eye is my own conviction, *but this can add little* to the testimony of those who have so much

experienced authority; of posterior adhesion, Von Graefe has said "*that the principal cause of the recurrence of iritis is the existence of synechiæ, especially when broad and inextensible.*" Adhesions not only may induce a second iritis, but when the iris is first inflamed they may protract the recovery indefinitely; a single point of adhesion may do this, as in a case of mine in May, 1859, in which, with mild mercurials, a syphilitic iritis had ended in resolution, with no ill result but a single point of adhesion to the lens, all redness of the globe had therefore disappeared, but opposite this one band it remained exactly there, and there only for a long time afterwards.

Of the advantages of corelysis, whenever we may consider its possibility instead of making an artificial pupil, I believe the most important to be the optical superiority of a central active pupil, the appearance is also an advantage dependent, in fact, on much the same qualities as those for vision; these better optical qualities which almost alone need be considered being the natural position of the reinstated pupil, and the better recovery of its normal adjustment in size; this power of dilating and contracting being often perfect n cases in which the bands of adhesion have been few and narrow. Of the operation, I must say, in anticipation of the objections of those who believe the lens-capsule is so very easily torn, that in about fifty cases cataract has only once followed, when I have known for certain beforehand that the lens was clear; this case was one of the earliest on which I have operated, and the first spatula-hook I had had made and then used, was not perfectly rounded or well polished; of other ill results of the operation I say nothing, for I believe, if the cases promise well for the operation and all care is taken in its performance, there will be none, or that none have resulted that do not more frequently occur when the iris is operated on for an artificial pupil: of this I shall speak again. When I call the operation trifling, I do not mean that it is so to the operator; for him, I believe, it requires much consideration beforehand to determine upon its adoption, an examination by daylight and with atropine and the ohthalmoscope, the latter to decide if any space seeming clear is really unobstructed, or if occlusion seeming complete, really has no space left for the instrument to be admitted. Besides this perfect knowledge of the state of the parts, and so of what can be done, it requires the closest attention on the part of the surgeon, during the operation, to the exact place of the lens, and above all things that the end of the instrument introduced through the membrane or beneath the bands of adhesion should be then at once carried away from the lens and kept always forward. Lastly, of the advantages of corelysis, I must remark, that when other apertures of the body are obstructed by disease, we do not think of making a false passage, or a counter-opening if it can be avoided; and if corelysis is impracticable or unsuccessful, still as before, we can make an *artificial* pupil.

Corelysis should certainly not be attempted until the ciliary redness and other signs of inflammation are past; I do not propose it as a cure for iritis, though it may divert its return. The inquiry should be always first made before operating, if the lens is transparent or not; this, in the case of an occluded pupil, can only be learnt by the probabilities connected with the amount of vision left, but, of course, if the pupil is only limited by bands of adhesion, atropine and the opthalmoscope will go to prove an uncertain case. If the depth of the eye can be examined, it is well for a favourable conclusion of the case to know also that there is no cognizable vitreous disease or white patches in the choroid, &c. If, with adhesions of the pupil to the lens, cornea, or a false membrane, I have known the lens to be opaque (corelysis, *for its own sake*, is out of the question), I have proceeded to operate for the cataract, then always using atropine; if by *extraction*, the corneal section is made very readily, and without need of the chief ordinary precautions, then, and especially at its conclusion, necessary; a membrane filling the pupil may be opened out with one or two needles to make a way for the lens, as *the common* practice has been; or if separate points of adhesion of the pupillary margin exist, I have preferred my spatula-hook for their detachment. *If it is to be done by solution*: in the case of a false membrane, I have used

the fine needle or needles to open out the membrane (without enlarging the pupil), and then to the cataract itself; or in the case of bands partially confining the pupil, I have introduced the needle where they can be most conveniently reached, and with it broken them through as a preliminary step: two needles may, in such a case, be well used if bands of adhesion at opposite sides of the pupil are to be reached. The needle operation for the cataract with iris adhesions, and subsequent *linear-extraction*, may be well adopted if, as is often the case, the patient is young, and the inflammation has not been allowed to proceed to more serious injury than the limiting of the pupil. The patients are not so old as most of those who require an operation for cataract, so that "division" of the lens will have this advantage, and linear-extraction afterwards, if the pupil is large enough to allow of it, will curtail the task that by the sound eye may be better performed. This "extraction through a closed pupil" by a small or large section, however, is hardly connected with the subject of corelysis, but as a supplement to the latter operation when it has revealed a cataract not before known or believed to exist; indeed it is especially applicable to some cases in which corelysis, *for itself*, as I have mentioned, should not be attempted, those in which a cataract is known to exist, or the occluding membrane is quite opaque and far back in the contracted pupil, &c.; and corelysis, as I wish it understood, is for those cases in which the disease of the pupil is the only serious morbid affection that exists.

The instruments necessary for corelysis are, besides the spring-speculum and forceps with which to hold the eye, a broad needle, my spatula-hook, and perhaps canula, scissors, and forceps. The broad needle should be a small one, only to make an opening large enough, without cutting, to admit the other instruments (this may be proved beforehand with the aperture made by it in a piece of leather), so as to retain the aqueous humour as much as possible. The spatula-hook is figured and described at page 8 of the first volume of the 'Ophthalmic Hospital Reports:' the thickness of the spatula part, "the terminal quarter of an inch," should be equal, and not much reduced so that it may be very blunt (the instrument, especially at its extremity, and the hook, should be highly polished); the only difference I have made from the instrument figured is that the one I now use is rather less altogether in its width and every way; the spatula part should, moreover, be wider than the diameter of the round part of the instrument (made so next the handle for facility of turning it "in the corneal wound upwards or downwards"), that when it is in the anterior chamber, the aqueous humour may be better retained. The canula instrument should be the smallest, and well polished: the scissors should have blunt ends, and the *outer* side of the blade used should be kept next the lens during the operation.

Of the dangers of corelysis in anticipation of the operation, especially of causing cataract, I have already spoken, in comparing the reinstated pupil with iris-operations. I do not know how severe an injury of the lens-capsule is necessary to produce a cataract, but I am sure with instruments well made for the purpose I frequently touch it without any ill result; but, as I have said, I always keep the end of the instrument rather forward, that is to say, against the posterior surface of the iris or pupillary exudation, in detaching bands or a false membrane, that I may touch the lens as lightly as possible. I should not be so careful in this matter but for the difficulty of perceiving the minute distance between the iris and the lens, and of appreciating the slight changes of place which would bring the instrument upon the lens: so that, during corelysis, I constantly bear in mind to keep the end of my instrument away from the lens. I have, at the present time, a case at Moorfields in which the lens would, according to the common notion, have been expected to be changed from its transparency.

My method of operating is the following: atropine has been applied to the eye; the patient is recumbent, and I stand behind him. I have chosen a broad needle which is *suitable to* the spatula-hook, that is, as I have explained, *a little wider than it; the* patient has probably taken chloroform.

I secure with toothed forceps in the right or left hand as it may be most convenient, a fold of the conjunctiva close to the corneal margin on the side opposite to that at which the puncture is to be made, thus I draw the eye sufficiently in the desired direction and steady it. (It is best to secure a considerable fold of the conjunctiva close to the cornea, and to have a deep hold of it that it shall not give way; sometimes it is convenient not only to draw the eye with the forceps in an opposite direction to the proposed corneal puncture, but at the same time to rotate the globe somewhat on its antero-posterior axis: this may be done either way by having the conjunctival fold a little to one or other side of the direction in which the globe is to be drawn.) It is impossible, in the variety of cases in which corelysis is applicable, to say generally which way the globe is to be held, because, for instance, if an adhesion of the pupil is most conveniently to be reached from above it may be detached without any rotation of the globe, if it is so prominent that the brow will not interfere; or in another case the reflection from the cornea of the light of the window opposite the patient may make some rotation of the globe, when it is held with the forceps, more convenient than a change of place on the part of the operator. It is also impossible to say where the puncture should be made in the cornea, even at what distance it should be from the margin: this is, in each case, very important, for, when made, the corneal opening is a fixed point from which alone the operation has to be carried on to its completion. I have not generally made it very near to the margin (perhaps at a line from it), but my rule will be found by considering first, that a (posterior) adhesion should be always detached as close to the lens as possible; second and third (in this special instance), is the band corresponding to the position of the pupil when contracted or dilated? and so at what part is it of the convexity of the lens? of which the natural curve is to be considered (Fig. 1), as it should not be pressed upon by the instrument as it passes over it to reach the band at its point of attachment, the severance of which alone is to be attained; on the other hand, it can never be required to make the puncture far from the corneal margin, or the end of the straight instrument might come upon the lens, which it would be much more likely to injure than the flat side of the spatula would.

FIG. 1.

*a*. A posterior adhesion of the *contracted* pupil.
*b*. The same, of the pupil when it has been less contracted.
*c*. The normal iris.
*d*. The lens.
*e*. The spatula-hook introduced through the cornea in the best position to detach an adhesion at *a*,
*f*. The same, to detach an adhesion at *b*.
The figure is intended to shew that to detach adhesion *b*, the spatula-hook must not be introduced so near to the margin of the cornea (seen in section) as at *e*, or the adhesion cannot be reached *at the point of its attachment without pressing upon the lens.*

The easiest case of pupil-adhesion to be detached is when a single band connects the pupil with the lens, then the corneal puncture may be made most conveniently, perhaps (as in Fig. 2, *M. n.*) not exactly on the opposite side to that at which it is, or at a quarter of the circumference of the cornea, but between the two, that the spatula-hook may be advantageously used as a spatula or as a hook; as a spatula, that is to say, for its blunt edge, the instrument is most generally useful, and perhaps most safely or easily used, as the stretching of the iris that may occur, is thus more distributed than by collecting some part of the width of the iris, to which the band is adherent, in the hook to be severed at once. With the instrument entered in the direction *I have described*, the band, if it does not detach before the blunt edge, *will pass downwards* with the sloping direction of the instrument, and be ar-

rested by the hook, with which it can be then broken through. If two such bands exist at opposite points of the pupil, the spatula-hook will be most conveniently used when entered from a part of the cornea, towards its margin, midway between them (see Fig. 2, *N. m.*), and then, if the hook of the spatula is required for their detachment, the instrument must be semi-rotated when the first one is separated, before the instrument is applied to the second band. When the instrument is thus rotated in the anterior chamber (the aqueous humour should not have escaped and) the end of it should be brought forward towards the central part of the cornea, that as the blunt edge instead of the flat side of the instrument passes the lens, it may only touch it lightly, if at all. If many bands exist the instrument is introduced to most advantage from the side at which there is the greatest interval for its admission (see Fig. 2 *N. n.*), the division, of pupil attachments being always attainable excepting on the side at which the instrument is being used (Fig. 2, *M. m.*). The broad

Fig. 2.

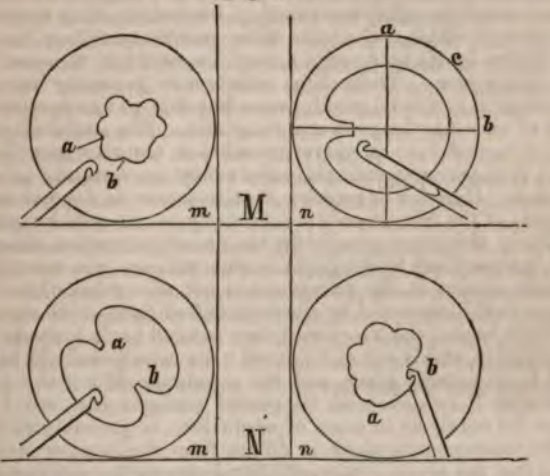


Fig. 2. The four diagrams show (magnified) various posterior adhesions of the pupils, all under the influence of atropine.

*M. m.* The six points of attachment, nearly equi-distant, are so much the more difficult; the two, *a* and *b*, cannot be easily detached with the spatula-hook introduced through the corneal margin thus most conveniently for the others.

*M. n.* The instrument, to reach the point of adhesion, is best entered where it is (or at *c*) and not at *a* or at *b*.

*N. m.* The spatula-hook should be brought up to the two opposite adhesions (*a* and *b*) from a point midway between them.

*N. m.* With such various adhesions the instrument is used to most advantage at the widest interval (between *a* and *b*) and can be passed (as it is shown at *b*) even behind those on the same side as the corneal opening.

needle to make the opening in the cornea, is directed towards the pupil and is introduced with a thrust, without cutting, as far as the widest part of its blade, to which its size in relation to the spatula-hook has been calculated beforehand; it is then quickly withdrawn, and the spatula-hook as quickly passed in by the opening that is made, the hook being on the upper or lower edge as may be required, the end of the instrument is brought up to the adhesion and passed under the margin of the pupil (as at *b*, in Fig. 2, *N. n.*) close to it and far enough beneath the iris; the end of the spatula being at the same time kept in apposition with it, the instrument is then brought towards the pupil up to the adhesion, not pressing the iris forwards or the lens backwards, but just between them, that the attachment may be released, as it generally will be very readily at its place, without tearing the iris or compressing the lens; in all this it must be remembered that the lens is not felt in its position, the direction of the *spatula-hook is a matter* of calculation throughout, the course

it is to take has been determined and is well known; and the iris or its pupillary adhesions will offer no appreciable resistance. If all the cautions I have given respecting the lens, appear contrary to what I have said of the unnecessary fear commonly entertained of its being wounded, I can only say, that by taking such care I have had the success which induces me so confidently to ask attention to the new proceeding, and I do not know that cataract would have resulted from less care-taking. The withdrawal of the spatula-hook should be, of course, with the flat part in the direction of the length of the corneal puncture, and it is effected without any chance of the hook being caught in the cornea, if the edge of the spatula, *without the hook*, is kept lightly pressed into the corresponding extremity of the linear wound.

The most important cases for corelysis remain to be considered. I have known vision more impaired with bands confining the pupil to the lens, than with a false membrane, leaving to all appearance in the pupil no free space whatever; the case of simple bands of posterior adhesion with very imperfect vision, may be possibly explained by various complications of the iritis, which has caused the adhesions limiting, but not blocking up the pupil; and the other case in which a complete false membrane filling the pupil exists with fair vision, by its being only a very fine network, thinnest at its centre, in the pupillary area. These false membranes generally have been recognized as surgical (artificial pupil) cases, but only to be operated on when serious loss of vision is the patient's complaint. The occluded pupils which I am about to consider, in an operative relation, are also most important, inasmuch as they are most difficult, and as they are certainly irremediable but by operation. Here I will premise that it seems to depend very much on the diagnosis of the false membrane, if corelysis can effect a cure when the pupil is filled; the cases suitable for the operation are not those in which a perfectly opaque patch in the pupil is seen with an iris, more or less standing forwards around it, the membrane is not of a dead white, but gray by daylight, or half illuminated by the transmitted light of the ophthalmoscope, and it is little behind the iris, which does not fall back towards it, or is not bulged around it, and is not discoloured; the condition I allude to, in which the iris is in its natural place, and the membrane filling the pupil is about on a level with it (which, with its partial transparency, are, I believe, the indications for corelysis in cases of occlusion), is probably to be explained by two circumstances in which it differs from the opaque membranes with prominent iris, the closed communication between the anterior and posterior chambers not being altogether impervious to the aqueous humour, and the membrane not being firmly attached behind to the lens-capsule, so that there, as it is not closely adherent, some fluid transition is allowed. Indications favourable to the operation, are the evident contractions of the fibres of the iris when the eye is exposed to a strong light, a large anterior chamber and the (occluded) pupil not very small, improvement of vision, so as to read large type, by the use of atropine, &c. My best results have been, I think, in cases of young persons who have been judiciously treated for a first attack of syphilitic iritis, and to whom some mercury has been already given. If the false membrane seems altogether to occupy the pupil, it must be examined with atropine and the opthalmoscope, for the discovery of any possible gap in it, by which the spatula-hook may be introduced, a chink even smaller than the end of the instrument may be made to admit it, and the membrane is torn through with the hook of the spatula, or divided with the canula scissors. If the hook of the spatula is to be used, the instrument must be introduced through the cornea, on the *opposite* side to that at which the opening is in the membrane, and in this way I have generally concluded the operation at once: the canula scissors can only be used when introduced from the *same* side of the cornea as the opening in the membrane, and probably, only with safety after the spatula has been passed between the membrane and the lens; this method, therefore, I have not followed since the case of Sharp, I have referred to. When no space at all is left in the occluding membrane, the plan I have adopted is to introduce the broad needle through cornea, in any direction which may appear to be lengthwise of the mem-

brane, then to advance the needle across the pupil to the opposite side of the membrane, and transfix it just within the pupil close to the iris, but carefully not to cause bleeding from it, raising the membrane from the lens with the needle as soon as the point has pierced it, and advancing the needle a little to make the opening larger, whereby there is no greater fear of wounding any part, as the needle has been passed *across* the pupil, and the convexity of the lens, and beneath the iris. Because of the spherical prominence of the lens which is close behind the pupil, it is clearly impossible with safety to pierce the membrane with the needle on the same side as that by which it is passed through the cornea, but I do not know whether it would be safe in any case, and the pupil could be cleared by passing the canula scissors at once through the cornea, and the blunt point through the crevice, *when there is one*, in an occluding membrane, without previously using the spatula to lift the membrane and separate it from the lens, by all which the aqueous humour is so much let escape, that as in Sharp's case, the scissors could only be adopted at a subsequent operation, which, of course, it is very desirable to avoid. I passed the spatula-hook once through the membrane, and close behind it, to separate any filaments that might connect it with the lens, but I did not venture, at that time, to use the scissors, because of the diminution of the anterior chamber which had then resulted. I am inclined to the adoption of my early plan, of passing the spatula *between* the lens and the membrane, *before* the latter is operated on, in every case.

As to the after-treatment of a case in which the pupil has been freed, I have no special knowledge; I have had atropine applied after the operation night and morning, the eyelids closed, to insure rest to the eye, perhaps with a bandage and cold water at first, with a folded rag constantly until any ciliary redness has faded, or it is no longer agreeable to the patient. I have never known any more extensive inflammation than that of the iris itself, follow corelysis: in several cases, by this consequent iritis, the pupil has again become occluded or bound to the lens by adhesions, and then no benefit has resulted: in two or three, the pupil has been more obstructed than it was before the operation; and if it is not, at once, subject to treatment, this traumatic iritis is fatal to the operation and altogether hopeless: leeches and other antiphlogistic treatment are the means I have employed for its reduction.

In operations generally on old remains of lens-capsule, &c., in the pupillary area, either long after operation for cataract or wound of the lens, I have learnt to fear especially two consequences that are not uncommon or inconsiderable; either the protrusion of the vitreous humor into the pupil, through the opening made, or chronic inflammation in the ciliary region, perhaps consequent upon or coexistent with the former, or, as I have thought, by dragging upon the suspensory ligament. Both of these ill-results may occur, the former on account of the transparency of the vitreous humor is often not known to have happened, and the latter is only manifested some time after the operation. Once lately I discovered that the former had taken place by the bleeding which occurred from (?) the ciliary processes: the blood filled the empty aqueous chamber except in the centre, where the vitreous had come forward and was pressed against the back of the cornea (I had torn open, with two needles, some remains of capsule, left in the pupillary area, after extraction of cataract), the blood surrounded the vitreous as a red ring before the iris, and through the centre of the cornea the clear vitreous humour and the pupil "opened out," the patient saw at that time very well. To obviate this projection of the vitreous humour into the aqueous chamber, which some have thought to be actually advantageous, when a false membrane (lens-capsule, &c.) is rent, as in the case I have just mentioned, the needles should not be passed farther than is just necessary through the membrane, or it should be divided with canula-scissors; it should not be lacerated very extensively, or repeatedly, and in the cases also of old traumatic cataract (remains of lens and lens-capsule) once operated on, it is best to be satisfied with a fair result for vision although some portions remain in the pupil *which might be removed*; because these portions of lens-cap-

sule, &c., after extraction or wound of the lens are *held in the ciliary region, by means of the (suspensory) ligament*, which has been perhaps confirmed and toughened by the former operations, and in their removal the vitreous humour is always likely to come forward and perhaps also irritation of the ciliary body, is induced. Considering this to be so, and the ill-results there have been, I devised the method of tearing through the ligament *in detail, before* attempting the removal of the obstructing lens-capsule, &c., it is obvious that if it is seized at once by the forceps, as it has been usual to do in these cases, the instrument has one-half, more or less, of the suspensory ligament

Fig. 3.

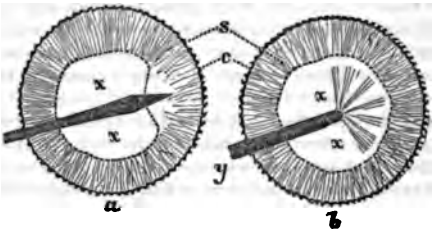


Fig. 3. *c.* ciliary process; *s.* suspensory ligament; *x x*, *x x* shrunken lens-capsule, remains of lens, &c.

*a*, shows the manner of separating with the needle (*z*) the lens-capsule at its margin, in detail.

*b*. shows how advantageously the canula forceps (*y*), are *opposed* by the retaining influence of the suspensory ligament, in such a case as the foregoing.

opposed at once to its traction, and acting very favorably *against* the removal (diagram, *b*, Fig. 3,) thus in fact, it is well adapted to support the healthy lens, and as I have said, it is, in an abnormal state, probably stregthened; however, with force applied at some one part of the ligament it is easily there torn through and the needle-point will effect this at various parts, and so entirely separate the lens-capsule, &c., and leave it to be removed with the canula-forceps, with little disturbance of the vitreous humour and without irritation of the ciliary body. In other surgical operations the *gradual* separation of parts to be removed is the rule whenever it is applicable, and in ophthalmic surgery it is often to be followed; the iris itself must not be dragged upon to a great extent at once; and so, in corelysis, as I have said, I have always avoided its ciliary separation (iridodialysis), or any laceration of its substance in detaching the strongest adhesions of the pupil, *by taking them in detail.—Ophthalmic Hosp. Reports, No.* 11, *p.* 309.

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## 84.—ON IRIDECTOMY IN GLAUCOMA.

By William Bowman, Esq., F. R. S., Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital, Moorfields.

[In the 'Dublin Quarterly Journal of Medical Science,' a review lately appeared intended to discredit the treatment of glaucoma by iridectomy, as advocated by Von Graefe. Mr. Bowman was the first English surgeon who applied this treatment, and being convinced of its importance, has since constantly advocated its employment. The term 'Glaucoma,' as now understood, embraces much more than the etymology would import. Mr. Bowman says:]

The treatment by iridectomy has been no hap-hazard guess, but a rational conclusion gradually worked out. The dominant idea guiding the mind to the appropriate remedy has been that of the existence of augmented intra-ocular pressure, as the main characteristic of the glaucomatous process. In fact, whatever the essential nature of that process, its concomitant seems to *be a marked tendency* to hardness or tension of the eyeball: and thereupon *arise the most serious* secondary effects,—paralysis of the retina, oftentimes

interrupted circulation, congestions, inflammatory attacks, with their various consequences,—ending sooner or later, and with more or less intermissions, in total loss of sight, and a spoiling of all the tissues of the eyeball. The general states met with in practice and falling under the general heads of amaurosis with excavation of the optic nerve, chronic, subacute, and acute glaucoma, all seemed to be allied to one another and may be termed glaucomatous diseases." Some are very slow in their progress, and the loss of power in the retina very gradual; there is no inflammation or even congestion: in others there is congestion, slight or considerable, intermittent or continuous. In others, again, acute and intense inflammation arises. A glaucomatous state may also come on in the course of other diseases, choroidal, retinal, or cataractous. To relieve intra-ocular pressure seems to be the prime indication in all, and it is rational to suppose that it will be the more effectual, the less deterioration of structure the retina has previously suffered. To relieve this pressure is to disarm the glaucomatous state of its chief peril, and apparently to restore the eye to the influence of the reparative powers belonging to it as a living organ. The tension once relieved, and so long as it remains so, the circulation tends to its natural equilibrium, and the retina which has been compressed recovers itself more or less, or ceases to degenerate, in a degree usually corresponding inversely with the intensity and duration of the preceding pressure.

It is certain that this critic would never have attacked the new practice had he really read Graefe's memoirs, or believed the cures reported by others, to be genuine. He says, "Graefe's doctrines, as published by the Sydenham Society, are plausible, but in a practical science of this nature we require something more than doctrines; cases must be given, and Graefe was wiser than his countrymen* of the London Ophthalmic Hospital, and kept his cat in his bag." Will it be believed that the reviewer can have read the memoirs of Von Graefe which he criticises, which are enriched with numerous cases, fully detailed and epitomised, and constantly referred to, illustrating all the main doctrines advanced? As for the experience acquired in England, I shall at present speak only for myself; and I can assure the reviewer, in the most decided terms, of the reality of the influence of this operation in relieving, even permanently, the unnatural tension of glaucomatous eyes, and of its effect in arresting the glaucomatous process, and often in restoring sight in a marvellous manner. My own cases in the hospital and in private have been numerous, and bear out the above general statement. This it is that makes me so anxious that the profession should be rightly informed as to the signs of glaucoma in its relievable stages, and should be ready to sanction the timely application of the only known real remedy. I can hardly conceive a greater comfort in practice than to be able, by this operation, to rescue sight thus imperilled, and to escape the disheartening task of treating a disease, evidently tending to blindness, by the old, ineffective remedies. The reviewer may class the eminent author of so great a boon with "Hahnemann, Preissnitz, and De Leeuw," but he may depend upon it he will be powerless to "arrest the spread of the 'glaucoma epidemic' in England." I could, indeed, heartily wish that when he has further examined the evidence, and dispassionately perused what he criticises, he will make trial of the operation for himself, in which case I doubt not he will be satisfied with his results.

In speaking in these terms of iridectomy in glaucoma, I must carefully guard myself from being supposed to uphold it as a sovereign remedy equally valuable in all forms and stages of the disease, and under all its various complications. At present the operation is being extensively tried in cases where augmented intra-ocular pressure evidently exists; and the proved tendency of the operation to relieve this injurious tension, the certain cause of secondary mischief, makes it our duty to employ it, when the patient is otherwise apparently drifting into inevitable blindness. It may be our

* Referring to the report of cases of operations for Glaucoma at the Moorfields Hospital, given in the 'Ophthalmic *Hospital Reports*,' by my friend Dr. Bader, who is quite able to defend himself.

misfortune, and that of our patients, that our experience is as yet less complete than a few more years will make it; but meanwhile we can only act on the light we possess, and store up the results for the future benefit of others.

While the modern doctrine and treatment of glaucomatous affections is so new, it is to be expected that errors in the diagnosis may occur even among well-instructed practitioners; and the operation is doubtless in some danger of being brought into discredit by being undertaken in cases to which it is not rationally applicable, or by being defectively executed. I do not see how this can be avoided; it is the necessary fate of all novel proposals, and this one must face the ordeal. It is the part of intelligent men to make allowance for these incidents of human progress, rather than to allow them to prejudice the march of truth.

In the present state of our knowledge it may be right to try the operation experimentally in some cases to which more extended experience may show that it is not usually applicable. Experience hitherto shows that it is most valuable when performed *early in acute cases*, in which turbid effusions exist, the absorption of which goes on rapidly when the tension is relieved. Temporising measures in such cases, such as bleeding, mercury, colchicum, and even the simple puncture of the eye, whether of the aqueous or vitreous regions are not to be relied on, and occasion dangerous delay. It is also most useful if performed in the subacute form, when the visual field is not as yet seriously contracted, but where the symptoms are steadily advancing. If postponed here till the sight becomes greatly impaired by gradual alteration of the tissues of the retina, and of the optic nerve-entrance, the ultimate advantage is smaller, though usually lasting. In many instances we are called on to treat patients already almost blind from the progress of the disease under one or other of its various forms. Here it has often been the means of sparing them a little sight, sometimes permanently, at others not. I have also tried the operation in some examples of blind and tense glaucomatous globes, the seat of excessive or wearisome pain, and on several occasions have been well satisfied with the result. How intractable such cases are apt to be under the usual palliatives is well known.

As for the operation itself, a large experience has convinced me that, though usually a simple proceeding, not liable to accidents, it is occasionally one of the most delicate and critical in its nicety of all the operations on the eye. The anterior chamber has to be entered at its extreme rim, where the sclerotica overlaps the cornea, close in front of the iris, which often bulges much, and which must on no account be pricked, lest the vitreous humour or the lens, immediately behind, should be wounded; in either case a most serious complication. The bulging of the lens, in many cases, and the dilatation of the pupil, add to the risk of injury to the lens. A little blood, too, escaping into the anterior chamber, either before or after the excision of the iris, may obscure the parts from view, and add to the chance of involving the vitreous humour or lens. It is obvious that these difficulties and contingencies ought to be acknowledged and recognised in order that they may be guarded against. In very few instances indeed can they lead to accidents in skilful hands. When blood occupies the anterior chamber in the course of the operation, it is wise to remove it, either by a gentle stroking movement of the *curette* over the cornea towards the incision, or, if that do not suffice, by carefully introducing the end of the *curette* within the incision itself. On two occasions only have I known the lens to become opaque after the operation, where the capsule was not directly injured (as it ought never to be) by the instrument. One of these was in my own practice, another in that of a highly esteemed colleague.

I am unwilling to extend this already long comment on the review by any inquiry as to the *rationale* of the operation; but there is much in favor of Von Graefe's original suggestion, that the diminution of intra-ocular pressure may be largely due to the lessening of the iris as the surface secreting the aqueous humour. My own opinion, however, was, and is, that the more *direct communication* opened between the vitreous and aqueous regions of the *eye facilitates* the play of currents between them, and thus allows an excess

of fluid behind to come forward to the corneal surface, through which exosmosis is much easier than through the posterior coats, the sclerotic, choroid, and retina. This would go far to explain the apparently contradictory influence of the operation in raising the tension to the natural degree when previously diminished; for this also it is capable of doing in some cases, as shown long ago by Von Graefe himself. That the size of the piece excised in glaucoma has a direct relation to the effect produced is true according to his latest observations, confirmed I believe, by those of Donders, and of Arlt, the distinguished and most able professor of Vienna. I can also add my own corroborative testimony. Von Graefe is thus led (in his latest memoir, just published) to insist once more, as he had done in his first paper, on the necessity of removing a considerable portion of the iris (not, however, so much as a third, or even a fourth, of its circuit), where it is wished to reduce permanently the intra-ocular pressure. A small iridectomy is insufficient, much less any mere puncture, such as some are still inclined to rely upon.

In conclusion, I may remind the reviewer, that after the reading of Mr. Hulke's excellent paper on the Pathology of Glaucoma, at the Medical and Chirurgical Society, on January 12, 1858, both Mr. Critchett and myself endeavored, from our own experience, to enforce the importance of Von Graefe's new proposal (see the 'Medical Times and Gazette,' January 23), and that in our weekly operations at Moorfields, all comers have had the opportunity ever since of watching the progress of our numerous cases, as well as of those of our colleagues. Mr. Hulke advocated and explained this treatment in detail in an able paper, "On the Surgical Treatment of Glaucoma," in the 'Medical Times and Gazette,' March 27, 1858, (see 'Retrospect,' vol. xxxvii, p. 194), referring to the favorable experience of Moorfields and to my own private practice, in which at that time I had the advantage of his assistance. He has recently reverted, to the subject in a second paper at the Medical and Chirurgical Society. To both these latter communications I venture to draw the attention of practitioners. The reviewer is in error when he states that there has been any holding back of opinions or of facts on the part of those who have introduced the operation into England.—*Medical Times and Gazette, Aug.* 25, 1860, *p.* 177.

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## 85.—ON GLAUCOMA AND ITS SURGICAL TREATMENT.

By J. W. Hulke, Esq., F.R.C.S., Assistant Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital.

[Our readers will do well to refer to previous articles on the subject, by Mr. Hulke, in vol. xxxvii. p. 194, and vol. xl, p. 173.]

*Premonitory symptoms.*—Rapidly increasing presbyopia; the appearance of a coloured halo round the flame of a candle; the spontaneous appearance of flashes and other spectra. Intercurrent obscuration of vision, attended with vague orbital and frontal pains, slight hardness of the eyeball, and contraction of the field of vision. The pupil is large and sluggish; the size of the anterior chamber is much diminished. The duration and intensity of these symptoms are very variable, but they are rarely absent.

*Acute glaucoma.*—The active stage sets in as a sudden and violent outbreak, often at night. Violent racking pain in the eyeball, often attended with sickness, and followed by rapid extinction of sight. The pupil is widely dilated and motionless; and the lens has sometimes the peculiar greenish tint which was formerly considered so characteristic. The ciliary vessels are swollen; the conjunctiva is red and often chemosed; the globe is very hard; the cornea is dull, and its sensibility is lowered. Remissions are followed by fresh paroxysms, and complete, irremediable blindness always ensues.

*Chronic glaucoma.*—The premonitory period slowly glides into the active. The obscurations, which were at first evanescent and separated by long in-

tervals, become more frequent, and last longer. The contraction of the visual field progresses. The tension of the globe increases. The iris becomes dull; the aqueous humour turbid; the cornea dimmed and flattened. Mr. Hulke lays stress on the flattening of the cornea, which is easily demonstrated, because it has been recently stated that the cornea becomes conical in glaucoma.

*Ophthalmoscopic signs.*—Excavation of the optic nerve entrance, and pulsation of the retinal vessels. To these capillary apoplexy of the retina is often added; and sometimes there are small blood-clots in the vitreous humour, which is unnaturally firm. It is only late in the disease, when all the component structures are undergoing atrophy, that the vitreous humour becomes fluid.

*The nature and causes of the glaucomatous process.*—All the leading features of the glaucoma are due to excessive tension of the eyeball from a superabundance of fluid within it, which distends the vitreous humour. This fluid—serum—is derived mainly from the choroid. Many circumstances show that the retina is only passively concerned. Glaucoma might be considered a serous choroiditis. Mr. Hancock has advanced the theory that spasm of the ciliary muscle forms an essential part of glaucoma. The author has, however, found complete atrophy of this muscle in some dissections of glaucomatous eyes; hence the inference that this muscle is not concerned in maintaining the glaucomatous condition. The author has been unable to trace any connexion between glaucoma and gout or rheumatism. Some other diseases and injuries of the eyeball occasionally assume a glaucomatous type. This is especially the case with wounds of the ciliary region and sclerotico-choroidal staphyloma.

*Treatment.*—Generally the age and broken health of the subjects of glaucoma forbid antiphlogistics; venesection is inadmissible; leeches and counter-irritants are useful as adjuncts, but cannot alone cope with the disease. The excessive tension of the globe is suggestive of the evacuation of some of the superabundant fluid by tapping. The old surgeons, Antonius, Nuck, Jobus, à Meekren, and others, were familiar with this operation, but they practised it chiefly in hypopion, onyx, and hydrophthalmos. Wardrop ('Med.-Chir. Trans., 1813,) tried it extensively. With a view to lessen fulness and congestion, he tapped the anterior chamber in superficial and deep-seated inflammations of the eye. The operation was at first warmly taken up by other surgeons, but soon fell into disuse. In our own day it has been strongly advocated by Desmarres, but it has found little favour with English surgeons, though most have occasionally performed it. In glaucoma the relief that paracentesis cornæ affords is too transient to render it of much value. Paracentesis scleroticæ has been practised by Desmarres and Hancock in glaucoma, though with different objects. Mr. Hulke reverts to this, after fully describing the operation of iridectomy as proposed by Dr. A. von Graefe.

*Iridectomy* consists in excising a segment of the iris, in its whole breadth, from the pupillary margin outwards to its insertion. This is effected through an opening of corresponding size at the extreme edge of the anterior chamber. Iridectomy may be practised at any part of the iris. Graefe usually makes it outwards; but adds that, when desirable for the sake of appearance, it may be made upwards. This latter position has been adopted by Mr. Bowman, and is that which Mr. Hulke has generally chosen. By removing the iris in this manner, the pupil is at once enlarged up to the corneal incision, which forms, as it were, the base of a coloboma iridis, and the edge of the lens, with the suspensory ligament, stretching in front of the vitreous humour and the ciliary processes, are exposed to view. The little blood which oozes into the anterior chamber from the cut edges or surface of the iris, should be at once pressed out or removed with a scoop. The after-treatment is very simple. A light compress may be applied for a short time as a precaution against hemorrhage. This may be replaced after an hour or two by a piece of wet rag. The room should be shaded. Usually nothing else is necessary. At first the aqueous humour trickles away; but the

corneal wound soon heals, and the anterior chamber fills again. The hardness of the eyeball is at once lessened, and a natural tension is gradually attained; the pain abates, and soon altogether disappears. As regards vision, the ultimate results are intimately dependent on the period at which the iridectomy is performed, being more perfect where it has been early undertaken than where it has been postponed. In the premonitory period, where the symptoms are well marked, the propriety of operating cannot be doubted. In acute glaucoma, where the operation is done during the first inflammatory attack, or soon afterwards, vision is very completely restored. In chronic glaucoma, the results are less uniform and less decided. This is in consequence of the insidious nature of the disease—structural changes in the retina creeping on *pari passu* with the gradually increasing tension.

*Alleged objections to iridectomy.*—1. Its *reported* uniform failure in the hands of some surgeons. This is in great probability to be generally attributed to its having been practised in cases which were not true instances of this disease. Many failures have proceeded from its having been done at far too late a period.

2. The great difficulty of the operation. This has been much magnified. It does not require more skill than most surgeons possess, and when chloroform is used it becomes really a simple matter; but even were it difficult, which it is not, in the absence of other known means of cure, we should be no more justified in rejecting it on this account, than we should be in refusing a patient the benefit of herniotomy where the taxis and other measures had failed.

3. The disfigurement produced by the coloboma iridis is so slight that it cannot constitute a real objection.

4. Its supposed injurious action on accommodation. Further experience has corrected some misimpression which at first prevailed respecting its influence on the adjustment of the eye. The previously existing presbyopia is not increased by removal of a portion of the iris; indeed, the refracting power of the globe sometimes actually increases after iridectomy—probably, as Graefe has shown, in consequence of the flattened cornea resuming its natural curvature.

To avoid these alleged disadvantages, paracentesis scleroticæ has been advocated by Middlemore, Desmarres, and Hancock, as a substitute for iridectomy. Middlemore proposed to evacuate the *turbid, diffluent, vitreous humour* with a grooved needle, and to replace it with a syringeful of clear water. But, except in very old cases, the vitreous humour is much too firm to flow out along a grooved needle; and probably few English surgeons would adopt Desmarres' suggestion, of introducing a probe and breaking it up. Mr. Hancock, considering a spasm of the ciliary muscle to be an essential part of glaucoma, divides this muscle by striking a knife through the ciliary region backwards and inwards towards the axis of the globe. But Mr. Hulke has demonstrated, by microscopical examination, advanced atrophy of this muscle in many glaucomatous eyeballs; whence it follows that the ciliary muscle is not actively concerned in maintaining the glaucomatous process. In all probability, the success of Mr. Hancock's operations is solely due to the draining away of some of the superabundant fluid. According to this view it is simply a peculiar mode of paracentesis, and cannot rank as a substitute for iridectomy until it has been thoroughly established that it *permanently* relieves excessive intra-occular tension, which, in common with most surgeons, Mr. Hulke has found that tapping the vitreous humour fails to do.—*Lancet, July* 21, 1860, *p.* 61.

## 86.—ON THE DIVISION OF THE CILIARY MUSCLE IN THE TREATMENT OF GLAUCOMA AS COMPARED WITH IRIDECTOMY.

By Henry Hancock, Esq., F.R.C.S. Senior Surgeon to the Royal Westminster Ophthalmic Hospital.

[A great difference of opinion exists between Mr. Hancock, Mr. Hulke, and others, on the new operations on the eye for the relief of glaucoma. We cannot pretend to decide between these eminent ophthalmic surgeons, and therefore we prefer to let each party speak for itself. Mr. Hulke, in some very talented papers, has called in question Mr. Hancock's practice; and here Mr. Hancock defends himself, and, we confess, makes out a very good case. Mr. Hulke's operation is like that of Von Graefe's, namely, iridectomy. Mr. Hancock says that his operation of simply dividing the ciliary muscle is sufficient. He maintains that the appearances of the blood-vessels in glaucoma are due chiefly to the constriction exercised by the ciliary muscle, and that to remove this constriction it is only necessary to divide the muscle, and that this is the best operation. He says:]

My first operation for the division of the ciliary muscle, was performed on the 9th of September, 1859; and on the 11th of February, 1860, I published an account of the operation, with cases; at the same time stating that I was led to adopt this mode of treatment from the belief "that the pathological and ophthalmoscopic appearances of the blood-vessels in glaucoma were due mainly to the constriction exercised by the ciliary muscle;" and that to remove this constriction I operated as follows:—"I introduce a Beer's cataract knife at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided." This is the description of my operation which I published: it does not contain one word about "striking a knife through the ciliary region towards the axis of the globe," which is the incorrect version given by Mr. Hulke—a proceeding which would be carefully avoided by any one conversant with the anatomy of the eyeball, from the risk of wounding the lens which would inevitably attend it.

As my operation has now been before the profession above twelve months, and still continues to be attended with the best results in the hands of my colleagues and myself, I cannot admit that iridectomy *is* the only known remedy for glaucoma. I propose, therefore, to consider, in the first place, the validity of Mr. Hulke's objections to my operation, as set forth in the paper already alluded to, and read before the Royal Medical and Chirurgical Society. Next, to inquire into the results of the operation of iridectomy, as furnished by Dr. Bader's papers in the 'Ophthalmic Hospital Reports' (Nos. 9 and 10); and lastly, to give the results of my own operation up to the present time, with my reasons for submitting it to the profession, not as the "only known," but as the *best* operation for the treatment of glaucoma.

1. The principal points upon which Mr. Hulke insists are, "That the leading features of glaucoma are due to excessive tension of the eyeball, from a superabundance of fluid within it, which distends the vitreous humour; that this fluid (serum) is derived mainly from the choroid; that it might be considered a serous choroiditis."

2. "That the excessive tension of the globe is suggestive of the evacuation of some of the superabundant fluid by tapping."

3. That he has "demonstrated, by microscopical examination, advanced atrophy of this muscle (ciliary) in many glaucomatous eyeballs; whence it follows that the ciliary muscle is not actively concerned in maintaining the glaucomatous process."

4. "To avoid certain alleged disadvantages, paracentesis scleroticæ has been advocated by Middlemore, Desmarres, and Hancock."

5. "*That in* all probability the success of Mr. Hancock's operation is *solely due to* the drawing away of some of the superabundant fluid. Ac-

cording to this view, it is simply a peculiar mode of paracentesis, and cannot rank as a substitute for 'iridectomy,' until it has been thoroughly established that it permanently relieves excessive intra-ocular pressure," which, in common with most surgeons, Mr. Hulke has found that tapping the vitreous humour fails to do.

The assertion "that the leading features of glaucoma are due to excessive tension of the eyeball from a superabundance of fluid within it," and that "excessive tension of the globe is suggestive of the evacuation of some of the superabundant fluid by tapping," are contradicted and rendered untenable by other portions of Mr. Hulke's paper; whilst the results he gives of the operation of iridectomy directly prove, that the operation is only of value when the fluid, for the evacuation of which it is performed, is actually not in existence to be evacuated.

We are told that the reason why iridectomy has failed in the hands of some surgeons "has proceeded, in many instances, from its having been performed at far too late a period....that the propriety of operating in the premonitory period cannot be doubted....that in acute glaucoma, where the operation is done during the first inflammatory attack, or soon afterwards, vision is very completely restored; whilst, in chronic glaucoma, the results are less uniform and less decided."

We may hence infer, that three stages of this disease are recognised: the premonitory, the acute, and the chronic, and that the success of the operation is greater the earlier it is performed. It is this great practical fact which seems to me to be fatal to Mr. Hulke's theory of glaucoma being due to a superabundance of serum distending the "vitreous humour," more especially as that gentleman, in the same paper, describes the vitreous humour as being "unnaturally firm in this disease;" and that it is only at a late period, when all the component structures are undergoing atrophy, that the vitreous humour becomes fluid, at which time the results of the operations are admitted to be "less uniform" and "less decided." And I am still further supported in the opinion I have expressed here and elsewhere, that fluid is not the cause, but the result, of the disease termed glaucoma, by the following very corroborative paragraph, extracted from Mr. Hulke's paper on the 'Pathology and Morbid Anatomy of Glaucoma,' read before the Medico-Chirurgical Society, Dec. 12th, 1857:—"With a view to relieve the tension of the globe, I have seen the sclerotic freely punctured with an extraction knife, after which firm counter-pressure with the finger upon the opposite side of the globe only caused the protrusion of a very small bead of yellowish vitreous humour, such great firmness had it." Whether the tension of the globe was relieved by the puncture is not stated.

Nor is it by any means so conclusive as Mr. Hulke appears to imagine, that, because he has "demonstrated advanced atrophy of the ciliary muscle in many glaucomatous eyeballs, this muscle is not concerned in maintaining the glaucomatous condition." The word "many" is very indefinite and inconclusive. Mr. Hulke does not state in *how* many instances he has found this muscle atrophied, or what proportion these instances bore to the number of glaucomatous eyeballs which he examined microscopically. He does not inform us whether these glaucomatous eyeballs were obtained after the death of the patient or before, or, if from the former, the time which had elapsed between the death and the examination; for the changes which take place, especially in diseased eyes, are so rapid, that very little reliance can be placed upon such examinations when they have been deferred for any length of time. Neither, where the glaucomatous eyes which he examined had been obtained from living patients, does he tell us the stage of the disease at which they were extirpated, or the circumstances which necessitated their extirpation. It had been but justice that these particulars should have been mentioned before a sweeping pathological statement was unhesitatingly advanced.

I have already alluded to the three stages of glaucoma described in Mr. Hulke's paper—the premonitory, the acute, and the chronic; to his admission that it is only *late in the disease* that the component structures of the

eyeball undergo atrophy; and that whilst iridectomy is most successful during the first two stages, its results are less uniform and decided in the last; whilst Dr. Bader states, in his report, "that the prognosis of chronic glaucoma depends upon the stage in which the eye affected is operated upon: when blind for some time, it is not expected to retain sight; a chronic glaucomatous eye, with mere perception of light, is rarely improved by operation.

Mr. Hulke's microscopical examinations, therefore, cannot be admitted as at all conclusive or of much value against my proposition, "that the ciliary muscle exercises considerable influence in maintaining and aggravating the glaucomatous condition," and still less against my operation for the division of this muscle. They only prove that in certain cases of advanced glaucoma, in which an operation is admitted to be rarely successful, the ciliary muscle may have undergone atrophy with the rest of the tissues; but they by no means prove that this is the case in all or even in average instances, as we cannot for one moment imagine that any surgeon would extirpate a glaucomatous eye capable of relief by operation, unless under very peculiar circumstances. I presume it is only recently that Mr. Hulke has discovered this condition of advanced atrophy of the ciliary muscle in glaucomatous eyeballs, otherwise he would scarcely have omitted all mention of a fact of so much importance in his paper upon the "Pathology and Morbid Anatomy of Glaucoma," read before the Royal Medico-Chirurgical Society in December, 1857.

On the other hand, my friend and colleague, Mr. Hogg, has kindly furnished me with the particulars of two glaucomatous eyeballs, extirpated at an advanced period of the disease, in which his microscopical examination demonstrated the ciliary muscle as highly developed, and anything but in a state of atrophy. I do not, however, advance these cases as of themselves sufficient to controvert Mr. Hulke's assertion—their number is too small; but, at the same time, they afford pretty conclusive proof that the ciliary muscle is not atrophied in all cases even of advanced glaucoma. Neither can I admit that the success of my operation in any way depends upon, much less is solely due to, the drawing away of some of the superabundant fluid; or that it is a peculiar mode of paracentesis, to be classed with the operations of "paracentesis scleroticæ" of Middlemore and Desmarres.

I think I have succeeded in showing that, according to Mr. Hulke's own statements, there is no superabundance of fluid in those cases most likely to be benefitted by the operation; therefore the drawing away theory falls to the ground, and is still further negatived by cases No. 15 and 16, appended to this paper.

The object for applying the term paracentesis scleroticæ to my operation, and classing it with the procedures of Middlemore and Desmarres, is transparent enough. Paracentesis of the cornea and sclerotica, as practised by these two surgeons, has not met with any amount of success. If, therefore, the profession could be impressed with the notion that my operation was nothing more than one or the other of these proceedings, and that any transient good which it might be the means of effecting was due to the mere evacuation and draining away of fluid, it would be looked upon as deficient both in value and originality, and would, as a matter of course, fall into disrepute, and proportionately give greater prominence to the operation—iridectomy. It is true that the word paracentesis means literally, a "piercing through," but its application in surgery has hitherto been restricted to the operation of tapping. If we were to attempt to describe the operation of tenotomy in club-foot as "a peculiar mode of paracentesis" of the leg, or of the foot, or if we were to designate the operation of extraction of hard cataract as "a peculiar mode of paracentesis" of the cornea, we should expose ourselves to the charge of pedantry; yet the name may with equal propriety, be applied to these operations, or even to iridectomy itself, as to my operation for the division of the ciliary muscle. Hence the term as *applied to this* operation by Mr. Hulke is a misnomer.

*The operations* of Middlemore and Desmarres were introduced for the

avowed purpose of relieving intra-ocular tension by the evacuation of fluid. My operation, on the contrary, is introduced for the avowed purpose of relieving the constriction of the several coats of the eye by division of the ciliary muscle.

An increased quantity of fluid may or may not be present, and, when present, some may flow by the side of the knife; but this is merely a coincidence, not by any means the primary object of the operation; for mere evacuation of fluid without division of this muscle is quite incapable of affording permanent benefit.

In my former paper I pointed out the variety of opinions entertained by the supporters of iridectomy in this country as to the *modus operandi* of the operation. I would here venture to suggest another. I believe it will be found in the course of time, that the element of success is the same in iridectomy as in my operation—viz., "the division of the ciliary muscle." That, from the situation in which Von Graefe makes his first incision, he at the same time cuts it through, and I have little doubt it will ultimately be found that the extent of this incision may be advantageously curtailed, and the tearing away of the iris altogether dispensed with.

[Mr. Hancock then goes on to say in another part of his paper:]

I have now to offer a few observations upon the results of my operation up to the present time.

I also propose to add the present condition of several patients upon whom my operation was performed some months; and I do this with the greater confidence since Mr. Hildige, in commenting upon the operation when first announced, fell into the error of assuming it was mere paracentesis, which he asserted produced merely temporary benefit, the symptoms returning in from one to three months; whilst it has been expected that the profession would accept and become devotees to the operation of iridectomy from a number of operations, the results of which are calculated at the comparatively short interval of from two to three weeks from the time of operation.

I have always felt that Mr. Hildige's objections possessed great weight, and my attention has consequently been carefully directed to this point, and the result has been to strengthen me very considerably in my original conviction, that my operation did not relieve by evacuating fluid, but by removing constriction. I scarcely know of a single instance in which the patient was benefitted immediately, or even shortly after the operation, experiencing the disappointment of a relapse; whilst, on the contrary, in very many instances, the relief has been so gradual that, for some three or four weeks, it has been scarcely appreciable.

The class of cases in which improvement is most rapidly manifested, is that of acute and subacute glaucoma, and what has been termed "evanescent obscurations." In chronic cases, and those of total blindness, or where a mere perception of light has remained to the patient, the improvement has been of much more tardy manifestation, rarely taking place under a fortnight from the operation, after which it usually increases, and rarely, if ever, recedes.

The great value of this operation seems to me that by an almost painless proceeding, however severe the previous suffering has been, the pain is almost invariably removed. Not only is this the case, but, if fairly tried (which I am justified in asserting it has not been by those who appear to be so anxious to disparage it), it will be found that whilst capable of effecting an equal amount of good as "iridectomy," it is free from the danger and objections attending this operation. For instance,—

*a.* It is much more easily and rapidly performed, and the incision made into the eyeball is much smaller.

*b.* The operation is attended with so little pain that there is no necessity for chloroform; the sickness and accidents resulting therefrom are consequently avoided.

*c.* As the eye does not require to be fixed by forceps, the danger of lacerating the "rotten" conjunctiva, or of rupturing the suspensory ligament of the lens, is likewise avoided.

*d.* The incision is made at once by a Beer's knife, and does not in any case require to be enlarged by scissors.

*e.* As the incision commences at the margin of the cornea, and is carried obliquely into the sclerotica away from the cornea (not backwards and inwards towardsthe axis of the globe, as erroneously stated by Mr. Hulke), and as the iris is left intact, and is not to be seized by forceps nor drawn out by a hook, there is no danger of the lens escaping, being broken up, or so injured as to cause traumatic cataract.

*f.* The integrity of the iris being preserved, the opearation is not necessarily or even beneficially restricted to the upper segment of the cornea in order to obtain the compensatory support of the upper lid. The patient, moreover, retains the power of adapting his vision to near objects; and in the majority of cases there is no permanent disfigurement of the eye, or coloboma iridis.

*g.* The iris not being injured, hemorrhage from its lacerated edges cannot occur; thus it can neither be necessary to employ a scoop to remove the blood, nor to make a counter-opening to admit of its escape.

*h.* The incision is so small that it is impossible for the lens or the vitreous humour in any quantity to escape through it. The necessity of extirpating the eyeball, therefore, from either of these causes, or on account of hemorrhage, as I have already shown, cannot by any possibility occur.

*i.* It relieves pain by the removal of the constriction of the eyeball, and the consequent pressure upon the nerves from the undue constriction of the ciliary muscle.

*j.* By it the impediment to circulation through the blood-vessels is got rid of; they are placed in a favourable condition to recover their normal state, and the probability of a recurrence of the effusion is greatly diminished.

*k.* Where it fails, it does so little damage to the eye, that iridectomy may subsequently be performed, if considered beneficial to the patient.—*Lancet, Oct.* 13, 1860, *p.* 355.

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87.—*On Division of the Ciliary Muscle, in Acute Glaucoma.*—Of the value of Mr. Hancock's important and successful operation of division of the ciliary muscle, we (Ed. of Lancet), have now abundant evidence; and to-day we record an illustration (selected from amongst a number in the Westminster Ophthalmic Hospital) which we had the opportunity of personally examining, and of witnessing the good effects produced, in a female nearly sixty years of age, effected with acute glaucoma. The patient was blind on admission, but in three weeks she was able to read large-typed words in the Bible. It was a case well verifying Mr. Hancock's views.

A surgeon recently writing from Paris, assured Mr. Hancock that he had the satisfaction of receiving M. Desmarres' testimony regarding the results of this operation in a case of glaucoma in which he had tried it. It was described as a very bad case, and the effects of the operation were all that could be desired, quite as complete as after the performance of iridectomy. It was the intention of M. Desmarres to continue the practice, more especially in his bad cases, one-half of which he should treat by paracentesis, the other half by Mr. Hancock's method. In our "Parisian Medical Intelligence" for May 19th, the successful result of the latter procedure in a case of acute glaucoma of extreme severity, in the hands of M. Desmarres, is thus announced: "Within twelve hours of the operation all the symptoms had improved, and at the present moment (it is now five weeks since the event) the patient is fully as much benefited as if the most successful iridectomy had been performed." It will be seen, therefore, from the results already obtained in London and elsewhere, that the success of the operation is established, and Mr. Hancock has thus rendered a most important service to ophthalmic science.

*For the notes* of the following case we are indebted to Dr. Sansom, the *medical registrar* to the hospital:—

Harriet B——, aged fifty-eight, was admitted into the hospital on the 21st of March. She complained of intense pain in the right globe, and throbbing pain over the brow; the sight was wholly gone; the conjunctiva was greatly congested; the sclerotic presented a purplish glow; the pupil was dilated and irregular, and the lens opaque and disorganised, presenting a greenish appearance. The light used for opthalmoscopic examination could not penetrate the disorganized lens. The eyeball was exceedingly tense, but the slightest pressure caused most severe pain. The patient was thin and pale, and presented the appearance of having undergone much suffering. Her previous history was as follows:—She had never had any disease of the eye until the present attack. Three months ago the eye watered as if she had caught cold in it, but she paid little attention to this. Occasionally, towards evening, she experienced the sensation of flashes of fire before the eye, and the flame of the candle seemed surrounded by a halo of colour, but all unpleasant symptoms disappeared towards morning. On the 14th of March (a week before admission) it rapidly became worse, great pain occurred over the brow, and a tense throbbing pain referred to the back of the orbit. The eye, she said, seemed so tight that it would burst. The symptoms continued to increase.

On March 23rd, Mr. Hancock divided the ciliary ligament, and a quantity of fluid and disorganized vitreous humour escaped. The eye was kept from the light.

March 26th. The pain has entirely left her; she is able to tell the number of fingers held up before the eye which has been operated on, the other being closed; the iris has been slightly drawn into the wound.

30th. Pain continues entirely absent. She can now distinguish the outline of any object, and she says that the sight is improving every day.—*Lancet, July 7, 1860, p. 7.*

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## 88.—CASES OF GLAUCOMA TREATED BY DIVISION OF THE CILIARY MUSCLE.

By James Rouse, Esq., Surgeon to the Royal Westminster Ophthalmic Hospital.

As it is possible that some of our readers may not know the exact steps of Mr. Hancock's operation for the relief of glaucoma, the following extract from that gentleman's lecture, published in the 'Lancet' of Feb. 11th, may be deemed acceptable:—"I introduce a Beer's cataract knife at the outer and lower margin of the cornea, where it joins the sclerotica; the point of the knife is pushed obliquely backwards and downwards, until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch: by this incision, the ciliary muscle is divided, whilst the accumulated fluid flows by the side of the knife."

If the reader will refer to the lecture from which the above extract is taken (see Retrospect, Vol. xli. p. 197), he will find that Mr. Hancock was induced to perform the operation just described from having noticed in several cases of acute glaucoma a peculiar constriction around the cornea corresponding to the situation occupied by the ciliary muscle; the operation was followed by most satisfactory results.

The following cases are the more interesting, inasmuch as they did not present that peculiarity; and it will be seen that the operation was equally successful.

*Case* 1.—H. W., aged 46, married, the mother of children, was admitted under my care on May 4th. During the last eleven years, she had suffered from occasional attacks of pain (accompanied by dimness of vision) in the left eye. Two months ago she was attacked with pain in the left eye, which prevented her from following her usual occupations. A week ago the pain became so intense that she could not procure sleep. During this time her sight was very limited; she could not see more than two yards before her, and then the objects looked *confused*; beyond that distance, everything appeared to *be dense fog. She* complained of intense throbbing pain in the

eyeball of the left eye, circumorbital pain, and occasional flashes of light. The pain was more severe at night, and at times appeared to affect the whole head. She could not distinguish any object. On examining the eye, the cornea was found to be slightly ulcerated. The anterior chamber was very much flattened, partly from pushing forwards of the iris, and partly from the stretching of the cornea from the intra-ocular effusion. The pupil was large and immovable. The conjunctival and choroidal veins were large. Pressure on the eyeball caused extreme pain. She had had no sleep for a week.

May 4th, 2 p.m. The ciliary muscle was divided; a considerable amount of fluid escaped. Within an hour after the operation, the pain was relieved, and she passed a most excellent night.

May 18th. She had had no pain since the operation; and, on a watch being given to her, she could tell the time; and could distinguish silver from gold. The eye had nearly regained its natural appearance. The anterior chamber was of the same depth as that of the right eye. The ulceration of the cornea had healed, leaving only a slight cloud.

May 28th. She had had no return of pain, and saw perfectly. The cornea was rapidly clearing. She was made an out-patient.

June 15th. She came to show herself, stating that she was perfectly well.

*Case* 2.—A. H., aged 47, married, the mother of seven children, looking at least ten years more than her age, was admitted on May 18th under my care. Four years ago, her sight began to get weak, not being able to thread a needle without glasses. One night in the middle of last September, she was attacked with violent pain in both eyes and around the orbits, especially the left. The following day, the pain increased in intensity, and compelled her to keep her bed. The sight of the left eye became seriously affected, vision being very limited and indistinct. The right eye was affected, but in a less degree.

In December, she came to the hospital as an out-patient; at that time the sight of the left eye was completely lost; the pupil was large and somewhat irregular; the vitreous body was too opaque to allow ophthalmoscope examination. The pain was so intense in that eye and side of the head, that she could not sleep. Under these circumstances, the ciliary muscle was divided. The operation completely relieved the pain; but there was no return of vision. The right eye improved very much after the operation; in fact, she was so much better that she ceased to attend.

May 16th. She again applied for advice; the right eye had become so much worse, that she could no longer see to read. She could distinguish large objects, but not sufficiently to recognize a face. The pupil was large and fixed, the cornea was flattened, and the iris pushed against it. The vitreous body was semi-opaque. She complained of great pain in the right temple and side of the head. She felt so giddy, that she did not like to move about.

May 18th. She was admitted into the hospital; and the ciliary muscle was divided.

May 30th. She had progressed favourably since the operation, the mist had entirely gone, and she was able to read for a short time without inconvenience, but it caused subsequent uneasiness. She stated that she could not look very long at an object without the eye becoming slightly "dazzled." She was made an out-patient.

June 15th. The sight was improving—every day becoming stronger. She could now look at an object for a much longer time without inconvenience. She saw everything perfectly.

Aug. 13th. The sight perfectly good in the right eye. She finds that if she takes cold there is slight mistiness for a day or two. During the last month there is slight return of sight in the left eye. She can now perceive the window-frames.

*Case 3.*—L. N., aged 30, single, a dressmaker, was admitted under my care on June 8th. Two years since the sight began to get dim and misty;

she had pain from time to time in the temples across the forehead, after each attack the sight was less good. A year ago, she became decidedly worse. The pain in the eyes and temples was more severe, and the sight was so much affected that she could no longer see anything distinctly. She could only read large print, and see to do the coarsest work, and this very indifferently. She had not menstruated for two years.

On admission, the pupils were large and fixed; the refractive media were turbid. Ophthalmoscopic examination showed dilated veins on both retinæ. The optic discs could only be seen imperfectly; they appeared slightly cupped. The eyes were very hard, and the anterior chambers and corneæ were flattened. She complained of iridescent vision, and pain in the eyes and head. She was given a watch, but she could not positively tell the face from the back. She thought she could distinguish very large type, but the attempt to read caused pain and dizziness in the head. The ciliary muscle was divided in both eyes.

June 18th. She had had no pain since the operation. The irides had assumed the natural size, and acted perfectly. This morning she threaded a needle, and read some ordinary sized print. She said she had not seen so well since the first attack two years ago.

August 13th. The sight continues satisfactory.—*British Medical Journal, Aug.* 18, 1860, *p.* 645.

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## 89.—ON GLAUCOMA AND THE OPTIC PAPILLA.

By Dr. Mackenzie, Glasgow.

I have long since established to my own satisfaction the three following conclusions regarding glaucoma:—

1st. That the sea-green appearance behind the pupil, whence the name arose, does not depend, as was once generally supposed, on any thickened or discoloured condition of the vitreous humour, but on a diplochromatic state of the crystalline, by which it absorbs the extreme prismatic rays, and reflects the middle ones.

2nd. That the vitreous fluid in glaucoma is superseded by an unhealthy secretion, which over-distends the eye, makes it feel hard, causes severe pain by pressing on the ciliary nerves, and obliterate the sensibility of the retina; a state of matters which I found to be susceptible of relief by paracentesis of the eye, through either the sclerotica or the cornea.

3rd. That in advanced cases, the optic nerve behind the eye shows itself, on dissection, in a state of atrophy, deprived more or less of its proper nervous substance, and flattened.

Ophthalmoscopic investigations have added some new facts to our knowledge of glaucoma. One of these is the pulsation of the arteries of the retina; and another, the concave or excavated state of the papilla of the optic nerve. Neither of these phenomena, however, is peculiar to glaucoma; both of them being occasionally met with in other diseases of the eye. The former of them seems to indicate an increased resistance to the flow of blood in the vessels which supply the retina; the latter may be regarded as the effect of one or other, or of both, of two different causes, namely, the atrophied condition of the optic nerve, and the increased pressure on the internal parietes of the eyeball, from the superabundant secretion which occupies the place of the vitreous humour.

These phenomena can scarcely be discerned in the advanced stages of glaucoma, owing to the diplochromatic and muddy state of the crystalline. To see them, then, the student should be directed to examine a case, in which the dioptric media have as yet lost but little of their normal colour and transparency.

To discern the pulsatory movement of the vessels, requires a sharp and experienced eye, and the observer will find it of service, while directing his attention to this point, to have the patient's head supported, and the diseased eye steadied by the fingers of an assistant.

If the indirect or inverted method of ophthalmoscopic observation be selected. an optical deception is apt to bewilder a beginner, as to the condition of the entrance of the optic nerve, a nearly circular spot, though not unfrequently oval, variable in size, but measuring on an average 0·6 line in diameter, and which, although styled *papilla* or *colliculus*, is, in its normal state, nearly level with the retina, and even a little depressed in its centre. In the direct method, where the observer regards the illuminated non-inverted fundus oculi through an aperture in a concave mirror without the aid of any extraneous lens, the great magnifying power of the cornea and humours of the patient's eye, gives to the papilla an apparent magnitude larger even than that of the pupil; but in this way, it cannot be well seen as a whole, and generally requires the eye of the observer to be brought inconveniently near to that of the patient. Instead, then, of using the patient's eye as a powerful single microscope, in actual contact with the objects on the fundus oculi to be examined, and thus viewing them directly, it is better to obtain a smaller but more defined image, although an inverted and virtual one of those objects, by converting the patient's eye for the time into the object-glass of a compound microscope, which we do by holding in front of it a thick convex lens. The image which we then see of the several parts of the fundus oculi is an inverted one, like that of an object examined with any ordinary compound microscope; the entrance of the optic nerve is seen towards the temple instead of the nose; the macula lutea appears to the nasal side of the optic nerve instead of the temporal, and a little below the level of the nerve instead of above it; while the principal trunks of the retinal vessels, instead of branching in the direction of the temple to embrace the macula lutea, seem to bend towards the nasal side of the eye. The apparent position, in fact, of all the objects on the fundus, viewed in this way, is the reverse of their real position.

The most important optical deception which arises from viewing the fundus in the indirect method, as well as the most puzzling to a beginner, affects the papilla. The student has probably heard, that the papilla, in the glaucomatous eye, is cupped or excavated, but to his view it appears quite the reverse, it appears rounded and prominent.

To comprehend clearly that this is an illusion, all that one requires to do, is to impress with the head of a pin, a small dimple on a bit of paper, and put this under a compound microscope, with the concave side of the impression uppermost. The same appearance will then be seen, which is presented by the papilla of the glaucomatous eye, viz., that of a rounded and protuberant surface.

This optical deception arises from the inversion which the image suffers by being viewed through the compound microscope. We judge that an object, viewed with a single eye, is convex or concave, solely by the manner in which light is reflected from the body under examination. The light which falls obliquely on a convex surface illuminates that side which is nearer to the source of light; the side farther from it is in shade. The light which falls obliquely on a concave surface illuminates that side which is farther from the source of light; the side nearer it is in shade. Let the source of light remain in the same position, but invert the image of the object illuminated, so that the light which falls on the farther side of it may seem to fall on the near side, which is the case when we look through the compound microscope at the hollow on a bit of paper, or when we examine the optic papilla through the compound microscope formed for the occasion by the patient's eye *plus* the convex lens held in front of it; and both the dimple on the paper and the papilla, although they are actually cupped or concave, will then appear convex and prominent.

If, on the other hand, we turn the convex side of the dimple on the paper uppermost, and view it with the compound microscope, it appears concave. The inversion of its image causes the light which falls on its near side, to appear as if it fell on its farther side, and thus the eye is subjected to a deception the reverse of the former, and from which it cannot free itself. If there be cases, then, in which the end of the optic nerve within the eye ac-

tually projects in a convex form, they will offer, when examined in the indirect method, the appearance of a cup or depression.

Such facts have long been familiarly known; the apparent transmutation of an intaglio into a cameo, or that of a cameo into an intaglio, under the compound microscope, being a common source of amusement, fully discussed by Sir David Brewster in his 'Letters on Natural Magic, Letter V.' Important as their bearing is on pathological examinations of the eye, they seem to have escaped the notice of ophthalmoscopists, till attention was directed to the subject by Dr. A. Weber, in a paper in the 'Archiv für Ophthalmologie,' Band II, Abtheilung I, Seite 141.

In a highly interesting communication in the same journal (Band IV, Abtheilung II, Seite 1), Dr. H. Müller has directed attention to the normal, as well as to several diseased states, of the optic papilla.

His account of the entrance of the optic nerve into the eye. and the diagram which he gives in illustration, fully remarkably with the thirteenth figure in Mr. Bowman's "Lectures," and the corresponding explanation. From the statement and figures of these two observers, especially those of Dr. Müller, it may be gathered that the lamina cribrosa is normally somewhat concave towards the interior of the eye; that the fibres of the optic nerve, suddenly losing their white substance and dark outline, enter the eye on a level with the chorio-capillaris; that at this point the whole nerve, from the change which its fibres have just undergone, is considerably and rather suddenly reduced in thickness; that the fibres, bending more or less abruptly outwards, and spreading around, become clothed by the exterior or radially disposed layers of the retina; that the edge of the opening through which the nerve passes into the eye, as well as the fibres themselves as they traverse that edge, form a slight elevation or approach to a papilla, leaving in the situation where the trunks of the central vessels of the retina generally make their appearance, a small foveola.

Dr. Müller points out the difficulties which attend the anatomical examination of this depression, arising from the softness of the part, and its liability to change on being touched; and shows how these difficulties may best be obviated. He directs attention to the varieties which exist in different individuals; varieties in the depth of the depression, from 0·2 to 0.5 millimetre; varieties in its form and position, for it is not always symmetrical, and does not always correspond to the middle of the nerve, but is sometimes considerably nearer to the macula lutea, or has one portion of its edge more raised than the rest: varieties in the dispositions of the vessels, for while the large trunks generally emerge from the middle of the foveola, one or more sometimes seek a passage for themselves close to the edge of the chorio-capillaris, so as to make their appearance by the side of the nerve; facts, all of which should be carefully borne in mind by ophthalmoscopists.

In regard to abnormal prominence of the papilla, Dr. Müller shows, that this is likely to arise from an atrophied state of the exterior layers of the retina, a thickened condition of the primitive nervous fibres, as well as infiltration of the nerve by blood, inflammatory exudation, or new formations. He relates a case in which a concretion in the site of the lamina cribrosa caused a protuberance of the papilla. The very earliest stage of encephaloid tumour should show an abnormal prominence of the papilla.

On the other hand, intra-ocular pressure, and atrophy of the nerve, existing either singly or in combination, are the causes of abnormal excavation of the papilla. In case of intra-ocular pressure, either simple, or combined with atrophy of the nerve, paracentesis, either corneal or. sclerotical, is likely to produce a change in the depth of the excavation, sufficient to be recognized on ophthalmoscopical examination.

Morbid excavation of the papilla varies in depth, reaching in extreme cases to a millimetre beyond the level of the choroid. In such cases, the sides of the excavation are nearly perpendicular, or are even concave, and its edge, of course, independent, so as partially, or even completely, to hide from view the course of the *vessels* as they pass from the bottom of the excavation *and over its edge to the retina*. In slight cases, the sides of the ex-

cavation are convex, and its form that of a funnel. The lamina cribrosa, under such circumstances, keeps its place, but in more advanced stages, it is pushed back, and the excavation is much extended laterally. Such extreme cases of expansion are not the result of uncombined atrophy of the nerve, but must arise from the supporting resistance of the parts being weakened by previous inflammation, whence they come to yield more readily to intra-ocular pressure.—*Ophthalmic Hos. Reports. No.* 11, *p.* 252.

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## 90.—ON THE OPHTHALMOSCOPE AND ITS APPLICATION.

By Haynes Walton, Esq., F.R.C.S., Surgeon to St. Mary's, and to the London Ophthalmic Hospitals.

[Mr. Walton believes that the origin of the ophthalmoscope is to be traced to a communication.]

In the twenty-ninth volume of the 'Medico-Chirurgical Transactions,' by William Cumming, Esq., formerly house-surgeon to the London Hospital, "On a Luminous appearance of the Human Eye and its application to the Detection of Disease in the Retina and Posterior Part of the Eye."

[It would appear that Mr. Cumming's attention was attracted to the subject from the description of cat's-eye amaurosis by several authors; i.e. amaurosis in which there is a luminous glare like that occasionally seen in the eyes of some animals, this Mr. Cumming shows to be natural to the healthy human eye, when the pupil is widely dilated and the eye seen in a certain light.]

His simple process of examination is this:—"Let the person under examination sit or stand eight or ten feet from a gas light, looking a little to the side; standing near the gas-light, we have only to approach as near as possible to the direct line between it and the eye to be viewed, at once to see reflection. Or, in a dark room, a candle being placed four or five feet from the eye, if we approach the direct line between them, we shall be able at once to see it in many cases. If solar light be admitted through a newly closed shutter into a dark room, the luminosity may be seen when the pupil is tolerably dilated, the patient standing five or six feet from the aperture, and the observer occupying the position before indicated."

"These, then, are the circumstances necessary for seeing the luminosity. That the eye must be at some distance from the source of light, the distance being greater in proportion to the intensity; that the rays of light diffused around the patient (and sometimes around the eye itself) should be excluded; that the observer should occupy a position as near as possible to the direct line between the source of light and the eye examined; hence it is sometimes necessary for the observer to stand obliquely, that his eye may approach nearer to the direct line."

"The appearance of the reflection itself not only varies much in colour and intensity in different persons, but also from the circumstances under which it is seen; viz., the greater or less intensity of light, the position of the eye examined, and the distance at which it is viewed." Many other similar modes are given.

Three times I have seen the eye accidentally illuminated. The first time that I was called to a consultation in the country (an occurrence which a man is not likely to forget), I saw a beautiful illumination of the eyes of a gentleman who was sitting in the corner of the waiting-room of the old station of the Great Western Railway, a few feet behind a gas-lamp; and associated with it was this coincidence, that the gentleman, himself blind, who came to town with his servant to take me to one of his pupils, who had been wounded in his eye, was at the time telling me of his own blindness, and called it "cat's-eye amaurosis." After that, I witnessed the same thing *in a lad,* behind the gas-light, in a druggist's shop. I again saw it in my *study by my* microscope lamp.

*Mr. Cumming's* paper, with all its experiments and suggestions, and not

less its originality and philosophy, bases all the application of the discovery in the mere illumination of the interior of the eye-ball; full illumination being taken as the indication of health, and the absence of it as indication of disease in the retina and posterior part of the eye; for instance, he writes:—

"The establishment of the fact of a similar reflection from the healthy human eye to that from the eyes of animals, appears to me chiefly important in its adaptation as a mode of examining the posterior part of the eye. The retina and choroid, hitherto concealed in the living eye, and little opportunity being afforded of examining their condition after life, in consequence of their diseases not terminating fatally, considerable uncertainty has attended the diseases ascribed to these structures; but the existence of this luminosity having been recognized, its non-existence or abnormal appearance may enable us to detect changes in these structures heretofore unknown, or satisfactorily to see those which we only suspected."

If we dilate the pupil by atropine we have a means afforded of seeing the condition of the retina and choroid in every case. The cases I have examined in this way have confirmed the general impression that the retina is not frequently the seat of change in amaurosis; for, out of several cases of amaurosis, in which the non-opacity of the cornea, lens, and humours, allowed this mode of examination, I found but two, in which the retina was so changed that the reflection was not seen.

Among the physiological conditions of the eye enunciated by Mr. Cumming, are these: that the choroid is the principal reflecting structure, the light returned from the retina and concavity of the hyaloid body doubtlessly increasing the effect; that the brilliancy is considerably augmented from the concentrating influence of the concave shape of the retina, and the focal distance of the lens; that the luminosity of the healthy eye appears to be in proportion to the light colour of the pigment. The reflection from the albino is evident in ordinary day light. He further observes:—

"On placing a middle aged man, an albino ten or twelve feet from a gas-light, the reflection was extremely vivid, and of a pinker colour than ordinary, while the light transmitted through the choroid and iris evidently increased the effect. On placing close to his eyes a black card with an aperture a little larger than the pupil, the reflection was brighter than that from the eye of a fair person examined side by side, but was of a more decided pink colour.

"In persons of fair complexion and blue or grey irides, it is generally more brilliant and more readily seen than in those of dark skin and irides. In the mulatto, it is also dusky; but in them, as in persons of swarthy complexion, a silvery reflection is sometimes seen, and is most probably a reflection from the retina. In the albino, this reflection produced by the vascular choroid is most brilliant and lightest in tint. In proportion to the darkness of the pigment, its lustre is diminished and the colour becomes more dusky."

Perhaps there appears a contradiction here, but there is really no discrepancy. The choroid is found to reflect greatest where the pigment is wanting. The pigment has nothing to do with the reflection; on the contrary, it interferes with it, and therefore lessens the illuminating power. In man, the pigment is less abundant and less intense in color than in many of the lower animals; it is brown, rather than black.

It was then the first grand principle, the comprehensive idea, that Mr. Cumming gave us. He did not see the optic disc, nor the retinal vessels. The perfecting of the discovery and the practical application, involving as they did the difficulty of inventing an instrument and the working out of much detail, were reserved for other labourers. These were found in Germany. It would be fruitless to trace the instrument through its stages of development. I will merely give the names of its several improvers, as far as I can ascertain, in the order of time: they are, Helmholtz, Ruete, Coccius, and Anagnostakis.

The *ophthalmoscope at present in* use is a portable little concave mirror,

made of silvered glass, having a hole of sufficient dimensions in the centre, with a handle of about ten inches. The hole is usually too small; it should be of sufficient size to admit of distinct vision when the instrument is held with the requisite obliquity; the edges should be smooth and free from splinters. A double convex lens, of from two and a half to three inches focus, is needed as an appendage; but the power should be regulated by the usual range of the observer. It should be set in a frame with a stem of three or four inches long, which renders it more readily used. I know that mirrors of different curve and lenses of varying power compose the apparatus of some surgeons. That these varieties can be brought into requisition according to one's fancy, is undoubted; but I believe that they possess no utility, no decided practical advantage.

The ophthalmoscope demands an artificial light. Under an emergency, I have employed a candle, and with the faint illumination have gathered some although an imperfect, idea of the interior of the eye. A steady lamp-flame is needed for the full effect. Whenever I can command it, I employ gas, and use a small argand burner, at the bottom of which is an arrangement with glass collar that ensures a steady flame, that slides on a vertical rod with a pedestal. It is a gas-lamp much used by the artizans of the watch-making district of Clerkenwell, and sold at a lamp shop in Clerkenwell Green. A very light blue chimney whitens the light by reducing the predominant red rays, and the eye is then more naturally seen.

The examination should be made in a dark room.

As a rule, the eye should be prepared by having the pupil dilated by belladonna or some of its salts. Now and then persons are met with, especially fair young persons, whose naturally expanded pupils enable us to dispense with the artificial enlargement, particularly if it be not more than a mere glance within the eye that is needed, and above all, near-sighted eyes may be so examined; but generally, in adult age, and still more in old age with its discoloured lens, and in all cases where a thorough survey is called for, the natural pupil will not suffice. Vain discussions have been wasted on this question, which may be resolved into this common-sense remark, that if the desired object can be accomplished without interfering with the pupil, all the better, as the sequel will show; if it cannot, dilatation must be resorted to.

The pupil is generally more contracted by the light of day, than by the light of the ophthalmoscope. Often when it has appeared too small to allow of any serviceable examination, it has dilated sufficiently when the room has been darkened, and remained of such capacity as to afford all that was desired.

Unnecessary dilatation should always be avoided, because this unnatural state produces positive discomfort from indistinct vision, the degree and duration of which, from days to weeks, is proportionate to the dilatation and the healthiness of the eye. Patients frequently leave the surgeon on account of this, even when they have been forewarned. In spite of all assurance to the contrary, they are likely to attribute the temporary annoyance to the use of the ophthalmoscope, and declare how detrimental it has been to them. Any increase of disease is often set down to the same cause.

It is not generally known, indeed it is my own observation, that, when a strong solution of atropine is used to the one eye, the vision of the other is apt to be impaired for one, two, three, or even more days, and that without any apparent alteration in the pupil. The power of adjustment seems to suffer particularly. I shall not stop to discuss the physiology of this; I think it sufficient to mention the fact.

These are reasons why strong solutions should be avoided; and weak ones answer our purpose quite well. I have seen symptoms from the strong applications that leave me in doubt that they were those of belladonna poisoning. In one instance I believe that death was so caused. The physician *who was called to* the young mentioned his suspicions of such, without know*ing that the drug* had been used. A six-ounce bottle of lotion, containing *thirty-six grains* of the sulphate of atropine, was ordered as an eye-lotion,

with directions for the wash to be constantly applied to the eye. These were fully carried out. The eye was kept saturated with wetted lint; several bottles were so used. What a large quantity must have passed into the throat through the lacrymal conduits!

Under very exceptional circumstances only therefore, and which the sagacity of the surgeon must determine, should a strong solution be used, and then only in small quantities. Half a grain of the sulphate to an ounce of water is what I ordinarily employ. A single drop, put on the conjunctiva, suffices. I depress the lower eyelid, and with a brush, my finger, or any convenient thing, apply the drop. Dilatation is much slower, but it passes away quickly, and the disagreeables incidental to the stronger form do not appear. Dr. Garrod has shown by his experiments that dilatation may be obtained by a grain in several ounces of water, but it must be remembered that healthy eyes are easier influenced than diseased ones.

It has been asserted that the retina is likely to become congested by the atropine. I have not been able to confirm this statement; and it is difficult to reconcile such a statement with the observation of my colleague, Mr. Taylor, that the eye is more tolerant to the ophthalmoscopic examination when atropine is used, than otherwise.

[In a subsequent paper, Mr. Walton proceeds to diseased states of the eye, first noticing cataract.]

*Cataract.*—Few surgeons have long been engaged in active duty without being aware of the difficulty attendant on the diagnosis of incipient cataract. Nor is it the general surgeon only who may be so puzzled. From my own personal knowledge, such men as Tyrrell and Dalrymple were deceived, as the progress of cases showed. It would be easy for me to adduce parallel examples occurring among living practitioners. Indeed, till the ophthalmoscope appeared, we were necessarily often in difficulty; and no one felt this more than myself, as the means at our disposal—the catoptric test and the concentrated light—were not enough. It was especially so in old age, when the natural coloration of the lens, the amber hue existed, and particularly when it was unusually developed, for then streaks of opacity at the back of the crystalline lens, or general haziness of this body, were easily overlooked. The coloration itself was liable to be mistaken for disease, when there co-existed impaired vision from any cause.

A wine-merchant was assured by a gentleman engaged in ophthalmic practice in this metropolis, that he had cataract. An operation was proposed, and consented to. The chloroformist was even named. I was consulted, in order to satisfy some friends. I found the lenses very highly coloured, but not opaque, for I could see distinctly all the fundus of the eye. Much hepatic derangement with a very debilitated system seemed to be the cause of defective sight, and a few weeks' treatment confirmed the diagnosis I had given.

Whenever, therefore, there is a suspicion of cataract, an ophthalmic investigation should be instituted. A less intense light than ordinarily employed, is preferable, because it is possible here to over-illuminate the eye, and the striæ may not be seen. With very high illumination, opacities may be less apparent than without the ophthalmoscope. It is on this account, too, that the eye should be examined at first without the lens. A source of fallacy must here be guarded against. The dots of pigment that are so often seen on the capsule of the lens as the result of inflammatory attacks, by which the iris and the capsule have been brought into apposition, as also cicatrices or spots on the cornea, must not be mistaken for cataract opacities. It is enough to direct attention to this point to prevent that mistake. Positively, cataract may now be detected before a person is himself aware of it.

A lady applied to me for defective sight in the left eye. I had little doubt that I saw radiating striæ in her lens; but to be quite sure, I used the ophthalmoscope, and had the fullest evidence of cataract. I looked at the other eye, and discovered a few small and irregular linear opacities, which I had not seen in *my naked eye examination*. I was told, in answer to my ques-

tions, that that eye was quite well, and vision was not impaired; yet I found by some delicate visual tests, that there was not perfect sight, but the derangement was so trifling, that it had not been noticed. I thought this a great triumph of modern science.

It is of no small diagnostic value to have a correct idea of the condition of the interior of the eye in cataract, to ascertain whether the loss of sight is in proportion to the degree of the opacity, and, in fact, depends on it, or on a more extended diseased state. We can get this knowledge by no other means. Do not considerations like these warrant an often repeated assertion of mine, that for what we have gained in this respect alone, the ophthalmoscope is of immense value; and if we went no further, might be truthfully considered amongst the greatest additions to recent ophthalmology?

Another condition of the crystalline lens can be learned only in the recent state by illumination: I mean, *Partial Dislocation of this body backwards, or twisting out of its place by direct violence.* This may be palpable later when the opacity has pervaded the lens, but certainly not by any ordinary means till then.

*Disease of the Vitreous Humour* seems to cause more astonishment to an inexperienced observer than any other morbid changes; partly, I suppose, because any physical alteration apart from greater fluidity was never supposed, and because the moving about of shreds and granules, and flakes and threads, are such palpable and material things. We now know that this body, which certainly is of low organisation, since it is often very rudely treated in certain operations, and portions of it are lost with apparent impunity, does become materially altered, partaking of disease that invades the eyeball generally. Apparently, at times, it is the sole seat of morbid phenomena, which are great impediments to vision. This is quite new to us; and no speculation, guessing, conjecture, or indication, brought it within the range of probability. In inflammations of the eyeball in general, it mostly becomes opaque, and looks muddy, and more or less hides, as in a fog, the fundus of the eye. Careful examination generally discovers minute bodies in constant motion. I have seen opacity of it, in the greatest degree, without any other symptom that I could detect, or any cause; and some cases that seemed quite hopeless, from the degree of turbidity, and the corresponding loss of sight, and the absence of all indication for treatment, have got quite well. So frequently have I seen the cloudiness pass away entirely in acute specific inflammations of syphilis and rheumatism, even where there were floating patches and filaments, which we call exudations, that I have little prognostic dread of it, as compared with cloudiness unaccompanied by acute disease.

*Floating objects* are not uncommon in the vitreous humour while its transparency remains; and they may exist alone or in connection with diseases of the retina or the choroid coats. These are generally seen by the individuals themselves, and are often the circumstances that induce them to apply for relief. This brings me to speak of muscæ volitantes, in connection with the internal ocular inspection, and that only. I think that any one who has studied the writings of Sir D. Brewster, and the masterly essay of Dr. Mackenzie, himself a sufferer from muscæ, or investigated the subject with pains and care, cannot fail to conclude that they are minute floating bodies in the eye that cast their shadows on the retina, and that they are far back in the posterior part of the eye, almost in contact with the retina. As a consequence, this upsets the theory of pressure on the retina by varicose vessels causing them; but it does not invalidate the supposition that certain parts of the retina may be so pressed on, and rendered insensible, causing the loss of function to be manifested in fixed spots, or black disc that have not the motion, and other characters of muscæ.

I have not, so far as I remember, been able to detect moving bodies in the vitreous humour; but, in connection with other internal disease of the eyeball or defect of sight, I suspect that ordinary muscæ are too minute to be seen with the ophthalmoscope. It is becoming a prevalent idea that they

may always be found if looked for. It is a fact which cannot be accounted for, that floating bodies in the vitreous humour are not always seen by a person, and that the perception of them varies.

*Effusion of Blood in the Vitreous Humour* is another newly recognized state. It is most threatening when extensive, because vision is quite obscured, and the eye to all appearance lost. It is nearly always impossible to trace the source; but, I presume, it must generally be from retinal vessels. It may be traumatic or idiopathic. The first is the rarer, and the amount of hemorrhage bears no direct relation to the severity of the accident; a mere tap on the eyeball may produce as much internal bleeding as a smart stroke; and I shall presently show that a blow about the region of the eye only, may cause intra-ocular hemorrhage. The second, the idiopathic variety, may, like the first, be unaccompanied by any external sign whatever; indeed, only now and then is there associated redness of the surface of the eyeball. The subjective symptoms, by no means always present, are flashes, corruscations, combinations of colour, and spectra. The actual condition, therefore, the diagnosis, cannot be known, except by the ophthalmoscope. In partial effusion the clot may be seen in the always more or less hazy vitreous humour. But the whole of the interior of the eye may be so darkened that no illumination can be obtained. I do not know how much blood is needed for this. Among the idiopathic cases the worst have occurred in eyeballs long subject to chronic inflammation, and the hemorrhage has quite destroyed the sight. Two of my patients lost their eyes in this way in the night. I have known recovery ensue many times when there has been so much effused, that I could not illuminate the eye; and I do not give a decidedly unfavourable prognosis, except there pre-existed disease of the eyeball.

In the first case that I met with, I suspected the eye to be lost, but complete recovery ensued. A gentleman in good health, and with, as he thought, perfect eyes, perceived large soot-like bodies floating before the left eye. There were occasionally coloured clouds. He at once came to me. In a cloudy vitreous humour I saw the extravasation. There was no other objective symptom. I saw him two days after, and the vitreous humour was quite obscured. At the end of five weeks he could read any type, but was a little interrupted by some floating shreds in the vitreous humour temporarily obscuring the type. A few weeks later his sight was quite restored, and I could not detect any abnormal ophthalmoscopic appearance whatever.

I will give the particulars of another case that occurred in the practice of my colleague, Mr. Taylor, and closely watched by him. Through his kindness, I saw the patient twice.

Mr. C. an artist, aged 30, applied to me for advice last February. A few mornings previously, on commencing work, he found that the sight of the right eye was obscured, to use his own words, "by various forms of dense matter, like those produced by blood dropped in water, or wreaths of blood-coloured smoke, the denser parts of which, when looked at against a strong light, appeared of a brilliant brown red." About two-thirds of the field of vision were totally obscured, the lower third being of a pale green colour, through which he could discern objects "with tolerable distinctness." During the following two days, the obscuration extended over almost the whole field of vision, and the colour of the wreaths deepened to black centrally. His written description was accompanied by a beautifully executed water-colour drawing of the spectrum. Though his health is generally pretty good, yet at the time his strength was rather reduced by over-work at his profession; he had had several slight attacks of bleeding at the nose, and had a bad cold and cough. The external appearance of the eye was perfectly natural. With the ophthalmoscope it was impossible to distinguish the fundus of the eye, the vitreous humour being of a dense grey or smoke colour; here and there only a faint red tinge could be perceived. As the eye moved, numerous small spots and filaments were seen floating across the pupil. In about three weeks the optic disc could be seen dimly as through a fog; soon after, the retinal vessels *could be distinctly* traced, and a small dark red spot, the

source of the hemorrhage, was seen on one side. The improvement was now rapid; and a little more than two months from the seizure, the sight of the eye was completely restored, though it was apt to become dim after a few hours' work; and a few floating filaments could still be seen in the vitreous humour with the ophthalmoscope.

Perhaps the commonest morbid change in the retina is *Hyperæmia*. The colour of the fundus of the eye is changed, being more red, and sometimes of a dark red. A portion of the optic disc is obscured by vessels, or the whole is covered thinly, so that the disc is yet made out, or thick enough almost to obscure it, and then its position may be more readily traced by the vessels as they pass outwards, but which soon lose their distinctness.

This is a state that always exists in a greater or lesser degree with the train of symptoms called by Dr. Mackenzie "asthenopia," and supposed by him to be a defect of adjustment only. His reasoning is clear and powerful; his conclusion is inevitable, and, but for the opthalmoscope, every one must have been satisfied with it. It is worth referring to as an example of the fallacy that we may fall into by attempting to solve such subjects by reasoning alone. I may recall the disease to the minds of my hearers by telling them that "asthenopia" is the "choroiditis" of Tyrrell. It is the affection of those who for the most part get their living by minute sight. They are obliged to discontinue with their work, because the vision as it were gets fatigued, and the objects confused or indistinct, and vanish from view altogether. A slight cessation, a wink of the eyes, a look at a distinct object, or, in the worst cases, a pause for a minute or two, enables them to continue. It is strange that the indistinctness comes on when the vision is required to be sustained on minute things only, and not when large and distant objects are looked at. But it is not peculiar to the overworked eye. I have seen it in children learning to read, in the idle man of pleasure, and in the vigorous mechanic. I begin to think that it is almost a defect of organisation, rendered worse or apparent under much or peculiar use of the eye, and the general inutility of treatment, short of cessation of that employment which renders it annoying, tends to this conviction. I could fain dwell longer on it.

Among other ophthalmic revelations of retinal changes, are those of *dots*, or *patches of white*, usually seen in the aged, and associated with feeble vision. I do not know that their nature has been made out.

*Œdema* of the retina, rendering the membrane pale and of a yellowish tinge, less shining than in health, and apparently raised around the optic disc, is fully recognised.

*Pigment Deposit* on the retina, in every form of spot, and in extent from a few dots to almost complete covering, with but small interspaces, is the most remarkable change in this tunic, and not less in the absence of any co-existing external symptoms. I have seen an amount of deposit that seems incompatible with any vision on first examination of a patient for slight feebleness of vision, which is, of course, a physiological puzzle. Many times I have doubted the visual power till I have carefully tested it.

*Choroid.*—The choroid, like the retina, becomes *hyperæmic;* but the most striking change is *Atrophy*, chiefly manifested by the removal of its pigment; a common consequence of chronic inflammation. The alteration generally begins near the optic disc. At first a yellowish change is seen, and then the orange-coloured choroidal vessels appear, and these become more distinct, ultimately disappear, and leave a white spot, which is often mistaken by the inexperienced for the optic disc, especially if the disc be injected. With the loss of pigment, there is a deposit of grey or deep black spots in other parts, as if the colouring matter was removed from one portion and taken to another. Associated with this, extravasated blood, in small quantities, is not very unfrequent, so that the whole fundus of the eye has a mottled and variegated appearance, and withal a person may be able to read. These alterations rarely exist without some disease of the retina.

Early in my professional career, indeed in my studentship, I suspected *that in so-called Syphilitic Iritis* there was something more than the name at *first led me to* infer, and this chiefly on account of the subsequent damage,

the very defective eye that remained after the iris had recovered. I was particularly attracted to this in a young woman, during my dressership. The eye seemed perfect when the inflammation was passed, yet she could hardly count my fingers. Well do I remember this being attributed by the gentleman, whose patient she was, to deficiency of active treatment in the first instance, a remark which explained nothing. The evidence of implication of the eyeball by several local and general symptoms were unheeded by me, for I looked on them, as I was taught, merely as the consequences of an inflamed iris, that being the part which the disease primarily attacked, and on which it spent its violence. It was with inexpressible delight, some years after, that I read Dr. Jacob's most valuable monograph on inflammation of the eyeball, and became convinced by it that iritis, so called, was not disease of the iris alone, but general inflammation of the eyeball; and, indeed, who ever saw the iris discoloured and paralysed by inflammation from syphilis, or rheumatism, or struma, without vision being materially impaired? I have ever found that Dr. Jacob's writings stand the test of time, and are less eclipsed by the advancement of knowledge than those of any modern ophthalmic author. Ophthalmoscopic examination does not render them obsolete, but adds confirmation.

It is no longer a secret why syphilitic eyes become spoiled. But with faint implication of the iris, and little symptom of redness and pain, the retina may be spoiled, as we see by the deposit of lymph, and choroidal damage is subjoined by structural alterations. I once gave a wrong prognosis in a case of rheumatic inflammation of the eyeball, that annoyed me for a long time, and created a great desire to discover the cause of an eyeball being so insidiously lost. With but very slight external evidence of implication of the iris, I gave assurance of complete recovery; but, to the great disappointment of all, blindness ensued. This, together with ophthalmoscopic experience, has taught me always to give a guarded opinion, and I may here remark, with regard to prognosis in general, that most of us are too apt, in critical cases, at a great risk to our reputation and comfort, and without any adequate advantage to ourselves, to be too prophetic, and to promise too much. It is true that patients almost force us into it, and when we commit ourselves, it is our desire for a favourable issue that betrays us.

A more extended knowledge of syphilitic disease of the eye, has convinced me that the posterior part of the eye, certainly the retina and the choroid, and perhaps also the vitreous humour (which is indicated by haziness and flocculi), are always involved. I never saw an example without impaired vision, and all observers know that vision may become misty prior to any palpable iritis. Before I ever looked through an ophthalmoscope, I made the vision the chief ground of the diagnosis in the incipient stage of the disease. Many a case of reddened eyeball presents itself, and looks so like iritis, that the most experienced observer cannot say at once that there is or is not this affection; but the test that never fails me is trying the seeing power, and if I find no deficiency here, I am sure that there is only surface inflammation.

A surgeon in very large practice, in London, and well known to all of us, sent very lately for me to see a patient whom he had attended with rheumatism, and now, as he supposed, afflicted with rheumatic iritis. The eyeball was much injected, and whether superficially or not, I could not tell. This iris was clear. I tried the sight, and when I found that the almost microscopic inscription on my watch could be read, I was satisfied that there was no iritis—no "eyeball-itis." The calomel and opium pill, which was just commenced, was left off, the usual diet was allowed, and an astringent lotion given. In a few days the eyes were well.

More than all this, the ophthalmoscope teaches us that the retina may be the sole seat of syphilitic action. My attention was first drawn to this in a medical pupil saturated with secondary syphilis, and who lost the sight of one eye without any external symptoms. Internal inspection, however, showed that there was inflammatory exudation over the fundus of the eye.

obscuring the optic disc and retinal vessels. When I was last consulted, fortunately for him, his sight was almost clear.

Among the appearances at the fundus of the eye, after the acute stage of syphilitic eyeballitis has passed away, are dots and patches of deposit of different forms and sizes, more or less darkened with pigment, over the retina. The optic disc is often changed, sometimes contracted and ill defined, and greyish; sometimes, apparently, more spread out, and presenting many abnormal aspects. The choroid becomes patchy with white, yellow, and brown; its pigment is lost in spots, allowing the vessels to be seen, and exposing the sclerotic coat.

The ophthalmoscope has not increased the difficulties of ophthalmic practice, on the contrary, it renders it easier, after a man has taken the requisite trouble to learn certain preliminaries. What time used not to be spent in investigating cases of impaired vision, to discover to which of the subdivisions of amaurosis it belonged? How numerous the questions, how tedious and unsatisfactory the examination? Now we at once have a peep, and see things as they are, and when we are puzzled, the patient is not made to suffer for it. I believe, too, that the ophthalmoscope will reduce ophthalmic literature; the treatises will devote less space to the diseases of vision, because less is needed. There will be more fact, which is always put in a small space, and less of wordy theory.

There is just one topic, respecting therapeutics, that I venture to introduce. Do not abandon a patient, and say that he is incurable, merely because you see this or that change in the eye; it is one of the dangers of a speciality. Let nothing short of disorganisation prevent you from seeking indications of treatment, and catch at the shadow of a chance. We know so little of the natural reparative power, of function and of vitality, that we ought not to dare, except under remarkable and well recognised conditions, to give a fatal prognosis.

Do not look on a diseased eye merely as such; remember that this organ is but a part of a whole, and that recovery, or partial recovery, often ensues contrary to the expectation of the most experienced. Which of us has not seen this exemplified in disease of the lungs, and kidneys, and liver, by specialists in these departments?—*British Med. Journal, June* 16, *August* 4 *and* 18, 1860, *pp.* 452, 597, 643.

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91.—*On the Surgical Treatment of Short Sight.*—Mr. J. V. Solomon, of the Birmingham Eye Infirmary, has hit upon an operation by which the focal range of short-sighted persons, whose corneæ are not conical, may be doubled in length. He has found the plan equally successful, whether the eyes are prominent or small, the aqueous chambers deep or shallow. He has tested the operation on cases varying from the age of 12 to 45 years. A man of the latter age, who had worn double concave glasses of immense depth (No. 16) for a great number of years, and, unaided by lenses, could see with clearness the features of a person to know them at a distance of nine feet only, obtained at once by the operation an increase of seven feet in his focal range. In a child of 12 years of age, the operation increased the reading distance from four to eight inches, and the power of identifying persons' features from twenty to forty yards; and in one of 16 years, the effect was still more remarkable.

These results have been obtained by dividing in a *transverse* direction some fibres of the muscle of the lens—the ciliary muscle. Mr. Solomon does not consider it material which part of the muscle is selected for division, but generally prefers either the upper or the lower part of the circle. Supposing the latter situation to be selected, and the patient to be seated in a chair, the operator stands behind, and fixes the globe with the left fingers, as in extraction, holding a cataract-knife in his right hand; with the flat of the blade directed *upwards*, he pushes the point in succession through the *corneo-sclerotic union*, the pillars of the iris, and the ciliary muscle. The *direction given* to the instrument is obliquely downwards and outwards.

Care is taken that the incision in the muscle is of the same length as the puncture of entrance; namely, about two or two lines and a-half in diameter. In some cases, Mr. Solomon has found that the power of adapting the eye to distant objects has been increased by practice and lapse of time.

In a young man who had been myopic from his childhood, and had suffered for the last three years from congestion of the retina, the visual power and focus have been so much increased that the outline of large buildings at a distance of a mile and a half can now (six weeks after operation) be distinctly made out; whereas, before the ciliary muscles were divided, they appeared as mist.—*British Medical Journal, May* 26, 1860, *p.* 393.

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92.—*Rheumatic Ophthalmia treated by the Galvanic Current.* By HARRY LOBB, Esq., M.R.C.S.—R. J. had suffered more than fifteen years from repeated attacks of rheumatic ophthalmia, through which the pupil of one eye had become permanently contracted, and the sight of the other much injured. Being warned that a repetition of these attacks would be followed by total loss of sight, he was led to consider whether some means of prevention hitherto untried might not exist. He had found by experience that galvanism applied to the limbs removed rheumatic pains; he therefore thought that a modified current applied to the eye at the commencement of an attack might prevent its occurrence, and if applied for a sufficient length of time the cure might be permanent.

On the approach of the next attack, he accordingly applied an excited thirty-six link Pulvermacher chain, the positive pole upon the spine, the negative upon the closed eyelid, upon retiring to rest. This was allowed to remain on during the night. In the morning, every trace of the disorder had vanished.

Other attacks followed at gradually increasing intervals of time, but they were invariably arrested by the same means, the consequence being that, during the last five years R. J., has found that his eyes have been gaining strength, enjoying the uninterrupted use of his sight during the whole time.

The above case, communicated by the patient himself, requires no comment from me, as it is related in so plain and straightforward a manner. At the same time I may corroborate it by stating, that in all cases of ophthalmia for which I have used the continuous galvanic current a rapid cure has been effected.—*Lancet, Sept.* 1, 1860, *p.* 210.

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93.—*A Simple Mode of treating a frequent form of Entropion.* By M. SICHEL.—There are varieties of this affection which only yield to appropriate operative procedures, but there is one form which almost always precedes the others, and which can be relieved in the great majority of cases by very simple means. It is well known that in ophthalmia accompanied by violent photophobia, and by great distension of the vessels of the conjunctiva, the patients insensibly acquire the habit, especially when the inflammation has become chronic, of strongly contracting the eyelids, with the double aim of excluding the rays of light and of expelling the foreign bodies which, from their sensations, they believe to be present at the front of the eye. This constant contraction of the orbicularis at last inverts the free edge of the lower eyelid, the very narrow tarsus of which yields more readily to the blepharospasm than that of the upper eyelid, which has a more considerable vertical diameter. After a while the cartilage of the lower lid can no longer resist this incessant action, and it undergoes an incurvation which becomes more and more considerable, until at last complete inversion is produced. When the affection has attained this degree, the means now to be mentioned are usually of no avail.

The entropion is confined *in most* cases to the lower eyelid. It is most frequently *so after chronic ophthalmia* and the operation for cataract, when

the patient, either from indisposition to open them or from excessive sensibility of the retina, keeps the eyelids too long closed. In this variety of entropion termed spastic, the advice is usually given to practise frequent tractions on the lower eyelid, or to keep it everted on the cheek by means of adhesive strips. In the author's hands these means, powerless against the permanent contraction of the orbicularis, have always failed, while a very simple manœuvre has generally succeeded. In the place of producing complete abduction and depression of the eyelid, it should be only moderately depressed, and stretched vertically by means of the index and medius fingers, so as to sensibly separate it from the anterior surface of the eye. The pulp of one of the fingers is next applied above the inferior edge of the orbit, upon the adherent part of the eyelid, and gently carried from before backwards into the anterior portion of the cavity, until the free edge of the eyelid is strongly everted. The pressure must be made very gently, the finger sliding along the anterior portion of the floor of the orbit without compressing or irritating the globe. This simple manœuvre, repeated every quarter of an hour or oftener by the patient himself, leads to the re-establishment of the eyelid in its normal position. The same procedure, with some modification, may be advantageously employed for the treatment of entropion of the upper eyelid—an affection which, indeed, is much more rare, and infinitely more difficult to cure.—*British and Foreign Medico-Chirurgical Review*, *Oct.* 1860, *p.* 544.

# MIDWIFERY,

## AND THE DISEASES OF WOMEN, ETC.

### 94.—NEW PATTERN FOR THE OBSTETRICAL FORCEPS.

By Dr. Samuel T. Knight, Baltimore.

[Dr. Smellie's short forceps have been in common use in America and England for many years, modified by various practitioners. Dr. Davis's forceps are nothing more than one of these modifications. Dr. Knight's forceps have a lesser pelvic curve, have no fenestra, have smaller blades, taper more gradually from the handles to the blades, and the blades are more convex, especially near their termination; and, finally, the blades are more narrow, rendering their introduction simpler. All these seem to us to be great advantages. We consider that the forceps, if made to be introduced more easily, especially the upper blade, and if made to fit the head better, might be much more frequently used. These forceps of Dr. Knight's are the best kind we have seen engraved, and we only wish he had sent us a pair from America, that we might have shown them to our friends.]

The objections to Davis' or Hodge's forceps (as they are variously termed) offered for sale by the instrument makers, and the only ones now kept as stock on hand by them, are—1st. The pelvic curve is greater than necessary, according to the laws of mechanics. 2d. The suddenness of the curve, with the fenestra beyond, demands too great an amount of metal at the point of curvature. This is the precise spot where the greatest volume of the head is brought in contact with the perineum, when these instruments are used in extracting, and by this abrupt tension endangers, one might almost say insures, in unskillful hands, the rupture of this part. 3rd. The large size of the fenestra allows too free a protrusion of the fœtal cranium through them. By this bulging of the cranium, the biparietal diameter of the fœtal head is but little reduced. 4th. The fenestra increases the breadth of the blades more than is convenient or necessary, particularly in many pathological conditions for which the forceps are especially applicable.

Those who have constructed the forceps, by taking a cast of the curve of the bones of the pelvis denuded of the soft parts, have seized a physiological, but not a pathological, idea. The difference of the axis of the outlets of the recent and of the bony pelvis is known to all obstetrical students. Nothing can be more false, therefore, than to adapt the pelvic curve of the forceps to the axis of the cavity of the bony pelvis and its outlet. The position of the patient during the operation must also be taken into consideration. Were it required to operate whilst the patient was in horizontal position, stretched upon the plane of a bed, the pelvic curvature of the forceps would have to be increased. But, in the proper position for the application, the depression of the handles of the instrument easily compensates for the curve in the blades. It must be granted that the straighter the blades, the greater the mechanical advantage and the increased certainty of action. This is particularly manifest at the point of outlet over the perineum. When the curvature of the blades is great and sudden, a sufficient elevation of the handles cannot be obtained by the operator with proper mechanical effect. The danger to the perineum, from the curvature of the blades and their sudden springing from the handles, is not the only objection to the physiological forceps of the learned Professor of the University of Pennsylvania. The fenestra are not only useless, but injurious. They weaken the blades and render them *more likely to slip*, and, when they slip, they tear and mutilate

the fœtal scalp. Moreover, the protrusion of the soft parts through them increases the resistance more than the polished metallic surfaces of the plain blades. When the forceps with plain blades, according to the measurements given, are properly applied, it is impossible for them to slip. They grasp the head accurately, and the strength of the blades makes good their position. The proper force to be used is, therefore, more easily estimated by and is entirely under the control of the operator. The fenestra weaken the blades, rendering them, to a certain degree, flexible and elastic, oftentimes yielding before a resistance, both justifiable and necessary to accomplish the end in view.

The breadth of the blade of the plain forceps and the moulded convexity *of their interior face*, which adapt them accurately to the arch of the cranium, *entirely prevents* any cutting or mutilating of the fœtal scalp. In no oper-

ation, when they have been used, has the print of their application been left. In the gradual taper from the handles to the blades, they approach more nearly to Dr. Smellie's forceps, and at this point give a decided advantage.

| *Measurements of Hodge's Forceps.* | *Measurement of Knight's Forceps.* |
|---|---|
| Whole length, 16 inches. | Whole length, 14¼ inches. |
| Weight, 1 lb. 1 oz. | Weight, 16 oz. |
| From joint to end of handle, 6·8. | From joint to end of handle, 6¼ in. |
| Length of parallel shank, 3·5. | Length of parallel shank, 3. |
| Length of proper blade, direct line, 6. | Length of proper blade, direct line, 6. |
| Breadth of blade at broadest part, 1.8. | Breadth of blade at broadest part, 1¼. |
| Perpendicular elevation of points of blades where the instrument is laid on a horizontal surface, is 3·4, which indicates the degree of curvature of the blades. | Perpendicular elevation of points of blades where the instrument is laid on a horizontal surface, is 2¼, which indicates the degree of curvature of the blades. |
| Breadth of fenestra, 1.1. | No fenestra. |

"Hundreds of modifications," says Cazeaux, "have been proposed in the forceps since the days of Levret and Smellie, nearly all of which have fallen into oblivion. Some of them were quite ingenious, but they imperfectly attained the end their authors had in view, and others were nearly destitute of value or utility." The present article would not have been penned were it not for the honest conviction, obtained by a sad experience, that it were better for suffering females that those now in common use throughout this country should *also fall into oblivion.* The particular faults in their construction have been pointed out, and it is to remedy these that the present instrument is offered.—*Maryland and Virginia Medical Journal, April* 1860, *p.* 272.

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## 95.—THE FORCEPS AND PERFORATOR COMPARED.

By Dr. Charles David Doig, Louth.

Towards the close of last year an article was contributed by me, and printed in the 'Medical Times and Gazette,' on the subject of Cross Presentations. The principal points illustrated in that paper were the following:—

1. That in the event of cross presentation of an average-sized fœtus in an average-sized female with a normal pubis, the result to be anticipated is rupture of the uterus and death of the mother by exhaustion.

2. That laceration of the uterus may also occur in the course of a natural labour.

3. That if the mother be not deformed, and have an average-sized pelvis, while on the other hand the contained fœtus is comparatively small, as in premature births, or abortions, the mother may be delivered in safety by the natural powers.

4. That podalic version is an efficient means of delivery as far as the life of the mother is concerned, and not necessarily fatal to the life of the child.

5. That the operation should be performed as soon as a transverse presentation is diagnosed, and the vaginal and uterine passages permit the introduction of the hand.

6. That in cases of version from all causes at the ninth month, one mother in fourteen is lost, and sixty per cent. of the children still-born.

It is unnecessary to carry these inferences further; I shall therefore briefly consider the treatment applicable to another class of difficult labours, which forms the immediate object of this article.

One of the diseases liable to be developed in the course of protracted parturition is vesico-vaginal fistula. Mr. I. B. Brown, who has conducted the treatment of twenty-six cases of this disease, and published minute details thereof, maintains as the result of his experience and observation, that vesico-vaginal fistula had *in almost every instance* been consequent on protracted

pressure of the child's head on the mother's vagina, and strongly recommends obstetricians not to sit days and nights with women in labour, but at once afford to the patient the aid adapted to the circumstances of the case. "Dr. Veit" shows, "1. That the danger for the child when the birth is completed within twelve hours, is only half as great as when the labour is protracted to twenty-four hours, and that further protraction is still more dangerous. 2. The danger is increased when the second stage of labour lasts longer than two hours. 3. The male sex is more endangered than the female." Protracted labour may then not only be the means of impeding the perfect recovery of the mother by originating dangerous complications, but also of endangering the life of the child.

Delivery by the forceps and perforator are long established obstetric operations. The following cases indicate the principal distinguishing feature of these two methods of delivery. The first does not of necessity destroy the life of the child; by the second the life of the child is intentionally sacrificed.

*Case* 1.—In 1852 I was requested to visit a female in lingering labour. She mentioned to me that she had been very ill all night. On examination I ascertained the passages to be well dilated, the presentation cephalic, and the existence of no impediment from retained urine and fæces. After waiting several hours to assure myself that the natural powers were acting inefficiently, I called on a medical friend (Dr. Gillespie, Leith), to get the loan of a pair of long forceps, as I had not a proper pair by me. We visited the patient together, and extracted the child instrumentally. The child was alive. The mother made a good recovery. Low diet, a dose of opium, and a general aperient, were all the remedies adopted in the after-treatment of the case.

*Case* 2.—In October, 1854, I was requested to attend a patient at her confinement. Mrs. C. was a primapara, about 30 years of age, and resident at St. Mary's-place, near Wapping. I sat up all night with the lady, and towards morning, getting weary of the charge, determined to extract by the forceps if such could be accomplished. She had made no progress, and there was no difficulty in satisfying myself as to all the points of the presentation. However, the space left for the application of this instrument was so slight in consequence of the large size of the fœtal head, and the swollen state of the surrounding parts, that the blades could not be introduced; I determined therefore, to consult with Dr. Lever, of London, before performing craniotomy. It was his opinion that the use of the forceps was impracticable, and perforation imperatively demanded. We accordingly anæsthetised the lady, introduced the scissors, broke up the nervous matter of the child, and extracted. The mother recovered well with very simple treatment. She was put on low diet, took small doses of calomel and antimony, one or two aperient senna draughts, and tonics.

Dr. Lever was of opinion that the child had been dead some days, which removed any qualms of conscience that we might have had, had we been breaking up the cerebro-spinal axis of a mature living child.

In these, as in all other cases, it has been my custom to apply the binder after delivery. It is important always to have the attention directed to the state of the rectum and bladder, as accumulation of urine or fæces retards parturition, and their removal by the catheter or enema further delivery. No such obstacle occurred in these cases.

The long forceps is now in more frequent use than the short. The instrument consists of two doubly curved fenestrated blades, variously shaped, and constructed according to the taste and judgment of the Practitioner. The necessity for their use being determined, the patient is placed on her left side at the edge of the bed, the passages lubricated if necessary, and the forceps warmed and greased. The blade is gently insinuated with its concavity applied to the convexity of the child's head, the upper or anterior blade being applied first, then the inferior blade. If the forceps do not *lock, a blade must* be withdrawn and re-applied so that this object is attained.

The forceps are efficacious partially by compressing the fœtal head, but more particularly by a kind of undulatory traction which the operator should exert in the direction of the axis of the pelvis during uterine action, or if this be wanting, at intervals. In using such force as is needful to change the position of the head, great care is requisite to avoid injuring the soft parts of the mother, or lacerating the perineum.

The instruments required for the performance of craniotomy are a pair of scissors with shoulder-straps, a crotchet, and a pair of bone forceps. Besides these there are many other weapons that are found of service in particular cases. The scissors or perforator have sharp outer margins. Either should be introduced by the right hand, the left hand being used as a guide and guard, through a parietal bone and not through a suture, pushed into the skull and moved about in such a way as completely to break up the brain and medulla oblongata. The force should be applied at right angles to the parietal bone, to prevent the instrument slipping and tearing up the scalp or doing other injury. The crotchet is then used to complete the operation. It may be used to tear up the brain as well as to extract. It can be fixed on the parietal bone, on the occipital, or on the foramen ovale. Before using strong extraction force with the crotchet the firmness of the hold should be tested. The craniotomy forceps become useful in many instances as a means of applying traction to the child. It is a rule only to work during the pains.

Such are the formidable instruments that were required in the treatment of the two cases detailed. We come next to consider what are the cases in which the forceps should be applied, and what are those suitable for the perforator. Dr. Davis used the forceps six times among 7302 deliveries:

1. Three were uncomplicated head cases.
2. Two were first children in twin cases.
3. One was puerperal convulsions.

Dr. Ricker, of Nassau, mentions the indications for the use of the forceps in a large number of instances which, tabulated, give the following ressIts:—

| | | |
|---|---|---|
| 1. | Disproportion between child's head and mother's pelvis | 287 |
| 2. | Feebleness and absence of pains | 269 |
| 3. | Weakness, exhaustion, and illness of the mother (hernia, phthisis, &c.) | 33 |
| 4. | Prolapsus of the umbilical cord with the head | 29 |
| 5. | Spasmodic and preternaturally painful pains | 22 |
| 6. | Face presentations | 20 |
| 7. | Ecclampsia and convulsions | 12 |
| 8. | Prolapsus of smaller parts of child with head | 8 |
| 9. | Placenta Prævia, 3; other hemorrhages, 7; total | 10 |
| 10. | Oblique presentation of the head | 7 |
| 11. | Rigidity of the structure in elderly first-bearing women | 4 |
| 12. | Great swelling of soft parts | 4 |
| 13. | Erysipelas pudendorum | 7 |
| 14. | Putrescence and consequent unusual centractility of uterus | 1 |
| | | 713 |

Dr. Davis performed craniotomy nine times among 7302 deliveries:

| | | |
|---|---|---|
| 1. | In consequence of deficient pelvic space | 3 times. |
| 2. | ,, ,, pelvic contraction caused by hip-joint disease | 1 |
| 3. | ,, ,, carcinoma uteri | 1 |
| 4. | ,, ,, patient aged 14½ years | 1 |
| 5. | ,, ,, face directed to pelvis | 1 |
| 6. | ,, ,, *rigidity of* os uteri | 1 |

The preceding data indicate that deficient pelvic space is one common condition requiring operative procedure; in what cases, then, of pelvic contraction may the forceps be applied with success, and in what must the perforator be resorted to? It is difficult to come to a definite conclusion on this point, as each case requires to be considered individually. In the instance detailed, craniotomy was performed in consequence of disproportion between the child's head and the pelvis, attended by comparative feebleness of the pains. In many cases, however, requiring this operation, or the less serious one, the forceps, the pelvis is distorted, as well as of an unequal size. Although it is impossible to lay down rules applicable to every individual instance, we can come to conclusions, which admit of general application. The conjugate diameter of the normal pelvis varies from four inches at the brim, to four and a-half inches at the outlet. Dr. Simpson, of Edinburgh, used to lay down the following rules that the forceps could be applied if the conjugate diameter was three and a-quarter inches, and that embryulsio was to be adopted when the conjugate varied from one and three-quarter inches to three and a-quarter inches.

We come next to inquire what is the frequency of these operations compared with other obstetric operations, and with midwifery practice in general; and what is the mortality attending the operations. The results of the statistics of different authors are more various than is desirable, and are exhibited by the following figures:

| | Labours. | Forceps. | Proportion. | Craniotomy. | Proportion. |
|---|---|---|---|---|---|
| Dublin.....Clarke..... | 10,199 | 14 | 1 in 728 | 49 | 1 in 248 |
| „ ... Collins..... | 16,654 | 27 | 617 | 118 | 141 |
| Paris.....Baudelocque. | 17,388 | 31 | 561 | 6 | 2898 |
| „ .....Lachapelle.. | 22,243 | 76 | 293 | 12 | 1854 |
| „ .....Boivin..... | 20,517 | 96 | 214 | 16 | 1282 |
| Vienna....Bon..... | 9589 | 35 | 274 | 13 | 737 |
| Heidelberg.Naegele.... | 1711 | 55 | 31 | 1 | 1711 |
| Berlin.....Kluge..... | 1111 | 68 | 16 | 6 | 185 |
| Dresden....Carus..... | 2549 | 184 | 14 | 9 | 283 |
| Berlin.....Siebold..... | 2093 | 300 | 7 | 1 | 2093 |
| London....Davis..... | 7302 | 6 | 1217 | 9 | 811 |
| Nassau.....Ricker.... | 304,150 | 4223 | 72 | 165 | 1843 |

The following figures give more minute particulars with reference to the frequency of these two operations in British Midwifery—

| | | | | | | |
|---|---|---|---|---|---|---|
| Simpson..... | 1 Forceps in | 472 | cases. | 1 Craniotomy in | 1417 | cases. |
| Lever..... | 1 „ | 518 | „ | 1 „ | 186 | „ |
| Churchill..... | 1 „ | 546 | „ | 1 „ | 149 | „ |
| Ramsbotham.... | 1 „ | 611 | „ | 1 „ | 805 | „ |
| Collins..... | 1 „ | 617 | „ | 1 „ | 141 | „ |

It may therefore be with safety inferred that in British Midwifery forceps cases occur once in 550 births, and craniotomy once in 530 births.

With regard to the mortality attending forceps cases, Dr. Ricker mentions that out of 4223 instances 93 women died either during, or soon after, the operation, and 684 children were still-born. Hence 1 woman out of $48\frac{38}{93}$ deliveries by the forceps died, and 1 child out of $6\frac{419}{684}$.

Dr. Ricker also states the results attending the breaking up of the fœtus, which may be arranged under two divisions:—

| | No. | Proportion. | Lived. | Died. | No Information. |
|---|---|---|---|---|---|
| Perforation..... | 143 | 1 in 2196 | 88 | 35 | 20 |
| Embryotomy.... | 22 | 1,3825 | 16 | 6 | |

In the course of the preceding remarks have been considered—1. The mode of using the forceps; 2. The method of practising craniotomy; 3. The circumstances demanding the performance of these two operations; 4. *The diameter of* the pelvis that admits of the use of the forceps or the per*formance of* craniotomy; 5. The comparative frequency of the two opera-

tions; 6. The success attending each. The investigation might be profitably carried to a much greater length, but the object of this paper has been rather to associate this statistical inquiry with actual cases, than enter into minute details.—*Med. Times and Gazette, July* 28, 1860, p. 74.

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## 96.—ON A NEW METHOD OF VERSION IN ABNORMAL LABOUR.

By Dr. Braxton Hicks, F.L.S., Assistant Physician-Accoucheur at Guy's Hospital, and Physician to the Royal Maternity Charity.

[Several continental writers have mentioned the possibility of version by external manipulation,—as Wigand, Mattei, and Stolz; and Martin, of Jena, has related many cases in which he was successful in turning by external manipulation. All these writers have used external manipulation *only*.]

Before I had read their plans of operating, I had already employed a method, combining the power possessed by external manipulation with the power and certainty derived from pressure and touch through the cervix, and which, even since the perusal of them, appears to me more satisfactory, and more easily managed by those who have not given, nor are able to give, to the external diagnosis of fœtal position such care and skill as Prof. Carl Esterle; for even Martin, of Jena, insists that by his plan it is necessary—1st, that immediate delivery be not called for; 2nd, that there be a capacious pelvis; 3rd, that there be active pains; 4th, that the child be living. From the following remarks and illustrative cases, I think I shall show sufficiently that the method I have employed does not require such conditions. Accepting, then, the valuable fact, that the fœtus in utero can be turned without the necessity of the entry of the hand into the uterus, I shall now prove the extensive applicability of the plan I have adopted to labour in its various complications.

I do not consider it needful to stop here to prove the major proposition, because it will be immediately acknowledged, after a few trials on the plan described below. The points to which I wish therefore to draw the attention of the profession are—1st, the most certain method of operating; 2ndly, the general reasons of its superiority to other methods; and 3rdly, the cases to which it is more particularly, and also exclusively, adapted.

In considering the first point—viz., the most certain method of operating, it will be necessary to reflect upon the principal facts in regard to version. One of the most important, and one which more particularly bears upon the method presently to be alluded to, is, that if we place by any means in our power the fœtus in utero in a transverse direction and abdomen downwards, the knees of the fœtus are opposite the os uteri: and if the os is large enough to introduce a finger, the knee can be hooked down and the presentation secured. Thus it is evident that it is merely necessary for the fœtus to assume the above position, where delivery by knee or foot is sought for, or where we wish that the delivery may be at our command. I shall defer alluding to version of cephalic into pure breech presentation till I speak of the method of operation.

The next fact to be borne in mind is that, should the fœtus, either naturally or by the method to be described, present by its back to the os uteri, the case would be very rare in which one side or the other was not the more dependent, and consequently one or the other knee be within finger-reach of the os.

The third fact needful to reflect upon is this, that the shape of the cavity of the fully-developed uterus being oval, with the long axis placed vertically, the tendency of the slight pains towards the end of pregnancy is to place the longer axis of the fœtus in a corresponding direction; or, in other terms, to cause it to present by the head or breech: so that if the fœtus be at any time situated transversely, a slight preponderance of force in either direction will be *sufficient to determine* the ultimate presentation. It will be

thus perceived that, the head being heavier than the pelvic extremity, the natural tendency of a spontaneous rectification will be to head presentation; but at the same time it would have taken but little force to have caused it to rise to the fundus.

Having premised these considerations, we will discuss the other facts as they arise in the application of the above principles, taking for granted that it is admitted possible to turn the fœtus in utero from the outside. The method I have found successful, and very easy of application, is conducted thus:—We will suppose the simplest condition, a case where the uterus is passive, membranes unbroken, the liquor amnii plentiful, the os uteri expanded sufficiently to detect the presentation, which is cephalic, and in the first or fourth position (occiput to left side); the patient is in the ordinary position, the trunk curved forwards as much as possible, to relax the abdominal muscles. Introduce the left hand, with the usual precautions, into the vagina, so far as to fairly touch the fœtal head, even should it recede an inch. (This generally requires the whole hand.) Having passed one or two fingers (if only one, let it be the middle finger) within the cervix, and resting them on the head, place the *right* hand on the *left* side of the breech at the fundus uteri. Employ gentle pressure and slight impulsive movements on the fundus towards the right side, and simultaneously on the head towards the left iliac fossa. In a very short time it will be found that the head is rising and at the same time the breech is descending. The shoulder is now felt by the hand in place of the head, it in like manner is pushed to the left, and at the same time the breech is depressed to the right iliac fossa. The fœtus is now transverse; the knee will be opposite the os, and, the membranes being ruptured, it can be seized and brought into the vagina.

Having now the labour at command, the case must be treated according to the circumstances which call for turning. In obedience to the law above stated, when the fœtus is placed transversely, a slight impulse will determine the final position of the head. When the leg is seized, therefore, it is advisable to place the right hand beneath the head in its new position on the left side, and gently press it towards the fundus. The same law renders it very easy to convert the cephalic, shoulder, neck, or even natural transverse, into a breech case. In either of these conditions it is merely necessary to push up the head, and, removing the left hand from the vagina without bringing down the knee or foot, place it on the breech in the right iliac fossa, so as to depress it into the cavity of the pelvis. No extra force should be used, for it will be found to obey a very "gentle persuasion." Indeed, the change is completed sometimes spontaneously, and the foot is at hand before it is expected. The breech now presenting, it is advisable to retain it till labour is fairly set in, by a firm bandage placed externally, and, should it be required, a pad on either side of the fundus. It should now be treated as an ordinary pelvic presentation.

We have in the above case supposed the fœtus to have been placed with the back to the left side (first and fourth positions). Should it, however, be certainly ascertained to be placed in the opposite direction (second and third position), the alteration required is to make the pressure and impulses in the opposite directions, following the same rules. But suppose the exact position cannot be made out: in that case it should be considered as being in the first and fourth, and treated as above directed; and for the following reasons:—1st. The fœtal head presents in that position (back to left side) in from sixty-five to eighty per cent.; consequently the practice would be correct in that proportion. 2nd. Should the child be turned over on its back, one side is almost certain to be the most dependent; consequently a knee or a foot will be within a finger's reach. 3rd. Should the lower limbs not be near, it can be readily converted into a breech case, whereby the breech will be close to hand. Suppose, again, the liquor amnii has escaped—although it certainly does make the operation more difficult, still, as the case to be quoted will show, it is easily managed even then, the principal impediment *being the activity* of the uterus; should that render it troublesome, suspend *it for the moment*, either by chloroform or opium, and I believe the escape

of the waters will be found to be no objection to operation. With respect to other conditions which may retard or interfere with it, they must be treated in the same manner and on the same principles as apply to the ordinary method, and require the same judgment on the part of the operator.

In considering, secondly, the general advantages of this operation over the ordinary method, I disclaim all intention of unnecessarily depreciating an exceedingly valuable and ancient operation—one which has saved numberless lives, and one with which, at present, we cannot dispense. Still, if it can be shown, that in a considerable number of cases requiring version, the operation can be accomplished as quickly, or even more so, without the necessity of introducing the hand into the uterus, with the exception of one or two fingers passed a little way into the os, I am sure that such a modification of this more or less hazardous operation will recommend itself without any panegyric on my part. For in that case it will readily be perceived that we shall avoid—

1. The addition of the hand, and perhaps arm, to the uterine contents; and the irritation, present and future, caused by it.
2. Entry of air within the uterine cavity.
3. Liability to rupture of uterus.
4. Much of the pain and distress felt in the ordinary plan.
5. The removal of the coat, and baring the arm of the operator; and, as a minor consideration,—
6. The fatigue and pain endured by the operators while the hand is in utero.

I think the removal of these objections a matter of considerable importance, even if option were given us in all cases; but more especially so when we add to this the power the plan I have suggested gives us—namely, of being enabled to turn under circumstances totally impossible by the older method. I feel confident that all impartial practitioners will, after a fair trial, adopt it—I will not say to the total exclusion of the other, but certainly upon every possible occasion in preference; for there is one thing upon which much stress can be laid, which is, that the unsuccessful attempts to accomplish the new method, instead of increasing the difficulty of delivery by the older plan tends rather to make the latter more easy; at any rate, the fœtus can easily be returned to its original position.

Prof. C. Esterle has shown great skill and patience in endeavoring to detect during the seventh and eighth months of pregnancy the position of the fœtus, and deserves great praise for practically showing the possibility of version at that time; but as such opportunities are rarely to be found in ordinary daily practice, and as it scarcely can be expected his skill and experience are to be met with except in such as devote their time specially to that subject,—and as, coupled with other reasons, it is a practical fact that the majority of the malpresentations and complications of labour do not come to the knowledge of the practitioner till labour has set in, or till, as in hemorrhage, active interference is necessary,—I think that I am not overrating the value of the above easily-applied and safe method of version; for the finger inside the cervix gives a sure indication as to progress of the pressure on the breech above.

It may also be noticed as an advantage that the membrane need never be ruptured, as was generally practiced, though not always by the older method.

The third point I proposed for consideration was the question—To what cases is the above method more peculiarly applicable, and to what conditions is it exclusively so?

From the foregoing remarks, it will be readily seen that it can be applied at the earliest period a malpresentation is detectable. As soon as the finger can enter the cervix, so soon can version be performed, converting all forms into breech presentations. In malpositions of the head, perhaps it may be found capable of improving its position without having recourse to complete podalic version; in puerperal convulsions, diminishing the great risk in such cases from the addition of the hand to the contents of the uterus; in narrow

brims, when version is decided upon, it will save the pressure upon the os uteri against the projecting parts of the brim. To these points I shall allude in a future communication, and shall for the present confine myself to the advantages this method gives us in *placenta prævia*—at least, in every form of parietal insertion. I have as yet had no opportunity of testing its applicability in absolutely central insertion, and perhaps we can scarcely hope for any peculiar advantage in that condition, which becomes so formidable if the os continue undilated. Although in most writings it is said the hemorrhage which in these cases has already occurred is generally sufficient to dilate the os, still many will present such a prolonged rigidity of that part, that death may take place before entry of the whole hand can be effected; and it has so happened that this form has in my own experience been by far the more frequent. These cases are the more serious because of the difficulty of obtaining a firm compress on the bleeding part. The contracted cervix generally has been seen where the hemorrhage has begun sometimes before the full term (at the seventh or eighth month), when the lower portion of the uterus has not fully expanded.

*Case* 1.—Mrs. B., of Walworth, a delicate, feeble woman, the mother of six children, about thirty-eight years old, and eight months advanced in pregnancy. For the four previous days she had been losing blood in gushes. Her attendant requesting me to see her, I found her, on my arrival, very blanched, pulse thready, very weak, and quick; voice very feeble; altogether in an exceding low state from flooding. The os uteri was about the size of a crown-piece, thickened, irregular, and unyielding; the placenta presenting three parts across it; the membranes not broken; no pains had appeared; fœtus alive; movements feeble and convulsive; head presenting. Blood was still oozing. I, therefore, gave her plentifully of spirits and water; and introducing my hand into the vagina, I endeavoured to gently dilate the os. After trying some time, with very little effect, I thought it would be a great advantage if I could turn without entering the uterus, and put in practice the method previously described. The head receded to the left side as the breech came down. The shoulders then presented; but in a minute more they followed the head, and then the leg was easily felt through the membranes. Having broken them, I drew the knee into the vagina, and, plugging the os with the leg and breech, and keeping up a slight traction, I waited to allow rallying further. I also gave a dose of secale, with stimulants, to ensure contraction subsequently. As the os expanded, I gently drew down the fœtus, which was fully born (but dead) in about three-quarters of an hour. She did not lose half an ounce of blood during the whole process, nor during the removal of the placenta, which was slightly attached, but not adherent. Had she lost another gush, she must have died at the time. However, the quantity already lost had been too much; for, after lingering out three weeks of extreme exhaustion, she died.

*Case* 2.—Mrs.——, of Castle-street, Borough; aged about thirty-five; thin, but tolerably healthy; eight months advanced in her eighth pregnancy. During the fortnight previous to my seeing her, she had lost blood by gushes and clots, at no time very severe; and about six hours before there was a small gush, with clots, and at the same time the liquor amnii escaped, but no pains followed. I found there was still some bleeding, though not extensive, with clots in the vagina. Her appearance had not been affected by the loss. The os had been dilated to half the full size, but now it was so excessively irritable, that upon the slightest touch of the finger it contracted so that only one and at the most two fingers could enter. The placenta was stretched two-thirds across it posteriorly. It here seemed to me that if the leg and breech of the fœtus could be made to form the plug, the patient would be secure. The only way possible, without much risk, was to endeavour to put in practice the method before described, which was easily done, though not quite so readily as in Case 1, in consequence of the irritability of the cervix; still it was effected, notwithstanding the escape of the waters, without any force or pain, and under circumstances which would have precluded entry of the whole hand. By means of two fingers in the

os, I drew down the knee into vagina; and Mr. Woodman (the senior obstetric clerk) having passed a loop of tape through the bend of the knee, I left the case in his charge, with instructions not to hurry the birth, but to keep up gentle traction, and to plug the cervix. In about three hours, the os relaxed and dilated to its full size, when the fœtus was brought down. It had apparently been dead for a day or two. Not the slightest drop of blood was lost during the whole process of turning and extraction, and the woman recovered without a single bad symptom.

*Case* 3.—Mrs. P., a fine, healthy person, well made, who had always had rapid labours. At the time I saw her, she was at the full term of her seventh pregnancy. Having been seized twice within the previous month with a gush of blood (each time about a half a pint) she was kept quiet. Twenty-four hours before I saw her, she had had the third gush, which had continued ever since. I saw more than a pint, and the nurse said she had lost altogether as much more. At the time of my visit, the face was not much blanched, nor was the pulse much affected. The os uteri was fully dilated, the placenta half-way over it; uterus perfectly inactive. The liquor amnii had escaped, but the fœtal head was high up; no violent bleeding, but considerable oozing. There can be no doubt that the older method of version would have been very practicable. Still, as every circumstance was favourable, I turned by the method previously recommended, and found it very easily and speedily managed. In about two minutes sooner than I expected, the foot was in my hand, and drawing the leg through and the breech to the os uteri, I kept up a gentle traction. No uterine action ensuing, secale was given. In a quarter of an hour pains appeared. After the breech had passed the cervix, finding the funis pulsating, I hastened the delivery, good pains assisting. The child, although not breathing when born, was soon recovered by artificial respiration. No hemorrhage appeared during the operation or expulsion of the fœtus. The placenta not being thrown off soon, and a little hemorrhage appearing, it was removed, being rather firmly attached.

*Case* 4.—Mrs. C. Borough, aged about twenty-eight, of strong constitution. At about three weeks to completion of full term of her fourth pregnancy. About three weeks before she had been seized, after a fright, with sudden and severe flooding. This passed off till twelve hours before I was called to her, when she was taken with a sudden flooding, nearly equal to a chamber-vesselful, accompanied by faintings, which continued during the twelve hours before I saw her. I found her perfectly blanched; pulse very feeble; extremities cold; could still swallow, and had taken about half a pint of brandy. The os uteri was about the size of a five-shilling-piece; the placenta protruding, apparently centrally attached, but on carefully searching I found a portion of it thinner at the posterior aspect, and, carrying the finger backwards, the membranes were reached, unbroken; the head presenting. Being anxious to have command of the labour, with as little shock as possible to the patient in her very low condition, I employed the method mentioned above, with so much ease and rapidity that in about one minute the foot was in the vagina. In this case the membranes were ruptured at the beginning of the version; this, however, made no difference to the accomplishment of it. Continuing to make a plug of the breech, I waited till the system had rallied by stimulants, and in order to allow the os to expand and the pains to come on. In about an hour and a half the pulse improved, and two doses of secale were given; about half an hour later, gentle traction being employed, the child was born dead. The placenta was expelled by natural efforts. No hemorrhage occurred after I commenced version. She recovered satisfactorily.

*Case* 5.—Mrs. ——, the mother of eight children, about eight months advanced in pregnancy, was taken in the night—seven hours before I saw her—with sudden flooding, which was estimated at more than two quarts. Four hours after she sent for her attendant, who found her with her extremities cold, blanched, and lying before the fire. From that time till I saw her she had been losing blood by continuous oozing, considered to be about half a

pint. On my arrival, I found her blanched, pulse scarcely perceptible, voice strong, and considerable muscular power. The os was dilatable, about the size of a crown-piece, but had not been so more than an hour. The placenta came at that time only to the margin. It had before been felt over the os in part. The liquor amnii had escaped; no pains. As the oozing was still going on, I was anxious to secure a plug. I therefore directed and assisted Mr. Wadkins (the senior obstetric clerk) in performing version by the method above described, which was done in a few minutes. The moment the head rose into the iliac fossa, the shoulder presented, and then as instantly the foot was in the hand of the operator. During this time we gave largely of stimulants, and apparently the patient had still considerable power. By the weight of the arm traction was kept up against the cervix, and in about half an hour the child was born (but dead), without any hemorrhage at all. The uterus contracted well, and the placenta came away naturally. Warmth and stimulants were employed; but the pulse began to flag still more, and the powers gradually failed, till, about half an hour after, she died, rather suddenly. It seems to me that if the version had been performed as soon as the os was capable of admitting a finger, the draining of blood might possibly have been prevented. The quantity lost at first was not precisely known till after death, and, coupled with the muscular power and uterine pains, there did not seem any justification for retarding delivery, as has been recommended in extreme exhaustion.—*Lancet, July* 14 *and* 21, 1860, *pp.* 28, 55.

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## 97.—ON THE OPERATION OF TURNING IN LABOUR OBSTRUCTED THROUGH CONTRACTION OF THE PELVIC BRIM.

By Dr. Robert Barnes, F.R.C.P., Physician to the Royal Maternity Charity, &c.

On the 2nd of February, 1858, my assistance was required at a protracted labour by a midwife of the Royal Maternity Charity. The patient, a pluripara, had always had lingering labours, and the last child was still-born. She was now in labour at term. She had been seen by a surgeon at ten p.m. the day before. The head was at that time presenting at the brim, and it was reported that she had been in labour some hours without progress. I saw her at three a.m. on the 2nd. The pains had then remitted; the patient was weak and restless, her pulse small. The head was in the first position. The sacral promontory projected so as to be easily reached by the finger. The antero-posterior diameter was estimated at 3.50". The cranial bones overlapped considerably at the sagittal and lambdoidal sutures, the skull being somewhat flattened between promontory and pubes. But still the full transverse diameter of the head had not entered the strait, nor could it do so unless under the influence of considerable additional force. A fair degree of uterine contraction now returning, I waited an hour; when, finding that the force required could not be supplied by the patient, that there was no advance, that symptoms of exhaustion were appearing, and being satisfied that the forceps would not deliver a living child, I lessened the head and delivered by crotchet. The extraction was easily accomplished; for the moment the capacity of the head was diminished, strong expulsive action set in. The placenta was removed in five minutes, and the uterus contracted well. The patient recovered favourably.

*Second Labour.*—The woman had disregarded my injunction to have labour brought on prematurely in the event of another pregnancy, so that on Feb. 15th, 1859, she being in labour at term, my assistance was again required. When I saw her, the os was fully expanded, head and foot presenting. As the patient's strength was still good, I determined to deliver by turning. The version was effected under chloroform without difficulty, the presenting *leg being* brought down. The head was jammed for twenty minutes in the *brim between the* promontory of the sacrum and the symphysis: it was *dragged through* under considerable force, emerging into the cavity with a

sudden slip. The child was born asphyxiated, but in half an hour cried loudly. It was not of a large size; the head was much compressed at the sides. Mother and child did well.

*Commentary.*—This case gave me both satisfaction and surprise. I little expected that a living child would be born after the head had been subjected to such great and protracted compression. Had not the head been of somewhat less than average dimensions, the result would not, I believe, have been so favourable. Still in this case, all the conditions of safety to the mother and child could be secured by bringing on labour at the end of the eighth month, and resorting to version should the head not freely enter the pelvic brim. Here were a conjugate diameter of 3·50", and a head below the average dimensions. In most instances, the natural forces, if of ordinary vigour, might overcome a disproportion of this kind. In this particular case the natural forces were insufficient, and it became necessary to choose between the forceps, turning, and craniotomy. I postponed craniotomy as the last resource. It is possible that the forceps might have been successful; but cases which I shall hereafter relate will show that, except under special circumstances, this instrument will not effect delivery obstructed through disproportion, with less risk to the mother or with more safety to the child, than can be attained by turning. The last operation could place the child's head in the most favourable position for entering the contracted brim. The hold obtained upon the legs and trunk would give the obstetrician a degree of power which neither the natural forces, nor the forceps, nor both combined, could supply. The power so obtained, when exerted, would equalize the transverse diameter of the child's head and the conjugate diameter of the pelvis most directly, and with the least possible violence. Brought into the brim base foremost, the plastic skull most easily adapts itself to the rigid distorted contour. The compressing force, first bearing upon the skull in its extreme transverse diameter, soon lessens this, because above the promontory and on either side there is no obstacle to the expansion of the skull. On the other hand, the forceps cannot exert the requisite degree of compressing force under equally favourable conditions. The compression cannot be applied directly to the transverse diameter of the skull; it must fall either obliquely or in the occipito-frontal diameter. In either case, the compression, by lessening the occipito-frontal diameter, must increase the bulging of the bi-parietal diameter, and bring this—the obstructing part—into more violent impaction between the promontory and symphysis. The instrument thus adds to the very difficulty which it is sought to overcome, and calls for a supererogatory increment of force to balance that excess of resistance which it has itself created.

Turning has this great advantage, that it brings no artificial difficulty to enhance that difficulty which already exists.

The case before us does not mark the limit of the applicability of turning in contracted pelvis. It is true that with a conjugate diameter of 3·50" and a small head, the passage through the pelvis was not effected without some difficulty, and some risk to the child's life. But experience has proved that a living child of full size may be extracted through a yet smaller pelvis with safety to the mother. We must, therefore, pursue the inquiry, and study fresh cases, before we can determine the limits of the conservative application of turning in contracted pelvis.—*Lancet, April* 21, 1860, *p.* 390.

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98.—*Pregnancy with Duplex Uterus.* [In a work lately published by Professor KUSSMAUL, of Heidelberg, a most elaborate array of cases is given, illustrative of pregnancy in the perfectly developed duplex uterus. Amongst others occurs the following anecdote.]

"One of our most celebrated accoucheurs met with a remarkable case in a woman who had a duplex uterus and a double vagina. She asked him whether she might become married; and he gave her the advice, that if she did so, she *should only make use* of one of her vaginæ. In course of time

she was brought to bed of a child. *Sed quid fit?* After three months she again fell in labour, showing clearly that she had twice conceived, at different times." "Truly a most remarkable case!" adds old Baldinger. Lobstein states, that he "delivered a woman of two infants, one a month after the other; and was able to convince himself that she had two uteri, and to each a distinct vagina."

In cases of parturition with such duplex uteri, labour may occasionally be quite natural; but generally it is the contrary. The malformation of the womb and of the vagina, as well as the abnormal shape, size, and axis of the uterus, may be assigned as causes which render parturition difficult and perilous. Many of the women who have borne children under such circumstances have died during the puerperal stage.—*Edin. Med. Journal, July* 1860, *p.* 61.

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## 99.—PUERPERAL CONVULSIONS SUCESSFULLY TREATED BY SUBCUTANEOUS INJECTIONS OF MORPHIA.

By Professor Scanzoni, of Wurtzburg.

Since the attention of the medical profession was first directed by Dr. Wood, of Edinburgh, and more lately by Hunter and Béhier, to the advantageous effects of subcutaneous injection, especially of narcotics, Professor Scanzoni has employed this method with success in numerous cases of neuralgia, hyperæsthesia, &c., but he attaches especial importance to the following case of puerperal convulsions, because it seems to prove, in accordance with the views laid down by Hunter, that the subcutaneous application of narcotic agents furnishes a means of acting on abnormal irritations of the brain with greater rapidity and certainty than the administration of the same remedies by the mouth. It will, doubtless, be admitted that opium, and its different preparations, deserve the first place in the treatment of puerperal eclampsia. In his own experience, the observation of a large number of cases has convinced Professor Scanzoni that a kind of intoxication produced by opium leads with more certainty to a favourable termination than any other means recommended in this terrible disease. But, unfortunately, it is not always possible to administer a sufficient quantity of opium or morphia; sometimes the comatose condition of the patient, at other times the rapid succession of paroxysms, prevents administration by the mouth; and opiate enemata are occasionally rejected as soon as they are received. The subcutaneous injection, however, supplies the means by which these difficulties may be overcome, and a sufficient quantity of opium introduced into the system to render its effects certain. Numerous experiments have convinced the author that, although the effect of this method is not always persistent (the neuralgiæ, for example, are not always cured by it), yet there are constantly produced,—within a very short time, often a few minutes, after the injection,—certain phenomena, which can leave no doubt as to the action of the opium upon the brain. Such symptoms are drowsiness, giddiness, headache, sickness, feeling of constriction in the throat, even vomiting, and depression; or, if the dose is large, somnolence. These facts, taken along with the known effects of the subcutaneous application in delirium tremens, mania, chorea, tetanus, &c., induced him to try the same treatment in puerperal convulsions, and with the most satisfactory results. After three injections of meconate of morphia there occurred only two attacks in nine hours, while previously there had been three attacks in an hour and three-quarters. This diminution of the convulsions after the injections is so much the more remarkable, since experience has shown that, as a general rule, the paroxysms become not only more violent, but follow at shorter intervals as the labour advances. And although the author does not imagine that he has discovered in the subcutaneous injection an infallible panacea for this dreadful malady, he is of opinion that the following case should induce physicians *to give this* means a trial:—

*Case.*—D., aged 21, primipara, strong and robust, was brought into the

lying-in ward at a quarter to eight o'clock on the morning of June 8th, 1859. Labour had commenced in the night, and she had been seized with nervous paroxysms and loss of consciousness; no account was given of the nature of the attacks; the patient remembered nothing of what had occurred during the night. The whole body, and especially the lower extremities, were œdematous; on the right side the tongue showed marks of being bitten by the teeth; the uterus corresponded to the pit of the stomach, and seemed sufficiently consistent; sounds of the fœtal heart distinct. On examination, the os uteri was dilated to the size of a sixpence, the bag of waters was partly formed, and the head presented: the urine was very albuminous, and exhibited under the microscope numerous fibrinous cylinders. At eight o'clock she was seized with a second convulsive attack, which was of a very marked character, and lasted for some minutes. On recovering consciousness, she could answer questions, although slowly. A third attack succeeded at a quarter to nine, a fourth at a quarter to ten, a fifth a quarter to twelve, and a sixth at five o'clock,—the last the most violent. After the fourth paroxysm consciousness did not return, and the breathing became stertorous. At ten o'clock she was bled to about eight ounces, an enema with twenty-five drops of laudanum was given, the body was put into a warm bath, while cold irrigation was applied to the head. As opium could not be administered internally, a solution of the meconate of morphia was now, at three different times injected under the skin, the quantity amounting in all to about 10 grains (75 centigrammes) of opium. The labour advanced very slowly. At three o'clock next morning the membranes burst; the os dilated to the size of a half-crown; the head still high up above the brim; sounds of the heart very distinct. After this period the dilatation went on more quickly; at seven o'clock the os was larger than a crown piece, very extensible and dilatable, the head high up and immovable; complete loss of consciousness, profound coma. In these circumstances, which left little hope of saving the patient, and in spite of the high position of the head and the incomplete dilatation of the os uteri, it was decided to employ the forceps. Their application was by no means easy, but the extraction presented no difficulty. After a few tractions, a fœtus was born, which breathed feebly at first, but soon began to moan vigorously; the placenta followed. During the operation there was no paroxysm. Some wine and ten drops of tincture of amber and musc were now given to the patient, which revived her a little, but did not restore consciousness. At eleven o'clock, a seventh attack came on, but was slight and short; after which she became excited, and tried to escape, but towards morning she grew calm. At nine in the morning she could answer questions put with a loud voice. During the whole day she remained like a drunk person; pulse 128. The musc was stopped; nothing but lemonade given. Towards evening the abdomen was somewhat painful. During the night there were several slight attacks of mania; she constantly attempted to escape. In the morning she answered rationally; pulse 108. The œdema had diminished, the abdomen was still tender; there was difficulty of breathing; and numerous rales, fine and coarse, in the lungs. Warm bath, lemonade, expectorants, were prescribed. In the evening the patient was completely herself again; pulse 132. June 11th and 12th: She slept well during the night, the expectoration becoming easy, and the pain of the abdomen relieved by fomentations and poultices: pulse 120; the urine contained little albumen, and no fibrinous cylinders. June 13th: Good condition; œdema gone, abdomen soft; some incontinence of urine during the night was relieved by leaving in a catheter. All medicines were now suspended; the patient was put on good diet; and ordered to take every morning a glass of chalybeate mineral water. On the 17th there was no albumen found in the urine; and on the 21st the patient left the hospital with her child, being advised to continue the use of steel for a considerable time.—*Bull Gén. de. Thérap.—Edin. Med. Journal, May* 1860, *p.* 1045.

100.—*Post-partum Hemorrhage.* By P. HUBBERT, Esq., Croydon.—Much has been written on kneading, plugging, emetics, ice, &c., none of which I think can be fully relied upon, but I have always succeeded by the simple plan of injecting about half a pint of very cold water (iced if you can get it) into the rectum, giving at the same time cold brandy-and-water, with a drachm of Curtis's liq. secalis cornuti in it. There is less difficulty in this operation than in injecting the uterus, which I imagine, when attempted, is not always done. The application of the infant to the breast frequently fails to produce the desired contraction. I invariably carry in my midwifery case a small elastic enema instrument.—*Lancet, Sept.* 8, 1860, *p.* 250.

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## 101.—ON THE DIAGNOSIS OF DROPSY OF THE FALLOPIAN TUBE.

By Dr. J. Y. SIMPSON, F.R.S.E., Prof. of Medicine and Midwifery in the University of Edinburgh.

[The characters by which it can be determined whether a cystic tumour, lying in the pelvis, is due to the dilatation of a Fallopian tube, are shortly these:]

First. *Its free and independent Mobility.*—The tumour is freely moveable in the cavity of the pelvis, and does not move synchronously with the uterus; but, on the contrary, it remains at rest when the womb is moved, and can be readily moved about by the exploring finger when the womb is kept firmly fixed, either by the hand placed above the pubes, or better still, by means of the uterine sound introduced into its cavity.

Secondly. *Its elongated Form.*—As the fluid collects in and distends the tube in nearly all its length, the resulting tumour is of an elongated conical form, with the rounded base corresponding to the obliterated fimbriated extremity and the apex at the upper angle of the uterus.

Thirdly. *Its wavy Outline.*—In all cases the surface of the tumour feels more or less undulating and indented, for, in most instances, the distended tube is folded once or oftener on itself, and tortuous; and when it is not thus wavy and convoluted, the smooth outline is interrupted by the pressure of bands of plastic fibrine stretching over and indenting it.

I have also referred to the comparative size of the tumour and its situation in the ordinary position of the Fallopian tube as guides—though certainly of less value—in enabling you to discover and recognise the disease. If you have satisfied yourselves in any case as to the existence of the characteristics which I have attempted to pourtray, you may proceed at once to confirm your diagnosis of the disease, and initiate your treatment by introducing an exploring needle into the interior of the cyst, and examining the fluid that escapes, which, as I have stated, is usually perfectly limpid and clear. It is, no doubt, a matter of considerable difficulty to make out clearly in certain cases the actual presence of a tumour with all the characters I have described; and to decide positively that it is a dropsy of the Fallopian tube will demand a certain amount of experience on your part, and no small care and caution in conducting the examination. But the difficulties of the case will be lessened, and the diagnosis will be rendered more certain, if you will always take the precaution to investigate the pelvic organs by means of both hands simultaneously—one being placed over the fundus uteri, and pressing down upon the pelvic organs through the abdominal walls, while two fingers of the other hand are passed into the two canals at the outlet of the pelvis, for the purpose of examining the state of its contents from below. Where the patient has previously had a family, the abdominal walls are soft and flabby, so that the pelvic organs can be readily felt through them; and where they are firmer and less yielding, by putting the patient under the influence of chloroform they will be so far relaxed as to enable you to have all the parts thoroughly at command.

*Prognosis.*—In the great majority of cases the disease has remained unrecognized and unsuspected during the lifetime of the patient, and has only

been discovered after death when the body was subjected to a post-mortem examination; and this is more particularly the case where the cyst is not of very large size, and its contents consist simply of a serous fluid. But where some morbid process has been going on in the interior of the cyst, and the altered mucous membrane has become the seat of inflammation in its higher grades, or where it has been subjected to injury, so that the contents become mixed up with purulent effusions and bloody extravasations, the ultimate issue is apt to be less favourable. For in such cases it sometimes happens that the cyst-wall becomes ulcerated through or bursts, and the morbid and irritating fluid escaping into the cavity of the peritoneum lights up a degree of inflammation on its serous surface that proves rapidly fatal. In some few cases, again, the disease may be recognised during life, even when the cyst is of the simplest kind, but where it is of sufficient size to give rise to the series of symptoms to which I have alluded. In such cases you may succeed in diagnosing the disease in the manner I have described to you, and in conducting it to a favourable termination by an appropriate plan of

*Treatment.*—Medical treatment of any kind seems to be of but little avail for the cure of this kind of malady. The only effectual means of affording relief to the patient, and her only hope of a permanent cure, is the removal of the fluid by puncturing the cyst.—*Med. Times and Gazette, July* 21, 1860, *p.* 49.

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## 102.—CASE OF SPONTANEOUS EXPULSION OF A FIBROUS TUMOUR OF THE UTERUS.

By Dr. Joseph Griffiths Swayne, Physician-Accoucheur to the Bristol General Hospital.

The case I am about to relate is a good instance of the manner in which the *vis medicatrix naturæ* may sometimes, under favourable circumstances, effect the removal of a tumour which is quite beyond the reach of a surgical operation.

A lady, aged 37, previously in good health, married on August 14th, 1856, and on July 22nd, 1857, miscarried, when about eight weeks pregnant. At that time she was staying in Edinburgh, and was under the care of Dr. Duncan. About six weeks after the miscarriage, she experienced considerable pain in the left iliac region, accompanied with rigors and feverishness; and, on placing her hand upon the part, thought that she could detect a slight swelling. About ten days after that, she was attacked in the night with a great deal of hemorrhage from the vagina, and afterwards with an increase of pain in the part affected. The pain was more severe at some times than at others, and never entirely left her. About six weeks after the hemorrhage, she again went to Edinburgh, and consulted Dr. Duncan, who examined her, and detected an enlargement on the left side of the uterus. Soon afterwards she left Edinburgh and went to Kissingen. She now never entirely lost the pain, and always found it increase at the menstrual periods. At this time she could herself feel the tumour, especially in the morning. She returned to England in July 1858. She was then in tolerable health in other respects, but yet was never entirely without pain, and had a return of hemorrhage at each menstrual period. In October 1858, she came to Clifton, and soon after consulted Dr. W. Budd, who then examined her, and distinctly felt the uterine tumour. He prescribed medicines for her which had the effect of restraining the hemorrhage at each period, until, on February 24th, 1859, she menstruated for the last time, and then became pregnant.

She continued pretty well during her pregnancy, with the exception of a good deal of sickness, and occasional pains in the left side, especially in walking. She was not able to distinguish the tumour after the second month of pregnancy. On November 26th, 1859, I was sent for to attend her in her confinement. The first stage of the labour went on favourably, although rather slowly, and occupied altogether about forty-eight hours. The first part *of the second stage* was tolerably expeditious; but towards the end

of this stage, the progress of the head became less rapid, although the pains still continued powerful. At last the head became completely arrested at the lower outlet, and remained in that position for four hours. As the pains began to diminish very much in force, I applied the short forceps, and delivered her of a fine male infant. The placenta was expelled without any hemorrhage. For the first week after delivery, both mother and child went on well. When I called on the eighth day (Sunday December 3d), I found my patient feverish, and complaining of headache, which she attributed to taking rather too much animal food on the previous day. I prescribed an aperient and a less stimulating diet, and called to see her on the following Tuesday. She was still rather feverish; the pulse was 104; and she complained of dull aching pain, with slight tenderness on the left side of the abdomen, just above the ilium. I ordered her eight grains of Dover's powder, and three grains of hydrargyrum cum cretâ, to be taken at bedtime; together with poppy-head fomentations to the abdomen. On the following Thursday, the symptoms were much the same; and she was directed to repeat the powder, and apply bran poultices. As the pain and tenderness still continued, I agreed to meet Dr. Budd in consultation on Tuesday, December 12th.

We found her in much the same condition as regards the general symptoms. On examining the abdomen, we felt that the fundus uteri reached nearly as high as the umbilicus; and the uterus itself was as large and hard as an uterus just after delivery, before the expulsion of the placenta. The enlargement was more perceptible on the left side of the uterus, which was also somewhat tender on pressure. We were both of opinion that some unfavourable change, such as subacute inflammation and softening, was going on in and around the uterine tumour, which had been ascertained to exist before the occurrence of pregnancy. She was ordered to repeat the Dover's powder and fomentations, and also to use an opiate suppository. On the next day, the febrile symptoms and tenderness were somewhat increased. When I called on Friday, Dec. 15th, the nurse showed me two pieces of a fleshy substance, which had passed *per vaginam* since my last visit; the largest of these pieces was of the size of a hen's egg, and appeared to be made up of concentric layers of fibres. On examining it microscopically, I found that it consisted of unstriped fibres resembling those of the uterus. The expulsion of the substance was attended with a sanguineous discharge, like the lochia in appearance, and at first not at all offensive. This continued for several days, during which time a great number of smaller pieces were expelled, sometimes with distinct pains; the discharge and febrile symptoms still continuing. To promote their expulsion, ergot was given on each day, together with opiate enemata and suppositories, to relieve pain and tenderness. On Monday, December 18th, we found that the portions expelled were assuming a darker colour and a putrid appearance; and the discharge was becoming of a reddish-brown hue and very offensive. This was accompanied with a short hacking cough and hectic fever, and the pulse still kept up to 100 or more; although in other respects the general condition of the patient was not unfavourable. She was put upon a generous diet, and ordered to take quinine; and the nurse was directed to syringe out the vagina frequently with chlorinated lotions. During this fortnight, the offensive discharge still remained, and broken down shreds and portions of tumour were repeatedly coming away. The uterus, which at first could be felt nearly as high as the umbilicus, gradually diminished in bulk and subsided in the abdomen, until, on Monday, December 25th, we could scarcely feel the fundus above the pubes. The whole of the tumour appeared by this time to have been expelled. On collecting all the portions and putting them together, they were found to form a mass about the size of a man's fist, and to weigh rather more than half a pound. The treatment adopted on December 18th, was continued up to this time. The discharge now began to improve very sensibly, both in quality and quantity, and the hectic fever to diminish; while at the same time our patient was steadily gaining strength.

On Tuesday, January 10th, 1860, there was a very slight pale mucous discharge, which was not at all offensive. The uterus was now so much reduced in bulk that it could not be felt. There was no fever; the pulse was 84; and the cough had almost entirely ceased. From that period she progressed rapidly, and in another fortnight had completely recovered.

*Remarks.*—In this case, before the occurrence of pregnancy, the presence of a fibrous tumour occasioned considerable hemorrhage at each menstrual period, and also a certain amount of pain, symptoms which are commonly noticed in connection with these morbid growths. But after the patient became pregnant, it is surprising how little the process of gestation was interfered with. There was a marked improvement of health, and no symptoms of the presence of a tumour was observed, except occasional pain in the left side after walking. During the progress of parturition the tumour gave rise to no untoward complication, except, perhaps, that it had the effect of retarding the second stage of labour; but of this I am by no means certain. After the birth of the child, a fibrous tumour is very likely to occasion *post partum* hemorrhage, from its interfering with the due contraction of the uterine fibres; but in this case nothing of the kind took place. But it is during the changes which take place in the uterus after delivery, that we have most reason to dread the results of a fibrous tumour. As Dr. West remarks, "it may interfere with the process of involution of the womb after delivery, and may either itself undergo a morbid softening and disintegration, or may be the occasion, either in connection with inflammation of its own substance and of that of the womb, or independently of them, of peritonitis, always dangerous, too often fatal." In the case I have just described, the termination was much more fortunate. The tumour no doubt was one of those which are imbedded in the walls, and between the fibres, of the uterus, with which they have a very slight connection. In all probability, it was much nearer the mucous than the serous surface of the womb, and thus it was that during the degeneration and absorption of the uterine fibres after delivery, it lost its connections with the uterus, and consequently its vitality, and this gave rise to inflammation and ulceration of the uterine fibres between itself and the mucous surface, until a sufficient opening having been made, it was cast off piecemeal into the cavity of the uterus; and thus the natural processes in this instance, instead of being destructive, as they usually are, were curative, and were attended with the happiest results.—*Brit. Med. Journal, May* 12, 1860, *p.* 358.

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## 103.—ON THE DIAGNOSIS OF OVARIAN DROPSY.

By M. Boinet.

An elaborate paper on this subject, from the pen of M. Boinet, has been published in the 'Gazette Hebdomadaire de Medicine et de Chirurgie' for January 6th, February 3rd and 17th, and March 9th of the present year. As the essay represents the opinions of a practitioner who is well known to have had large experience in the management of the disease in question, we subjoin a copious extract; abridging little but the histories of some of the cases which he relates in support of his views.

The affections which are most commonly mistaken for ovarian cysts, are —ascites, pregnancy, extra-uterine, pregnancy, tympanitis, encysted dropsy of the peritoneum, fibrous and cancerous tumours, masses of indurated fæces, abscesses in the iliac fossæ, retro-uterine hæmatocele, hydatid cysts, &c.; and yet all these have only the appearance of ovarian cysts, and may be recognized by particular signs. But it is not enough merely to diagnose an ovarian cyst from any of the other abdominal tumours; it remains to be ascertained whether it is unilocular or multilocular, simple or compound; whether it is complicated with ascites, pregnancy, fibrous tumours, &c,; whether the contained fluid is serous and clear, or coloured, thick, thready, gelatinous, sero-purulent, &c.; whether the cysts are moveable or adherent. In its bearing *on prognosis and* treatment, the recognition of an ovarian tu-

mour in any of the varied states in which it may present itself, is of the highest importance.

The characteristic signs of ovarian cysts differ, according to the nature of the cyst and its contents, and according to the presence or absence of complications. One of the first symptoms is a dull pain, a feeling of weight, a dragging, in one of the iliac regions. This feeling often arises from turgescence of the ovary, and is especially felt before or during the catamenial periods: at other times the pain is only excited by pressure or by certain movements. These early symptoms, it is true, have no great diagnostic value at first; but at a later period, when the abdomen has become enlarged, and it has become impossible to discover by palpation on which side the cyst has arisen, then these symptoms become important, as indicating its point of origin.

Afterwards when the disease has made some progress, there appears in one of the iliac regions a more or less painless tumour, readily felt by the hand, lobulated or round, and offering an equal elastic resistance. The tumour often passes the pubes and reaches the umbilicus before the patient has suspected its development; and then the medical man, called for the first time, ascertains with difficulty on which side it has commenced. At the end of a certain time the abdomen acquires an uniform development and the size of an advanced pregnancy. If the patient is young and in the way of becoming pregnant, she is likely to deceive herself willingly; and it is only when the duration of the enlargement has exceeded the natural limits of gestation, when the abdominal tension increases, when a sensation of pain is felt, and when functional disturbance of the abdominal and thoracic viscera appear, that she recognises that she has a dropsy in spite of her generally good health.

There are special signs which characterise ovarian cysts at different periods of development. When the surgeon is called in at the commencement of the disease, particular attention should be given to the form of the abdomen. It will be often recognised, on careful examination, that it projects more on one side than on the other. If it appears uniform, palpation will in certain cases discover the presence of a tumour, the limits and origin of which may be ascertained. In young girls and in women who have never borne children, it is sometimes difficult to determine on which side the tumour has commenced; since, while it has been growing, the mass of the intestines has been pushed over to the other side, so that the abdomen is equally distended: in such cases, careful percussion over every part of the abdomen discovers a tumour on one side and the intestines on the other. And, when the abdominal walls possess a certain degree of laxity as in women who have borne children, and the abdomen is not very voluminous, the tumour will change its position with the side on which the patient lies, and, in doing so, often causes a kind of undulation to be felt.

An ovarian tumour having been ascertained to exist, is it liquid or solid? Circumscribed pressure on the tumour, carried along the iliac fossa, will cause a more or less evident fluctuation to be recognised, and will cause an elastic depression to be perceived, which prevents the disease from being confounded with a fibrous tumour, impacted fæces, or any other solid swelling. Percussion gives a a dull sound along the whole extent of the tumour; and fluctuation is often felt at the same time. Examination by the vagina or rectum proves nothing, so long as the tumour remains in the ilio-lumbar region, and is not yet developed in the pelvis; but when it has acquired a certain development, vaginal examination may be useful in ascertaining the side on which the tumour has originated, if this can be ascertained in no other way; for the tumour, in growing, carries the uterus with it, and gives this organ an oblique direction, so that the neck is turned to the opposite side from the fundus. This sign, important as it is, should not be had recourse to unless it is otherwise impossible to discover from which side the tumour has grown, and unless such discovery is of absolute importance.

*At a still more* advanced period, when the abdomen is uniformly enlarged, *pressure and* percussion must afford the diagnosis. By pressure are ascer-

tained fluctuation, inequalities, and points of hardness. If the abdominal walls are not too tense or œdematous, it is possible, by pressing in the abdomen at every point, to feel masses varying in extent, mobility, shape, and size, formed of more or less resisting tissue, sometimes hard, sometimes soft and elastic. These masses, which resemble spherical bodies somewhat flattened, are sometimes so large, so diversely placed, that they simulate displacement of the liver, spleen, or kidney. Sometimes the sense of fluctuation is only partial and indistinct, and the different parts of the tumour offer an unequal resistance. In these cases, there is either a multilocular cyst, or a cyst or tumour complicated with ascites. In other cases, fluctuation is general, and more evident than even in the highest degree of ascites.

Percussion is the principal and most certain means of recognising (which may be done in by far the greater number of cases) whether a cyst is unilocular or multilocular, multiple or complicated; whether the liquid it contains is serous, thick, thready, or gelatinous; whether the tumour has several lobes, and what is their size, and to what extent they correspond with the abdominal walls. The progress of the disease, the more or less severe pains which the patient has experienced, the more or less evident signs of inflammation which have appeared on the abdomen, the state of the general health, will also aid in pointing out the nature of the fluid, whether purulent or seropurulent, &c.; and palpation will discover the presence of tumours, of greater or less hardness, which may complicate the cysts.

After these general considerations on the diagnosis of ovarian dropsy, the special characters of each variety of cyst remain to be described.

Simple cysts, when they have occupied the whole abdomen, and the distension is uniform, present the following symptoms:—

The abdomen is projecting, so that one would say that the patient had arrived at the last stage of pregnancy. If, on percussing gently with one or several fingers, or, still better, on giving a slight fillip on any one point of the abdomen, the impulse of the fluid is plainly felt by the other hand, the cyst is unilocular if, in addition, percussion gives a dull sound in all the anterior and inferior parts of the cyst, and resonance only at the epigastrium and in the flanks on lower and lateral parts of the abdomen, whatever be the position of the patient. This occurs because the intestines, pushed aside by the tumour, project only beyond the edges of the cyst. Whether the patient is caused to lie either on the right or on the left side, the dulness is never displaced, but is always found at the same points and in the most elevated part of the cyst. These signs are constant, and enable an unilocular ovarian cyst to be distinguished from ascites, for instance. Another circumstance that may aid in the diagnosis is the generally good state of the health, and the rarity of swelling or œdema of the lower limbs.

In one case only of unilocular cyst has M. Boinet observed these diagnostic signs to fail. The case was one, which he observed with M. Putel, of a woman aged 60, in whom percussion denoted the presence of a serous fluid, and fluctuation was so distinct that the case might have been supposed to be one of ascites, if other signs had not denoted the presence of ovarian dropsy. A trocar was introduced; but no fluid escaped. On introducing a thread of iron wire through the canula, a thick gelatinous mass was drawn out. Nothing in this case led to a suspicion of the nature of the tumour, for percussion constantly gave the same results; the only circumstance which could have, under other conditions, thrown any doubt on the diagnosis, was that the patient was much emaciated, had a dry yellowish skin, and showed traces of the cancerous cachexia; all the functions, besides, were tolerably well performed, except that the breathing was impeded. Examples of this variety of cyst are very rare.

When the abdomen has acquired a large size, it is sometimes difficult to ascertain on which side the cyst has arisen. In this case, recourse must be had to the history of the previous symptoms, and, as a last resource, to a vaginal examination.

Although the diagnosis of *unilocular* ovarian cysts appears easy, errors of diagnosis *are often committed.* A patient came to M. Boinet, telling him

that she had an ovarian tumour, of the nature of which she had already been informed by two hospital physicians. M. Boinet, relying on their diagnosis, drew off a large quantity of serous fluid, and proceeded to inject iodine, according to the plan which he strongly advocates; but the patient at once uttered so loud a cry, that it was evident that the injection had been made into the peritoneal cavity. Purulent peritonitis set in; the pus was evacuated, and iodine was again injected several times; the patient recovered, and was in good health in 1859, six years after the operation. M. Boinet observes, that the severe pain at the moment of injection is a certain sign that the peritoneum has been injected instead of an ovarian cyst.

M. Boinet is aware of numerous cases of error of diagnosis between ascites and ovarian dropsy. For instance, a young girl, on whom he operated with MM. Danyau and Fontés, had already been treated for ascites in the Hôtel Dieu. Another case occurred in the person of a Norman lady, who had been tapped four or five times for ascites. M. Boinet threw in iodine injections, with the result of obtaining a radical cure.

Proceeding to speak of the diagnosis of the forms of ovarian cyst, M. Boinet relates the case of a lady aged twenty-seven, in whom M. Gendrin diagnosed the presence of an agglomeration of cysts, and advised merely palliative treatment; M. Boinet believed that there was an unilocular cyst of the left ovary, containing a thickish fluid, slightly thready, and probably of a chocolate colour; M. Velpeau diagnosed a cyst with thick walls, probably single; and M. Trousseau was of opinion that it was a simple unilocular cyst. It was finally determined to inject iodine, which was done by MM. Boinet and Bougoin. The cyst was unilocular, with very thin walls; it contained five *litres* of slightly thready chocolate-coloured fluid. The injection was repeated three times; and the patient recovered.

When the impulse of the fluid is not felt at all the points of a cyst, wherever percussion may be made, it may be said with certainty that the cyst is multilocular, or that the fluid which it contains is very thick. In these cases, the fluctuation is limited to the points in which the surface of each lobe corresponds with the abdominal walls. By a careful examination, it may thus be possible to count the lobes of which the cyst is composed—at least, the superficial ones; there is almost always a principal lobe, a mother-cyst, round which all the others are grouped. Occasionally the cysts are divided into several pouches by more or less apparent projections or depressions, which may sometimes be felt through the abdominal walls. The presence of tumours in the walls of the cyst, excessive thickness of the walls, division into several cells, and especially density of the contained fluid, all obscure undulation. Extreme distension of the cysts with a thick liquid gives a remarkable sensation of vibration. In these cases, percussion is not enough; careful palpation in all directions must be employed. In cases of multilocular cysts, the general health is ordinarily not very good.

It remains to determine the nature of the fluid contained in the cyst, and its density. When it is serous and limpid, so as to be capable of flowing readily through the canula like water, fluctuation is free, easy, perceived at all points, resembling that of a considerable ascites. If the liquid is dense, thick, thready, glutinous, oily, fluctuation, although perceptible, is much less free. In cases where the liquid is too thick to give rise to fluctuation in the ordinary way, it may be felt by applying the two hands on the abdomen, and applying a to-and-fro motion, which will enable the fact to be discovered, that the liquid possesses a certain consistence.

If the patients experience or have experienced continued pains in the abdomen, if signs of inflammation have been present, if the abdomen has been or is slightly tender on pressure, the fluid, whether the cyst be single or multilocular, will probably be more or less coloured—reddish, sero-sanguinolent, coffee or chocolate coloured, &c. Multilocular cysts especially where the fluid is thick, glutinous and unhealthy, present these varieties of *colour.* It may with equal probability be announced that the fluid is purulent when the patients have a continued slow fever, loss of appetite, vomiting, diarrhœa, emaciation, pains in the abdomen and especially in the cyst

—in a word, the symptoms of hectic fever, or those which indicate the presence of purulent deposits.

To avoid confounding ascites with encysted dropsy—the mistake which is most commonly made—it is sufficient to remember that the course of ascites is generally more rapid, that the enlargement of the abdomen is more uniform; and that, in ovarian dropsy, the abdomen is more protuberant, and preserves the same rotundity on whatever side the patient may lie. But percussion affords the surest means of diagnosis: in ovarian dropsy, it gives dulness in all the anterior, superior, and inferior parts of the abdomen, and resonance in the flanks; while, if the abdomen is resonant above and dull at the sides, there is ascites; the intestine floating to the surface of the fluid, while in ovarian dropsy they are pushed aside by the cyst. In ascites, if the patient is caused to lie on either side, the fluid changes its position, and resonance can always be detected on the side opposite to that on which the patient lies; in ovarian dropsy, on whatever side the patient may lie, the dulness is always at the same point. Another sign which is rare in ovarian cysts, and almost constant in ascites, especially when the disease is of a certain duration, is the infiltration of the lower limbs, and the pressure of organic disease of the liver heart, and other viscera.

In encysted dropsy of the peritoneum, fluctuation is much more difficult to perceive. It is circumscribed and limited, and is never found except at the same points. The intestines lie below, and do not, as in ascites, occupy the most prominent part of the abdomen. In this form of disease (which M. Boinet has seen twice in males and once in a female), the liquid is either collected in a single cyst extending, it may be, from the xiphoid cartilage to the pubes, and from one hypochondrium to the other, or in several separate cysts placed very superficially. The phenomena of distension and fluctuation are manifested here as in ascites; but encysted dropsy always arises from peritonitis, is slow in its progress, forms a more projecting swelling than ascites, and produces less impediment in respiration and displacement of the intestines.—*British Med. Journal, April* 14, 1860, *p.* 228.

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## 104.—FOUR CASES OF OVARIOTOMY.

By T. Spencer Wells, Esq., F. R. C. S., Surgeon to the Samaritan Hospital.

In the last number of the 'American Journal of the Medical Sciences' (July, 1860), Dr. Sawyer, of San Francisco, says that of seven cases of abdominal section made in California, on account of ovarian disease, six terminated fatally; and he asks if surgeons "always furnish a report of their unsuccessful cases as well as of their successful?" He concludes his paper with the remark, that he is "satisfied that the statistics of ovariotomy are entirely unreliable, because but a feeble fraction of the fatal cases are given to the public; while there is not a single successful operation that does not find its way, either directly or indirectly, into some of the medical periodicals of the day." Somewhat similar opinions have been hinted, if not openly expressed, on this side the Atlantic. I am anxious, therefore, to assure the reader that he may find in the carefully-elaborated table appended by Mr. Clay to his translation of 'Kiwisch on Diseases of the Ovaries,' particulars of *all* the cases, successful or fatal, in which I had performed ovariotomy up to the date of the publication of that work. I have performed it twice since, with a favourable result in both cases; making a total of 19 operations, followed by death in 7 cases, and recovery in 12. I feel confident that this mortality may be much diminished hereafter; but, as it stands, it is far better than the result of lithotomy in the London Hospitals, which ranges from 50 to 53 per cent. in patients over the age of 14. As the details of the following four cases have not been published, they may possibly be acceptable to the readers of the 'Medical Times and Gazette.'

*Case* 1.—On October 18, 1859, I met Dr. Rigby and Mr. Pierce, of Notting-hill, in *consultation upon a single* lady, only 17 years of age, who had

been tapped eight times during the previous two years. She had been previously under the care of a physician who considered the disease to be ascites, but Mr. Pierce had recognized its true nature. It was arranged that this gentleman should tap in our presence, which was done, and a careful examination was made after the largest cyst had been emptied. It was then found that there were several groups of smaller cysts in different parts of the abdomen, and looking to the frequency with which tapping had become necessary, and the condition of the patient, who was much emaciated, it was agreed to recommend ovariotomy. The patient and her friends being anxious to have it done, after the danger had been fairly put before them, it was arranged after a short time that I should perform the operation. Some delay was caused by a continuance of very foggy weather, but it was performed on November 19.

Dr. Priestley having administered chloroform, and Messrs. Curling and Peirce, and Dr. Sanderson, being present, and kindly assisting me, I removed the cyst and its contents, by an incision only four inches long, opening the cyst to the same extent, and breaking down the inner cysts, withdrawing the whole as they were emptied. Moderately extensive parietal adhesions had been previously broken down; and a portion of omentum was separated, in which were two vessels which required the ligature. The peduncle was secured by strong twine, and, with the omental ligatures, was kept outside the wound, which was united by hare-lip pins carried through the peritoneum on both sides, and by superficial sutures. The cyst and contents weighed thirty-eight pounds.

The after-treatment of this case was conducted with great ability and care by Mr. Peirce; and it is only necessary to add that rapid recovery followed, without one alarming symptom. An occasional opiate was given by the rectum, and hot linseed poultices were used for a few days all over the abdomen. The young lady is now in vigorous health, and was seen lately by Dr. Rigby, who was much struck by the contrast between the blooming girl before him, and the pale emaciated being who was the subject of our consultation only a few months before.

*Case* 2.—On Jan. 31st, 1860, I was consulted by a married lady from Manchester, aged twenty-six, the subject of ovarian disease of about three years' standing. Dr. Whitehead had tapped her in March and August, 1859, removing at the first time twenty-five, and at the second fifteen, pounds of fluid. A number of small cysts had grown rapidly between the first and second tappings. When I saw her she measured forty-one inches in circumference at the umbilicus, and nineteen inches from ensiform cartilage to symphysis pubis. She had been in pretty good health until the last six weeks, but had suffered much since from the distention, pain, and want of sleep. A portion of the tumour could be felt in the pelvis, between the rectum and the uterus, pushing the uterus upwards and forwards; but as the uterus was moveable, I hoped there were no pelvic adhesions of consequence, and, seeing no other resource open to the patient, advised ovariotomy.

I performed the operation on Feb. 6. Dr. Priestley administered chloroform. Dr. Markham, Mr. W. Adams, and Mr. Price were present. The operation was precisely similar to that performed in the former case, the cyst having been emptied and drawn through a small incision. A group of smaller cysts moulded to the pelvic cavity were then also withdrawn through the wound, but the uterus followed the cysts closely, the tumour and uterus being connected together without any intervening peduncle. The Fallopian tube was much enlarged, elongated, and expanded over the growth, and adhering to it. I commenced the separation by the knife, dividing the Fallopian tube and part of the broad ligament, and secured some large vessels by ligature. The attachment lower down, towards the cervix and side of the vagina, was looser. Here I transfixed it, and tied in three portions, after *securing* it temporarily by a clamp. I found that the tissues included *in the ligature* could not be brought outside the wound without exerting a *dangerous* degree of traction on the uterus. Accordingly, after the wound

had been united by hare-lip pins, I was obliged to be satisfied with bringing the ligature as near the peritoneal edges of the wound as I could, and leave sufficient opening for the escape of the decomposing stump.

She recovered well from the operation, and had some quiet sleep in the evening; the pulse, however, was 135, and rather feeble. Flatulence was rather troublesome, but there was no sickness. She seemed inclined to perspire, and said she was "comfortably warm." She passed a good night, frequently dozing, and perspiring a little; but in the morning the pulse was up to 148. She had no pain, but flatulence was troublesome, and she said she felt "tired." During the forenoon the pulse became feeble, rose to 160, and she continued to sink all day, although she took stimulants and beef-tea freely, and was not sick. No urine was secreted after three p.m. The intestines became enormously distended by gas, and she died at nine p.m., thirty hours after operation. No post-mortem examination was allowed.

I am disposed to attribute the death in this case partly to imperfect recovery from the shock of the operation and the consequent exhaustion, and partly to the absorption of some morbid product of the decomposing cyst. When a peduncle is secured outside the wound on the surface of the abdomen, the portion of cyst or peduncle strangulated by the ligature becomes quite putrid in a very few hours, and a black offensive discharge is generally very copious. The same thing must occur when the stump is within the peritoneal cavity, and the effects might be expected to resemble those produced by the injection of putrid substances into the veins. It is known that Dr. Clay thinks it better to leave the stump within the abdomen, and acts up to his belief; but, with two exceptions, I have always kept it outside,—this case, and one in which the patient recovered.

*Case* 3.—On May 26th I was requested to meet Dr. Ramskill and Dr. Buss, of Shoreditch, to decide upon the treatment of a patient, aged forty-one, the subject of a large multilocular ovarian cyst of two years' growth. She had not been tapped, and it was agreed that Dr. Buss should tap her in our presence. Very little fluid was obtained at first, but by passing a uterine sound through the canula, and breaking down the walls of many inner cysts, a pailful of thick fluid was evacuated. She recovered well from the tapping, but filled again rapidly, and it was decided at a second consultation with the same gentlemen held on July 13th to perform ovariotomy, as she was already as large as before the tapping.

On July 17th I performed the operation. Dr. Echeverria administered chloroform. Dr. Ramskill, Dr. Routh, Dr. Drage, of Hatfield, Mr. Clay, of Birmingham, and Mr. Thompson, of Westerham, were present. The parietal adhesions were very extensive, requiring separation with the hand from almost the whole of the abdominal walls. A portion of omentum, also, had to be separated. A very little fluid passed through a very large trocar, so that I opened the cyst to the full extent of the wound, introduced my hand, and broke down the inner cysts while Dr. Routh and Mr. Clay withdrew them as they were being emptied. In this way the whole tumour was removed through an incision only just large enough to admit my hand, and little, if any, of the contents of the cyst entered the peritoneal cavity. There was some oozing of blood from the torn adhesions, and it was necessary to sponge out the pelvic cavity, but no ligatures were used. The pedicle was secured by twine ligature in three portions, and kept outside the wound, which was closed in the usual way by hare-lip pins and superficial sutures. The empty cysts weighed four pounds, the fluid collected thirteen pounds, and some eight or ten pints were lost.

The after-treatment was ably conducted by Dr. Ramskill and Dr. Buss. No medicine was given by the mouth. The abdomen was covered by hot linseed poultices, and when there was pain small opiate enemas were administered; but she only required three of these. Indeed, recovery was uninterrupted. As Dr. Ramskill said, "She had not even a dry tongue, a quickened pulse, any pain, or any heat of skin." The bowels did not act till the ninth day. On the *next day* the ligature separated from the peduncle, and on *Aug. 11th, twenty-four* days after the operation, the patient left

town for Brighton by cab and railway, having been walking about the room for nearly a week before.

*Case* 4.—On Dec. 8th, 1859, I saw a single lady, aged thirty-six, in consultation with Dr. Rigby, and removed ten quarts of fluid from an ovarian cyst. She had not been tapped before. The growth was of about three years' standing. She suffered nothing from the tapping, gained flesh afterwards, but began to fill soon.

I saw her again on May 9, 1860. She then appeared about as large as before the first tapping. The girth was 39½ inches. It was arranged that I should tap again, and inject iodine if no secondary cysts were found. Accordingly, on May 21, I removed nine quarts of fluid; but, finding that large groups of secondary cysts had formed on the left side since the first tapping, no iodine was injected. She remained rather feeble, with occasional nausea and faintness, for some days, but left town ten days after the tapping. She did not recover strength, complained of much headache at times, and lost flesh, while the size of the abdomen increased rapidly.

On July 13, consultations were held with Dr. Rigby, Dr. Tyler Smith, and Dr. Growse, of Brentwood. The result was that she determined to submit to ovariotomy, and I performed the operation at Brentwood, on July 28. The growth had increased so rapidly, she had become so thin and pale, had been so much distressed by the abdominal distension, and the pulse was so feeble, that at the last moment I consulted with Dr. Ramskill and Dr. Growse, as to the propriety of operating; but, fortified by their opinion that it was the only means of escape from speedy death, we determined to proceed, Dr. Ramskill carefully watching the action of the chloroform, Dr. Growse and Mr. Quennell of Brentwood, and Mr. Mason, assisting me. With the exception of one small band of adhesion at the site of the previous tappings, the cyst was quite unattached. This band readily yielded to the hand. The principal cyst was tapped, emptied, and partly withdrawn, But large groups of inner cysts prevented the escape of the whole tumour through the small incision. I therefore passed my hand into the large cyst, and by breaking down the smaller ones and squeezing out their contents, the whole tumour easily passed through an incision only just large enough to admit my hand. Dr. Growse and Mr. Quennell were so careful in drawing out the cyst as it was emptied, and protecting the edges of the wound by hot flannels, that not a drop of the contents of the cyst entered the peritoneal cavity. There was rather free bleeding from the broken-down adhesion, but it subsided spontaneously. The peduncle was long, but very broad, extending along the brim of the pelvis on the left side. It was secured by transfixing and by a twine ligature. The wound was closed in the usual manner, and the same plan of after treatment agreed upon. It was conducted most ably and attentively by Dr. Growse, and I have little more to record than gradual recovery. For three days she remained very weak, the pulse ranging from 104 to 130, the skin inclined to be hot, and there was a good deal of flatulence with occasional vomiting. But there was very little pain. She slept very comfortably after an occasional opiate enema, and all anxiety was over by the fourth day, when the ligature on the peduncle had separated, and the deep sutures were removed. The superficial sutures were left till August 4. I have since had most favourable accounts of her progress, and look upon convalescence as established. Her sister wrote to me on August 21, twenty-four days after the operation, saying:—"My sister walked into the front room yesterday, where we dined and spent the day. She is now sitting in an easy-chair, *at work*."

I am anxious to direct special attention to the simple after-treatment pursued in these cases,—as I am convinced that in many successful cases the fatal result has been principally due to over-active or meddlesome medication. The published reports of more than one case clearly point rather to *opium than to* ovariotomy as the cause of death. In other cases stimulants *seem to have been* given in mischievous excess; while the opposite treat*ment by starvation* and purgatives is not yet absolutely exploded. I have

made a great number of experiments on dogs, rabbits, and guinea-pigs, in order to determine the best mode of uniting wounds of the abdominal parietes; and nearly the whole of the animals operated on recovered,—yet the rabbit is peculiarly liable to peritonitis and the guinea-pig is in about the same degree as in the human subject. But although no after-treatment whatever was adopted, and the animals were left with their ordinary supply of food, and to their natural instincts, they recovered with hardly an exception. This experience has led me to regard perfect rest in a well-ventilated room, comfortably warm bedding, and extreme cleanliness, while simple food and drink are given in small quantities, as the patient's feelings suggest, as the principal things to be attended to. The application of warmth and moisture to the abdomen is very pleasant to all such patients, and undoubtedly useful. Opium in sufficient doses to relieve pain is also of great use; but in larger quantities it is unnecessary and injurious. If given by the mouth in any form it is apt to be followed by vomiting, but this evil is in a great degree avoided by injecting it into the rectum. Here also it seems to have a local soothing effect; especially in relieving irritability of the bladder, which is sometimes troublesome. Purgatives on the one hand, or artificial constipation by opium on the other, seem to be equally mischievous. Over-excited peristaltic action may produce peritonitis directly, while the opposite condition may do so indirectly, and certainly leads to distressing flatulent distension of the intestines. It seems to be a perfectly safe rule to allow both stomach and intestines to adapt themselves to their altered circumstances with very little, if any, medical interference, and certainly rather to do nothing than to run any risk of doing harm.—*Medical Times and Gazette, Aug.* 25, 1860, *p.* 178.

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## 105.—CASE OF TUMOUR IN THE ABDOMEN WITH A HISTORY OF SUPPOSED PREGNANCY.

(Under the care of Dr. JAMES JONES, Metropolitan Free Hospital.)

[The following case is one of great interest. It bears more especially on the question of diagnosis, and indicates the necessity for extreme care in arriving at a conclusion as to the existence of a dead fœtus in the abdomen.]

Mrs. W., a fairly healthy woman, aged twenty-nine, was admitted under Dr. James Jones' care in June last. She was the mother of one child now nearly three years old. The history which she gave of herself was, that after the recommencement of menstruation on the weaning of her infant, she continued to be unwell regularly, until Jan. 1859. At the latter date the menses were suspended, and she believed herself pregnant. In due time she felt the motions of the child, her abdomen enlarged, her breasts increased in volume, and her whole condition was such that she never entertained a single doubt as to her condition. In Oct. she had attained a size about equal to that of the latter months of her previous pregnancy. A week or ten days after the time at which she had expected the birth of her child she was attacked by pain in the abdomen, and indications of the commencement of labour. The midwife whom she had engaged to attend her was summoned, and remained in attendance for three or four days, when, as things did not progress, a consulting accoucheur was called in, who, with some hesitation, expressed his belief that the patient was not pregnant, and that an abdominal tumour existed. After this the woman got about again, and in November she received a blow on the abdomen subsequent to which she never again felt what she had considered to be the movements of the fœtus.

*Condition at the time of Admission* (eighteen months after the suspension of menstruation, and eight after the supposed death of the fœtus).—The woman's abdomen was about as large as that common in the seventh month of pregnancy. It was occupied by a large firm mass, which was centrally placed, and could be moved a little from side to side. In this mass the most careful handling failed to discover any irregularities like those presented by the limbs of a child. Over its right side at its upper part there was a broad

shallow furrow, but with this exception its surface was equally rounded and equally firm and unyielding. The tumour did not deviate to either side, and occupied the whole of the front of the abdomen from the pubes to midway between the umbilicus and ensiform cartilage. Her breasts were both of them of considerable size, and she could squeeze milk from the nipples. The areolæ were dark. A broad brown streak extended from the umbilicus to the pubes. On examination per vaginam the os uteri was found to be high up, the cervix was short and appeared to end by being expanded into a rounded solid mass which occupied the upper part of the pelvis. With some difficulty a flexible uterine sound was introduced for the length of four inches.

Here, then, was obviously the question of diagnosis between a fibrous tumour of the uterus and a defunct fœtus, the result of an extra-uterine or parietal gestation. The woman's history, and the fact that she had had no hemorrhages, were strongly in support of the latter hypothesis, and several of those who examined her were inclined to adopt it. Against such a view of the case, however, was the whole of the evidence derived from the examination of the tumour, whether through the abdominal wall or by the vagina. It should be stated that the woman was thin, and that the tumour could consequently be very satisfactorily handled. Then, again, as to history. The suspension of menstruation was the only fact to which much real weight could be attached. Cases of fancied pregnancy, on the part even of those who have previously borne children, and might be expected to have had experience enough to prevent such a mistake, are yet common enough. It is well known, also, that the breasts will enlarge in sympathy with any uterine tumour, and even that milk may be secreted under such circumstances. Although also it is perfectly true, that the growth of most intra-uterine tumours, more especially in their earlier stages, is attended by attacks of hemorrhage, yet exceptions to this are far from infrequent. Thus, therefore, the history, though at first sight appearing clear and definite in its indications, seemed to lose its conclusiveness on more searching scrutiny. Relying upon the more trustworthy evidence afforded by actual examination, Dr. Jones and most of his colleagues were induced to form a very positive opinion that the tumour was in reality a fibrous one, and had nothing to do with misplaced gestation.—*Med. Times and Gazette, July* 21, 1860, *p.* 58.

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## 106.—ON THE DIAGNOSIS OF TUMOURS OF THE BREAST.

By John Erichsen, Esq., Professor of Surgery and of Clinical Surgery in University College, &c.

[In the present article, Mr. Erichsen treats principally of the diagnosis of cancer of the breast from cystic and adenoid tumours of that organ.]

These cystic growths, though not so common as cancer and the other solid tumours, are yet of by no means infrequent occurrence. They are of three distinct kinds:—1. Those in which the tumour consists of a single unilocular cyst; 2. Those which consist of several independent cysts aggregated together into one tumour—multilocular cysts; 3. Those in which a series of small cysts are diffused through a fibrous or hypertrophied mass; in fact, a combination of cysts with a chronic mammary tumour. Two theories are in vogue as to the origin of this form of cystic development. According to one set of pathologists, it is produced by the obstruction and subsequent dilatation of a lacteal duct. But this theory, I think, is weak: from the fact of our not being able to trace one of these ducts into the cyst; from the fact that the fluid contained in these cysts shows no trace of lacteal origin; and from the fact that such cysts are met with elsewhere, in places where no lacteal duct previously existed. The other theory is, that these cysts are entirely new formations; and this, partly for the reasons before mentioned, and partly because these cysts closely resemble those met with in other secretory glands both as to structure and contents, appears to *me to be the more* tenable of the two.

*These tumours*, I must premise, whether unilocular, multilocular, or con-

sisting of cysts diffused in a mass of fibrous or hypertrophied gland-tissue, are always composed of thin walls, formed of condensed cellular tissue, and containing in their interior a fluid variable in amount and character; being in one serous, in another glairy, and in a third bloody; it is very commonly of a light straw colour, not unfrequently it is brown, and sometimes sanguineous, but these differences are accidental and of no importance.

We will now consider the diagnosis of these cystic growths from cancerous and other solid tumours of the breast. And, first, with regard to the unilocular cyst: this is the most common form, and occurs generally in women at what one might call the "cancerous age," viz., forty-five to fifty; it is frequently referred to pressure, or to a blow on the part, or it may be connected with uterine disturbance at the period of the cessation of the menses. Thus so far as the age and proximate cause are concerned, the history throws comparatively little light on the subject, and the diagnosis has therefore to be made entirely by palpation and examination of the tumour. Now you can easily conceive that, depending only on manipulation to form a correct judgment of the nature of the tumour, the surgeon may be exceedingly liable to form an erroneous opinion. I could relate to you many cases in which error of diagnosis has occurred, but I will confine myself to a few of the more illustrative.

I was requested some time ago, by my friend, Mr. Walter Wilson, to see a married lady, aged 45, who had in the right breast a tumour of about the size of an apple, hard and painful on pressure. This, she said, had been diagnosed as a fibrous tumour by a surgeon in the country, who had recommended her to come to London and have it removed. On examining the tumour I suspected it to be a cyst. It had not the stony hardness of a solid tumour, but felt obscurely elastic on deep pressure. Acting on this supposition, I introduced an exploring trocar and let out about two ounces of dark serous fluid; the cyst never refilled, and the patient went home perfectly cured.

I was requested one day to see an unmarried lady, aged 44. On going to the house I found her bathed in tears, and her sisters in great distress around her. I was told she had a tumour of the breast, which had been pronounced to be a cancer. On examination I found a tumour in the right mamma, about as large as a pigeon's egg. It had been noticed about fourteen months previously, and the patient had been under both medical and surgical treatment for it. It was rounded, circumscribed, situated at the outer and upper part of the gland, hard but smooth, and not heavy to the feel. On close manipulation, I felt some elasticity. I told the patient that I did not think it was a cancer, but a cyst, and that if I punctured it and drew off the fluid contained within it she would probably have no further trouble. She then informed me that she had seen an eminent surgeon, who had pronounced it to be cancer; had explained that an operation was necessary; that the whole of the breast must be removed, and had fixed an early day for its performance. I requested to meet this gentleman, but this was refused by the patient and her friends; and as our opinions differed so widely, it was agreed that she should have the benefit of Sir B. Brodie's opinion. That distinguished surgeon saw the case with me the next day, and, he having acquiesced in the opinion I had expressed, I tapped the cyst, and let out about an ounce and a half of yellowish serous fluid. The tumour collapsed, all idea of operation was abandoned, and the patient has continued well up to the present time.

I saw the other day a very similar case. It was that of an unmarried lady, aged 48, who had had for about twelve months a tumour in the left breast, which had gradually increased in size, until it had attained the magnitude of a Tangerine orange. It was hard, circumscribed, situated at the axillary border of the gland; it had also been pronounced to be scirrhus. But, suspecting from its elasticity that it was cystic, I punctured it with an exploring trocar, and drew off about ten drachms of clear fluid, causing the immediate subsidence of the tumour.

Now here *were three cases of simple* cystic or fluid tumour of the breast,

which had erroneously been pronounced to be solid, and which would have been submitted to amputation of the mamma if the mistake had not been discovered in time. It is of very great consequence, therefore, to be cautious in these cases in pronouncing a definite opinion, and to neglect no means in perfecting your diagnosis. How is this to be done? As I have already stated, the history very often throws no light on the nature of these cases. Cysts occur at the same age and among the same class of people as cancer of the breast, but there is one circumstance of great importance to which you must always attend, and that is the presence or absence of *elasticity*. This last character may be said to be the great diagnostic point between these tumours and cancer; and whenever you feel, or even suspect, elasticity, you ought to introduce an exploring trocar. If the tumour is cystic, the fluid escapes, and you probably hear no more of the case. But if, on the contrary, it is solid, a drop or two of blood only oozes out, the puncture soon closes, and no harm is done. In making this puncture, there is one little point to be attended to, and that is, if you use an exploring needle, take care to push it straight in, and not to make the puncture in any way valvular; for if you do, the fluid may not escape, and thus a serious mistake may arise; it is, however, far better, in all cases to use the exploring trocar in preference to the grooved needle.

If this little operation—tapping—does not procure the closure of the cyst, you must resort to other measures, such as injecting it with iodine, introducing a seton, or, if these fail, excision of a piece of the cyst-wall. But you will generally find that tapping and the subsequent use of iodine lotions will suffice to effect a cure.

The next kind of cyst—the *multilocular*—is more difficult to diagnose than the unilocular; firstly, because it is generally seated deeply in or beneath the gland, while the unilocular occurs chiefly at the border or anterior surface, and because there is not so much fluctuation, owing to the fluid being divided among several cysts. But yet there is that never-failing sign of cystic disease—elasticity. You will find, however, that there often exists a good deal of condensed fibrous matter round about these tumours, and hence the removal of the whole gland may be required, the extirpation of the cysts alone being impossible. One reason why cyst-tumours are often so difficult to diagnose as such, is that they are often associated with cancer. There great difficulty exists, especially in the early stage; and these cases are exceedingly liable to be confounded with cystic sarcoma, and indeed in some cases you cannot make your diagnosis until the tumour is removed, and then only by a careful microscopic examination.

A woman, aged 45, was admitted into this hospital with a tumour of the size of an orange, situated in the right mamma deeply over the pectoral muscle. It had existed for about five years, was not adherent to the skin, and there were several cysts of the size of plums. At the upper part of the mass, which could be felt through the lately mucous covering, the nipple was not retracted; there was only one slightly enlarged gland in the axilla. Now here were all the characters of a "cystic sarcoma"—slow growth, no adhesions, cysts, and no material glandular implication; and yet, after the removal of the breast, the microscope revealed the tumour to be distinctly and decidedly cancerous.

You will sometimes find that tumours of a mixed, possibly a semi-malignant character, occur with cysts, and give rise to great difficulty in the diagnosis of their exact nature, and render it impossible for you to pronounce with certainty whether they are benign or malignant. The following is a case of this kind.

An unmarried lady, 40 years of age, and in excellent health, was sent up to me by my friend Mr. Tuxford, of Boston, last November, with a tumour of the right breast, which, without assignable cause, had commenced growing about five years and a half ago; it increased slowly until it had attained the size of an orange about two years since, but afterwards much more rapidly, until at last it reached the size of an adult's head. The skin covering it was not reddened, and, though thinned, was neither adherent nor infil-

trated. There was no pain at night, or after handling it; and no enlarged glands could be felt in its neighborhood. The superficial veins were much enlarged over the tumour, and a good sized artery was felt to pulsate over its upper part. It was elastic, lobulated; and a large mass, of a cystic character, projected from its anterior surface. There were no adhesions between it and the pectorals, or to the skin. The operation was performed on November 10th. On removal, I found that the tumour weighed ninety-six ounces, and consisted of large encapsuled masses, of a dull grey or brown colour. One section, it was solid in parts, and infiltrated with a gelatinous fluid; in others, there were large cysts, containing several ounces each of dark stringy mucoid fluid. Dr. Harley, who examined it, pronounced it to be colloid. There were also masses of fibroplastic deposit in some parts.

Now here was a case that approached closely to malignancy in its local characteristics, without any constitutional symptoms of cachexy; and which presents an appearance on examination that renders it doubtful whether it may or may not yet recur.

The next class of cases to which I would direct your attention are the chronic mammary or *adenoid* tumours. These are of exceedingly common occurrence; and there are two or three conditions with which they are often associated, the knowledge of which materially assists the surgeon in his diagnosis. 1. With regard to the age, they generally occur before the cancerous age, in early womanhood, between the ages of 18 and 25. 2. They are almost invariably slow in their progress. 3. They are lobulated, distinct, non-adherent to the skin or pectorals, and circumscribed. 4. There is no cachexy or glandular enlargement; and, in fact, they appear to be quite local and benign. There is usually no cause to which they can be assigned; but I believe that they frequently occur in young women of nervous temperament, and are commonly associated with uterine disturbance of some kind. The chief diagnostic points are, therefore, the age of the patient, the slowness of growth of the tumour, and the perfect freedom from constitutional symptoms; but yet any one of these conditions may be present in cases of undoubted cancer of the mamma.

Scirrhus very rarely occurs at the early period at which the adenoid tumour is common; but yet it is occasionally met with in young women. Some years ago, I removed in this hospital a tumour from the right breast of an unmarried female aged 23. It was as large as a small flattened orange, was hard, nodulated, but not adherent to the skin. There was no retraction of the nipple, and it had been growing for about eight months. No cause could be assigned for its appearance, and it was supposed to be adenoid. However, on account of the large size of the tumour, as compared with that of the somewhat atrophied mamma, I removed the whole of the breast, together with the tumour; and it was well that I did so; for, on microscopic examination, it was found to be scirrhus. The patient subsequently married; and when I last heard of her, two years after the operation, she was in good health. Now, here is a case in which cancer occurred within the period usually assigned to chronic mammary tumour; and hence you cannot rely altogether on the age of the patient as a means of diagnosis.

Next, with regard to the slowness of growth: although it is an undeniable fact that benign tumours usually grow slowly, and that rapidity of growth is generally a sign of malignancy of action, yet this rule must also be taken with exceptions, as in the following case:—

Last June, I was requested to see an unmarried lady, aged 40, of a nervous sanguineous temperament, who had been in bad health for many years, suffering from uterine disturbance, dyspepsia, and latterly from pulmonary symptoms. At the age of 18—that is, twenty-two years previous to my advice being sought—she noticed for the first time a small tumour in her left breast; this continued perfectly stationery, and about the size of a pigeon's egg, until last February, when it for the first time became painful, and began rapidly *to enlarge*. *Being* at this time in Italy for the benefit of her

health, this lady saw two distinguished Roman surgeons, who, after careful examination and exploratory punctures, pronounced the tumour to be "myeloid," and recommended extirpation of the mamma. The patient, however, preferred to return to England, and have the operation performed here. On her way home she saw Velpeau in Paris, who, with great accuracy of diagnosis, pronounced the tumour to be benign, and gave her a written statement to that effect. When I saw her, on her return to this country, in June last, I found a tumour of the left mamma, perfectly mobile, firm, solid, inelastic: it had attained the size of an adult's head, and was rapidly increasing. There was no glandular enlargement in the axilla; the skin was thinned and reopened, but not adhered; the superficial vessels were much enlarged, a tortuous network of veins and one or two large arteries running over the tumour. There was no constitutional cachexy; but the patient's health and strength were at the lowest ebb, from general and long-standing constitutional derangement; and she could not sleep at night, not so much from pain (which, however, was constant, and at times very severe), as from the constant anxiety of mind which the presence of the growth produced. Sir B. Brodie and Dr. Walshe, who saw the case in consultation with me, both agreed that it was probably benign, but that no operation was practicable until the patient's state of health was improved. However, by attention to diet, and by being put on a proper plan of treatment, this was so much ameliorated that, in July last, I was enabled, with the assistance of Mr. Marshall and Dr. Cowan of Glasgow, to perform the operation for its removal. This was attended by no difficulty, and by very little hemorrhage, notwithstanding the size of the tumour, which weighed rather more than five pounds. The patient made an excellent recovery, and was able to leave town in less than three weeks. On examination after removal, the tumour was found to be lobulated on its surface. The section showed it to be homogeneous, and of a uniform greyish colour, with no soft points or cysts, but distinctly and firmly encephalous. Dr. Harley, who examined it microscopically, found it to be a specimen of the chronic mammary tumour of Sir Astley Cooper. In the plastic matter taken from different lobules, examples of the glandular tissue were found. Some of the blind tubes were remarkably distinct, and well filled with cells. There was no trace of cancer.

Now here was a tumour which, after remaining of small size and stationary for more than twenty years, suddenly, and without obvious cause, began rapidly to increase; so much so that, in less than six months, it had increased in size from that of a pigeon's egg to the magnitude of a mass weighing more than four pounds. Here was extreme rapidity of growth without malignancy of character. In fact, this extreme rapidity of growth resembled rather what is not unfrequently found in encephaloid disease, than what we expect to meet with in the chronic mammary tumour, and rendered the diagnosis not a little difficult; the more so, as it was possible that the chronic mammary tumour, which had existed for so many years in a stationary and passive condition, might have suddenly undergone malignant degeneration, and thus taken on rapid increase of bulk. This, however, was disproved by the careful microscopical examination made by Dr. Harley, who found that the tumour did not present a trace of malignant structure.

It is, however, important to bear in mind that cystic and adenoid tumours may remain for a long time in a benign and passive state, and then assume malignant character. This happened in the case of a woman aged 48, who was sent to the hospital by my friend Mr. Adams two years ago. At the age of 27, she had first observed a tumour in the left breast. This had slowly increased in size, until it had attained, at the end of fifteen or sixteen years, the size of the fœtal head. When I first saw it, in January 1858, one of the cysts of which it was composed had ulcerated, and a thin sero-sanguineous discharge oozed out of the opening. The general health was good. There was no glandular enlargement in the axilla, or adhesions of the skin, except around the ulcerated parts. It was freely moveable on the pectorals. An operation for the removal of the tumour was proposed, but this the patient refused to consent to. At the end of six months, she returned with a large

ulcerated cavity in the centre of the tumour, and fungating masses sprouting from the bottom of it. There was still no cachexia, or enlarged glands in the axilla. The patient now consenting to an operation, I extirpated the whole mass with the mamma. On examining the tumour after its removal, it was found to be cystic. There were several large cysts, of the size of pigeons' eggs, containing turbid but light coloured serous fluid. The central portion of the tumour, and that at the base of the fungus were solid, grey, and rapidly undergoing softening and disintegration. On squeezing the portion of the mass (the base of the fungus), a milky juice exuded; and Dr. Harley who examined the tumour, stated it to be encephaloid. The sarcomatous structure beyond this, constituting the general mass of the tumour, appeared to be adenoid. The surface of the fungus, when exposed and protruding beyond the cyst, was epitheliomatous.

Now here were cystic sarcoma, encephaloid, and epithelioma, associated in one growth. The encephaloid was stated by Dr. Harley to be cellular, without fibres, showing rapid development. The epithelioma was confined to the surface of the fungus. The history of this case, the very lengthened period (more than twenty years) that the tumour had existed, the absence of all constitutional cachexy, of deep adhesions, or of glandular enlargement or other secondary deposits, and its appearance only six months previous to removal, all pointed to primary simple cystic disease of the mamma, in which, as the result of secondary changes, encephaloid had developed itself; being an instance of the conversion of a simple into a cancerous tumour of the breast. This patient died about a twelvemonth after the operation, of gangrene of the foot and disease of the heart. The cicatrix was quite sound; but in the substance of one of the ventricles a nodule was found, which was considered by those who examined it to be of a cancerous nature.—*British Med. Journal, April* 14, 1860, *p.* 279.

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## 107.—RECURRENCE OF AN ADENOCELE OR MAMMARY GLANDULAR TUMOUR AFTER THREE SUCCESSIVE REMOVALS; ULTIMATE CURE.

(Under the care of JOHN BIRKETT, Esq., Surgeon to Guy's Hospital.)

A thorough acquaintance with the various tumours involving the mammary gland is essential to the forming of a correct opinion as to their malignancy and liability to recurrence. When growths appear at an early age—say in women under thirty—the probabilities are in favour of their benignancy, and that they pertain to the class of chronic mammary, mammary glandular, sero-cystic, or adenoid tumours, as they have been described by Sir Astley Cooper, Sir Benjamin Brodie, Mr. Birkett and other writers. A tumour met with in the breast of a young girl, for instance, is most commonly of this nature, although occasionally there are exceptions; and when once removed, they never return. Such examples are by no means rare; many have appeared in our "Mirror." We examined two on the 17th inst. at another hospital; one in a girl of fourteen, with a small adenocele of the right breast; another in a young woman aged twenty-seven. These patients had applied for advice under the impression that their tumours were cancerous; their true nature was, however, readily made out. In the latter part of 1859, we saw three examples in young patients, under Mr. Birkett's care at Guy's Hospital, all successfully operated upon. One of these was especially interesting, because both breasts contained a tumour; that in the lower part of the right had been growing for three or four years, and was always painful before the appearance of the catamenia; and some three or four months previous to her admission into the hospital, several small growths were developed in the left breast. The one occupying the right gland was alone removed on the present occasion. A similar form of tumour, developed during pregnancy, in a young married woman, we saw Mr. Birkett excise in October last; *it had prevented suckling*, and was deeply seated behind the nip-

ple. A good recovery ensued, without damage to the proper glandular structure.

That these simple or innocuous tumours, nevertheless, do occasionally recur, we have clear evidence to prove, and the following instance is an example. There is a point of some importance in the case, moreover, not to be overlooked—viz., that although the primary disease resembled the chronic mammary tumour, yet the age of the patient was greater than that at which the malady usually shows itself; and after three removals it recurred a fourth time, the extirpation of the entire gland having been followed by perfect success thus far. Curiously enough also, although the glandular elements were observable on each occasion, yet the tumours varied in their characters, being at one time mammary-glandular, and at another cysto-sarcomatous. In a former "Mirror," ('Lancet,' vol. ii., 1857, p. 114,) we have recorded an instance in which all the varieties of tumour belonging to the class Adenocele were developed in a single breast. Its removal, however, was followed by recurrence (idem, p. 548) within four months, in consequence of the nipple having been left behind.

The case of this patient, now fifty-three years old, is reported in Mr. Birkett's paper, on Adenocele, in 'Guy's Hospital Reports' for 1855, p. 161.

The facts are as follows:—The patient was forty-five years old when the first growth was discovered. This growth had existed eleven months when removed. One year and ten months elapsed before the discovery of the second tumour, which was excised three months subsequently. In the course of eight months a third growth made its appearance, and was removed four months afterwards. All the growths showed the elements and tissues of the mammary gland structure; but the first and last most closely resembled the "chronic mammary tumour" of Sir Astley Cooper, whilst the second belonged to the class "cysto-sarcoma" of other pathologists.

This case and another are related to prove the alternation which may occur between these new growths in the same individual at different times, as well as the probable identity in their physiological relations.

In November, 1856, the patient showed Mr. Birkett a fourth very small growth near the cicatrix, but on the axillary part of the breast, which had not been taken away. She had then been free from recurrence one year and eight months. During 1857–8, and the early part of 1859, scarcely any increase in the tumour took place; but in the spring and during the summer it rapidly enlarged, and at the time of removal measured about four inches in the largest diameter.

This fourth growth was excised August 23rd, 1859, together with every particle of the normal breast gland-tissue which could be found. It consisted of the same elements as those composing the former growths; but in this instance it showed, with the happiest effect, a combination within the same envelope of the "chronic mammary tumour" of Sir A. Cooper, and the "cysto-sarcoma:" for in some parts were firm, fibrous, solid gland-like masses; whilst in others, cysts, with the characteristic fluid and intra-cystic growths, met the eye. The patient suffered very little during the healing of the wound, which was nearly complete in about a fortnight, and the patient left the hospital quite well.—*Lancet, July* 28, 1860, *p.* 81.

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## 108.—SINGULAR CASE OF INVERSION OF THE URINARY BLADDER DURING PREGNANCY.

By Henry Johnston, Esq., L.R.C.S.I.

On the evening of Sunday, Aug. 8th, 1859, I was requested to visit Mrs. M., aged twenty years, and was informed that she was threatened with a miscarriage. I found her apparently suffering very intensely from "pains" attended with such tenesmic efforts as to induce her to remain almost constantly on the night-chair. I was told that there had been considerable hæmorrhage with discharge of "waters" the previous day. She had been

nine months married, and calculated that she was rather more than four months pregnant. For weeks past she had suffered from irritability of the bladder, but enjoyed very fair health until Friday, the 6th, when, after over-exerting herself in lifting a box, she felt great uneasiness in her back, attended with frequent and urgent desire to pass water. These symptoms became more urgent on Saturday, and were accompanied with severe attacks of vomiting. She was unable to retain the contents of the bladder; the tenesmus was most distressing; and she had no relief except when on the night-chair. It was thus I found her on my first visit; and after considerable persuasion I succeeded in getting her to retire to bed. On my attempting to make a vaginal examination, she became very impatient, and complained very much of the intense pain I gave her, far exceeding that commonly given in a vaginal examination. This did not escape my notice. On examining I found a soft fluctuating tumour, in size about that of a pear, pyriform in shape, extending as far down as the os externum of the vagina, and feeling very like a bag of membranes. My finger passed from the base of the tumour along its narrow neck-like portion, into a small narrow cavity, which I considered was the dilated os uteri, being also under the impression that the narrow neck-like portion of the tumour was the cord attached to the ovum. I followed up this cord into a cavity, which I presumed to be that of the uterus. My patient now insisted on being allowed again to remain on the night-chair, and complained of her inability to bear any further examination. Being under the impression that the uterus would expel its contents, and there being no hemorrhage, I gave some general directions and returned home.

I visited her on Monday morning, and found her in the same state of suffering; and having slept none during the night, her pulse was now excited, and the attacks of vomiting frequent. She was unwilling that I should make any examination. I therefore administered an anodyne, and returned in the evening with my friend Dr. Bryce, to whom I communicated my impression that my patient was going to miscarry. He examined the tumour; but in making his examination discovered that he could pass his finger up behind it, into the vaginal canal, where he felt the "os uteri" still undilated. He felt puzzled to explain matters, and at first suggested the idea that there was a malformation. On further consideration he concluded that the tumour was a vaginal vesicocele—a prolapsed bladder—and not a bag of membranes.

Dr. Bryce's discovery of the vaginal canal, and of the os uteri still undilated, was certainly new light to me. I again made an examination of the tumour; in doing so I found that it yielded to gentle pressure, continuing which, it retired upwards, through the cavity which I had supposed to be the os, into the cavity which I had supposed to be the uterus, but from which flowed a small quantity of urine. The true nature of the case was for the first time manifest, viz., that it was one of inversion of the bladder, that the os through which I passed the tumour was the "meatus urinarius," dilated to such a capacity as to admit two of my fingers. I suggested this explanation to Dr. Bryce, and, in order to satisfy ourselves on this point, we made an ocular inspection of the state of the meatus urinarius, which confirmed the diagnosis. Immediate and decided relief followed the reposition of the inverted bladder: the vomiting ceased; the pulse fell; our patient's expression changed; the pain in the back ceased; the urgent symptoms of irritation of the bladder were almost quite relieved; and she soon fell asleep. We visited her some time afterwards, and found that there had been no return of the inversion. I drew off a quantity of bloody urine with the catheter. We enjoined her to employ no voluntary efforts in micturating, for a time; administered an anodyne; and ordered an oil draught the following morning.

The next day I found her comparatively easy. During the next month, however, she suffered from incontinence of urine, even when sleeping; and complained very much of a scalding sensation in the urethra. The urine was bloody, *and there was a considerable* deposit of mucus. These symp-

toms were treated, and gradually disappeared. I saw her last on the 3rd of December. She was then quite free from any vesical uneasiness; was able to retain her urine as usual; felt in perfect health; and was soon expecting her confinement.

Believing that medical science may be advanced by the publication of our mistakes, as well as of our successes, in practice, I have given a fair and candid account of this peculiar, and I believe I may add uncommon, case. Prior to my meeting with it I had never heard of such an accident. I was, therefore, quite unprepared for it, and did not recognise it. It is well that it should be recorded, in order that it may enable others to exercise caution in dealing with tumours of an obscure character, and attended with perplexing and anomalous symptoms. We should ever be prepared to meet with cases presenting symptoms and features for which we may have no precedent. It may appear strange that I was so slow in recognising the true nature of the case, and that in the first instance I made a very decided, and what might have been a very serious, error in diagnosis (as, owing to the view I first took of this case, I contemplated puncturing the membranes.) I had been led to believe that Mrs. M. was about to miscarry. My mind was pre-occupied with that idea; and after I examined I felt so satisfied that the tumour was the ovum entering and protruding from the os that I did not see any necessity to push my external examination, and my patient was certainly very unwilling that I should do so. To Dr. Bryce belongs the credit of throwing that light on the case which led to the recognition of its true nature. I have been unable to discern any similar case on record, viz.—inversion of the bladder occurring during pregnancy.

Dr. Meigs, in his work on 'Diseases of Women,' details the history of a case of inversion of the bladder at the orifice of the urethra, in the person of a little girl three years of age. In her case, it was mistaken for a vascular tumour, and was about being ligatured when its true nature was fortunately discovered. Dr. Murphy had a similar case in Dublin, an account of which I have been unable to find. Jones and Sieveking, in their 'Pathological Anatomy,' mention the possibility of introversion of the bladder, and even, in females, of its projecting from the meatus urinarius externally. We find, therefore, that inversion of the bladder is an accident which we should be prepared to meet, and that as it occurs particularly in children, or in pregnant females, we should be careful to distinguish it in the former from a vascular tumour, and avoid ligaturing it; in the latter from a bag of membranes, and avoid puncturing it; as in either case fatal results would be almost certain to follow. I would only suggest whether we might not mistake inversion of bladder for polypus? Since this case was read, Professor Simpson has drawn my attention to two cases of inversion of the bladder, reported in 'Cruveilhier's Pathological Anatomy,' one occurring in the person of a male, the other in that of a female.—*Dublin Hospital Gazette, April* 16, 1860, *p.* 120.

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109.—*On the Treatment of Vaginitis and Superficial Inflammation of the Neck of the Uterus by Tannin Ointment.*—Dr. FOUCHER has observed that tannin combined with lard is an excellent local application for vaginal inflammations; and that ointments are preferable to injections, because they remain better in contact with the inflamed mucous membrane. In cases of simple vaginitis, he introduces every morning, by means of the speculum, into the vagina where it is in contact with the neck of the uterus, a large piece of wadding smeared with a thick layer of tannin ointment. A thread is attached to this wadding, so that the patient can remove it herself. Dr. Foucher has employed the same treatment for leucorrhœa, and he states that it has been successful, although in many cases constitutional remedies are necessary in addition to the local application.—*Brit. and For. Med. Chir. Review, July* 1860, *p.* 280.

## 110.—CLINICAL REPORTS OF SOME DISEASES OF THE RECTUM, &c.

By I. Baker Brown, Esq., F.R.C.S.

[The present paper gives some cases showing the possibility of affections of the rectum producing certain symptoms which have been ascribed to diseases of the uterus. In the author's work on 'Surgical Diseases of Women,' the opposite condition is shown, i. e., that the uterus may give rise to symptoms from the rectum.]

*Case* 1.—L. P., aged twenty-five, single, admitted into the London Surgical Home, Dec. 22nd, 1858. She complained that she had suffered for a long time from heat and pain in the womb, with pains in the back, and a general feeling of uneasiness in her bowels. She looked ill and worn, and was suffering much from dyspepsia. She had been treated by leeches to the uterus, for dyspepsia, and for the uneasiness in her bowels, but without deriving the least benefit. On examining the uterus, no disease could be found; menstruation was regular; there was slight leucorrhœa. On inquiry if she had more pain at the time of, or after, an action of the bowels, she replied, "Yes, always; that then she had a sharp shooting pain darting through the womb, and that she was obliged to lie down because the pain and uneasiness were so great in her bowels; in fact, that she dreaded going to the water-closet." On passing my finger into the rectum she complained of acute pain, and I found a deep fissure just within the sphincter, and opposite to it a small pendulous polypoid body, the pea-like end of which dropped into the fissure.

*Treatment.*—A dose of castor oil was ordered early in the morning of the following day, and after it had acted freely the rectum was well washed out with warm-water enemas; the fissure was then divided by Copeland's blunt-pointed straight bistoury, the polypus tied, and the rectum plugged with lint soaked in sweet oil. Two grains of opium were given, and generous diet ordered.

Dec. 25th. The bowels were opened by a castor oil enema, afterwards the nurse applied sweet oil on her finger to the whole cut surface, and repeated it once daily.

Jan. 6th, 1859. Discharged quite cured. I have heard of her since as continuing perfectly well.

*Practical remarks.*—This case well illustrates the proposition which I have just advanced. I would also wish to observe that a very large number of fissures of the rectum are produced by these little polypoid bodies, as they will be found in almost every case if carefully sought for. It will be observed that the dressing of lint and oil was never repeated. This has been my invariable practice for the last twenty-five years, having been taught the great practical fact by my esteemed friend, the late Dr, Copeland, that it is never necessary to interfere with the parts by the painful process of reintroducing the lint, if care be taken that the first dressing be left in for forty-eight hours; after that time there is no fear of union by first intentions, but, on the contrary, a healthy granulating process is set up, which continues till the end.

*Case* 2.—Emma C., aged forty-two, married, mother of eight children, admitted into the London Surgical Home on the 11th of June, 1859. She complained of having suffered for a long time from pain and bearing down of the womb, and also from piles since her first pregnancy, twenty-two years ago. Finding that there was no disease of the womb to account for her suffering, I inquired if she suffered much pain on defecation, and she replied, "Yes; that when her bowels acted she had great pain; and that as they were generally in a torpid state, she was constantly obliged to take aperients." On examination of the rectum, some old external piles were found, covered by skin, and within the sphincter some internal piles; at the root of one of them a deep fissure was felt, and opposite to it a large polypoid body.

June 16th. External piles cut off, internal piles tied, fissures divided, and polypoid body twisted off at its *root*. The after-treatment the same as before.

July 16th. Discharged cured.

*Case* 3.—E. F., aged thirty, married, mother of two children, admitted into the London Surgical Home on the 8th of December, 1859. She has been suffering for the last four years from pain in the womb, sensations of bearing down, pain in the back, general lassitude, and inability to perform her domestic duties. She had been in an hospital for a month, and was dismissed because she was supposed to have no disease—in fact, that she was shamming illness. On examination, the uterus was found healthy; but on examining the rectum, the same condition was found as in Case No. 1.

Assisted by my colleague, Dr. Hall Davis, I divided the fissure, &c. In a fortnight she was out walking, in a month she left the hospital, and I have since heard of her as being in perfect health, and performing actively her domestic duties.

*Case* 4.—M. R., aged thirty-five, married, was admitted into the London Surgical Home on March 14th last. For several years she had suffered intense pain about the uterus and rectum; and for the last three years has had caustic applied to the uterus twice a week, but without the slightest alleviation of her painful sufferings. She had been in the habit of taking three or four grains of opium a day, and her general health seemed completely broken down. On examination, an old fissure of the rectum was easily found, situated behind two large piles.

March 22d, 1860. The fissure divided, and the piles removed.

24th. The bowels relieved with less pain than she had had for many years past.

April 4th. Discharged, quite cured.

*Remarks.*—It will be observed, that when the bowels were relieved, only two days after the operation, she had a marked cessation of pain. This is invariably the case, and is a most cheering fact to both the surgeon and the patient.

*Case* 5.—H. A., aged 43, single, a lady from the country, had been suffering for some years from supposed uterine affection, and had had leeches and caustic applied to the uterus. But still derived no benefit. On examining per vaginam, I could find no disease or anything that required treatment. Menstruation was regular. On inquiring if she felt any pain on defecation, she replied, "Yes, frequently; sometimes acutely." On passing my finger within the sphincter, I found a painful fissure, opposite to it a small polypoid body, and two internal hemorrhoids. I advised that she should stay in town; and, after two or three aperient doses, that she should undergo the usual operation. This was done, and in ten days she was up, feeling quite well, and in a fortnight left town without an ache or pain in uterus or rectum. Two years have elapsed since, and she has continued perfectly well. She has frequently expressed her surprise that she had never been so thoroughly examined before when seeking medical advice.

*Case* 6.—P. C., a lady from the country, married, without children, had been for many years suffering from what she considered uterine disease, accompanied by painful menstruation, which was supposed to arise from constriction of the os and cervix uteri. Leeches, sponge tents, and uterine dilators were from time to time persevered in. Still she kept getting thinner and weaker; and from having been accustomed to ride on horseback, and to take much out-of-doors exercise, she was unable to do either, constantly reclining on her sofa, and being, in fact, a confirmed invalid. On inquiring if she suffered pain on defecation, she replied, "Yes, so much so, that she always dreaded any action of the bowels, and was obliged to take strong aperients; and then the pain extended a long way up the bowels, and left a long wearying, aching pain, which always made her feel sick and faint for some hours afterwards." On examination, I found the same condition of rectum as in the last case; but the fissure was deep, and evidently of long standing, and the sphincter itself was so firmly constricted as to admit with difficulty the point of my finger. I confidently promised a speedy cure if she submitted to the necessary operation. This she did on the 2d ultimo, and on the 4th, her bowels were relieved by castor oil enema, and an enormous

quantity of feculent matter removed. On the third day the bowels were also moved after a small dose of castor oil. The patient then, to her intense delight, found that she had none of her old painful suffering. Since then she has steadily and gradually progressed in health and strength. Her face, which had been thick and muddy, is now clear and bright, indicating all the signs of returning health. I could easily increase the number of these cases from my note-book.

*Remarks.*—Each of these cases clearly illustrates the proposition with which I started, and has a strong practical bearing to every obstetric surgeon. Cases of rectum disease, even without fissure—such as old and hardened polypus, stricture, prolapsus ani, internal hemorrhoids,—all produce more or less pain to the uterus, and are frequenly overlooked because the uterus itself is supposed to be the source of suffering.—*Lancet, July* 14, 1860, *p.* 31.

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## 111.—DEATH AND ALMOST ENTIRE ABSORPTION OF A FŒTUS OF SIX MONTHS' DEVELOPMENT.

By Dr. E. J. Fountain, Davenport, Iowa.

The following history will furnish a hint for a diagnosis in any similar case which may hereafter occur, and throw some light upon those rare and obscure cases of placental absorption, concerning which there has been so much difference of opinion. To me the nature of this case, until its final termination, was a mystery. The patient, whose recent condition has furnished the following report, is a lady of intelligence and refinement, and is the mother of several healthy children. She became pregnant, as she supposed, in the month of June. The symptoms were well marked, and the same as she had formerly experienced under like circumstances. She began to feel motion at about the usual time, and the enlargement of her person continued at the same rate as formerly, until about the first of December. At this period she felt slightly indisposed for a number of days, but not sick enough to take her bed or ask for medical aid. From this time her enlargement ceased advancing, but she continued to feel something like motion as before. Her health was excellent; better, she thought, than usual. During the winter she fancied she was growing smaller; and this opinion, at first a suspicion only, became confirmed before spring by positive evidence of diminished size of the abdomen. This diminution continued going on, but still she thought she felt motion as formerly, but always light. I was first consulted in reference to the case in the month of April, after the time when she expected to have been confined. Her appearance did not in the least indicate that she was pregnant, but on examination I found a very slight but tolerably well-defined enlargement of the hypogastric region. An examination per vagina revealed an increased size of the neck of the uterus, and its weight indicated some enlargement of the body itself. From a careful examination I concluded that the uterus could not be quite as large as the head of a child of ordinary size at birth. The lady felt a great anxiety to know the real character of her situation, but I had to confess that I could not determine in my own mind what it might be. She was certain that she felt motion every day, yet here was the uterus too small to contain a living fœtus of nine or even six months, and it was still constantly diminishing in size! Gestation had advanced regularly up to the sixth month, and then appeared gradually to recede, until at the time when she should have been delivered her figure differed but little from its natural appearance, and what remained of the enlargement was constantly diminishing. In the meantime there had been no discharge from the uterus of any kind, and she had suffered none but a slight temporary interruption to her usual good health. The case was a complete puzzle to me; and although various conjectures were made to explain her anomalous situation, they were all quite unsatisfactory to *myself.* I acknowledged to her that the case appeared a *mystery that I could not* understand; but as her general health was

good, I thought there was no occcasion for any active interference. Whatever might be the contents of the uterus, it was evident they were gradually being absorbed without injury to the constitution; and I concluded it would be time enough to interfere when any change in her condition would seem to require it. It is hardly necessary to say, that I referred the motion which she supposad she felt, entirely to her imagination, or to some sensation which she had mistaken for the movements of a living child. A few days after this she began to feel some pain in the back, and her appetite and general health was somewhat impaired. She became nervously anxious about her situation, and was disturbed by unpleasant dreams at night. In the absence of any knowledge of the nature of her condition, her imagination naturally furnished all manner of fancies which were calculated to excite her fears and depress her spirits. She still thought she felt motion, but never at any time had any symptom of labour pains. This was her situation on the 11th of May, eleven months from the commencement of gestation, when I determined to examine the uterus by the aid of the speculum, to explore its contents as far as possible, and endeavour to excite uterine action for the expulsion of whatever it might contain. I first passed into the neck a small silver catheter. This entered about half or three quarters of an inch, when its passage was resisted by some firm but slightly elastic obstacle. I then introduced a small and pointed injecting tube, perforated with a number of small openings at its extremity, and injected a small quantity of aqueous solution of iodine and iodide of potassium. The operation was attended with but very little pain, and I left her with directions to remain quiet and wait for the result. In about an hour labour pains commenced and continued regularly until I was called, which was about four hours after the operation. This method of exciting labour pains was entirely experimental on my part, and resulted in the effect desired with far more promptness than I anticipated. On examination, I found a membranous bag filled with a tolerably firm substance presenting in the upper portion of the vagina. As the pains were light, I administered ergot; stronger pains followed in about fifteen minutes, and soon my hand received the expelled mass in unbroken form and without a drop of blood. It was the remains of a fœtus, enveloped in its membranes unbroken, with the placenta attached. The placenta was very hard, evidently much compressed and reduced in substance, and partly rolled upon itself. The contents of the sac appeared almost solid, and of a dark colour. The whole mass was so small that it scarcely filled the hollow of one hand. A very slight flow of blood followed after a little while, and after-pains continued during the night. The patient's convalescence was quite rapid. No lacteal secretion was established, nor had her breasts enlarged in the least. On opening the sac at my office on the following day, I found it contained the greatly compressed and emaciated form of a fœtus. The flesh had been almost entirely absorbed, leaving little more than a skeleton, partially covered with a thin remnant of muscular tissue, and surrounded with a small quantity of an opaque yellowish liquid, of purulent consistence, but quite free from odour. The bones of the head were crowded over and under each other; the body and limbs were doubled up, and the whole compressed, with the placenta, into a small compact ball. Upon unfolding it, the skeleton was found to be entire in substance, and measured ten inches in length, without unlocking the bones of the head.

From the history of this case it is evident that the fœtus had been regularly developed, and reached its normal size and condition until at or near the sixth month of gestation; and that at this period the death of the fœtus had occurred from some unknown cause, and absorption of the contents of the uterus began and continued from this time up to the period of delivery. It is an interesting fact that this dead child should have been carried so long, about five months, without exciting uterine action. How much longer it *would* have remained had there been no interference, it is, of course, impossible *to know*; but it is certain that there had been no symptoms of any disposition on the part of the uterus to expel the fœtus. Still more remarkable

is the fact that so great an amount of absorption had taken place from the uterine cavity. The size of the patient, and the appearance and condition of the bones of the child, with other particulars stated, furnish evidence that the uterus had contained a living child of usual proportions at six months, and that all but the skeleton of this child, together with the placenta, membranes, &c., had been absorbed to such an extent that the whole mass which was finally expelled was not larger in bulk than a good-sized orange. With this case before us, we may readily believe that the entire absorption of a placenta is a possibility, and that it is more than probable that such an occurrence now and then does take place is established by undoubted evidence of a few cases of this kind, of which fact I am personally assured by Dr. C. R. Gilman, Professor of Obstetrics in the College of Physicians and Surgeons of New York.—*New York Journal of Medicine.—Glasgow Medical Journal, July*, 1860, *p.* 238.

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## 112.—ON DEODORISATION IN OBSTETRIC MEDICINE.

By Dr. Thomas Skinner, Liverpool.

On the 3rd of November, 1859, I communicated to the Liverpool Medical Society the value of simple tar-water as a deodorant injection in carcinoma uteri and other affections attended with offensive vaginal discharges. I then stated my belief that the active deodorising ingredient in tar-water, in Messrs. Smith and McDougall's patent disinfectant powder, and in the powders and pastes of M. M. Corne and Demeaux, of Paris, was creasote, or its analogue carbolic acid, or some such empyreumatic product. I am still of the same opinion, but further investigation has convinced me that so far as the preparations made from vegetable tar are concerned, besides creasote, the oil of tar acts the most important part as the deodorising agent. I was led to this conclusion by observing that, in preparing tar-water, so long as the tar is capable of communicating its peculiar smell to water, hot or cold, does its deodorizing property last. On making this observation I also concluded that the substance giving forth the tarry smell must be the active ingredient; and, as the substance is the oil of tar, I immediately instituted a number of experiments which have proved beyond any doubt, that however powerful creasote may be as an antiseptic and deodorant, it is very much surpassed by oil of tar, if not as an antiseptic, at least as a deodorant.

The oil of tar has been variously denominated oil or spirit of tar or pitch; ol. pini, ol. pini rubrum, ol. petrolei, ol. picis, ol. picis liquidæ, &c. There can be but little doubt that the oil of cade, (ol. cadinum, the huile de cade of the French—which is said to be prepared from the juniperus oxycedrus, or Languedoc juniper) is frequently, at least so far as the article of commerce in this country is concerned, nothing but oil of tar. Pereira says that the oil of tar is "a mixture of various volatile constituents of tar."—(Vol. ii. p. 1205, third edition.) The samples of the commercial oil vary much; I have scarcely seen two alike in colour, consistency, and smell. The best specimen I have seen is in the possession of Mr. Wharrie, chemist and druggist, of 50, Berry-street, in this town.

It was only the other day, while I was hastily glancing at the literature of the subject, that the following facts and opinions presented themselves:—In the 'Pharmaceutical Journal' for 1848–49, Mr. Griffin states, "Tar is a valuable ingredient in disinfecting mixtures; three pounds of tar will preserve twenty gallons of urine from putrefaction, and enable this valuable manure to be preserved or transported inoffensively." (Vol. viii. p. 14.) With this great fact before us, why, may I ask, is not tar turned to some account in deodorising our street, railway, and other urinals? In another article in the same journal, by the late Professor George Wilson, on 'Disinfectants, Antiseptics, and Deodorants,' he says, "The only other substance to which I shall at present refer is pitch-oil, one of the products of the distillation of tar; it is an antiseptic of the most powerful class, and very cheap,

and if not used in excess, it is applicable as a deodoriser." (Vol. xii. p. 283.)

For the last year or more, while surgeons, both in Paris and in London, have been actively engaged in investigating the subject of deodorisation in their own particular department, it seems strange that obstetricians should have done so little in theirs. It is not because obstetricians meet with fetid effluvia less frequently than pure surgeons; it is not because they are less sensible to their existence from being more used to them; but it arises simply from the fact, that they have never properly directed their attention to the best means of removing them.

In the practice of Obstetric Medicine there is no more crying evil, next to pain, than the universally-complained-of bad odour, inseparably associated with the lying-in chamber, particularly the odour of the bed, bedclothes, and patient. Some are much worse in this respect than others. The chief cause of the offensive effluvia is, without doubt, the lochial discharge. We all know how disagreeable, how heavy and how sickly the odour is; particularly a few days after delivery when it is becoming paler, and is passing into the condition termed by the nurses "green water." If it is so very offensive to us, how much more so must it be to the delicate feelings of the patient, particularly of the more accomplished and refined of the sex; and, if the sickly odour is so disagreeable in private practice, where there is only one individual in a chamber, it must necessarily be worse in an hospital for lying-in women, where several are congregated together. In some of the worst forms of puerperal fever also, the vaginal discharges are sometimes so putrid, and the smell so unbearable, that I have known nurses, and even friends, feel it most irksome to approach the patient, much less to extend to her their kindly offices. These may be looked upon as among the minor advantages to be derived from the practice of deodorisation in obstetric medicine. I would also direct attention to the more important question of the saving of life by deodorisation acting as a prophylactic measure in the prevention of puerperal fever and other *post partum* accidents.

Professor Simpson has shown that nearly 3000 mothers die in child-bed every year in England and Wales alone. He says: "Among these 3000 deaths, a comparatively small proportion only are the direct result of convulsions, hemorrhage, rupture of the uterus, and the other more immediate or primary complications and accidents connected with parturition. The great majority of these deaths is produced by puerperal fever."

Dr. Simpson here refers only to the deaths arising from puerperal fever, and not to the number of females attacked with the disease.

Granting, that 3000 females die in child-birth annually in England and Wales, that three-fourths of them—which is within the mark—die of puerperal fever alone, then that gives us 2250 deaths annually from fever attending child-birth. Granting, also, that the mortality in the absence of epidemic influence, ranges as high as 1 in 4, in round numbers this will present the fearful amount of at least 9000 women attacked annually with puerperal fever in England and Wales alone. Among the causes of this fearful scourge on puerperal females, there is one which I think has not been sufficiently recognised. I allude to the injurious influence of the *post partum* secretions and discharges of the puerperal female, and more particularly of those who have suffered from a tedious or obstructed labour, and who have been confined within the walls of Lying-in Hospitals.

In reference to the close relation which exists between Lying-in Hospitals and the origin of epidemics of puerperal fever, Professor Churchill, of Dublin, who may be looked upon as one of our best authorities on such a subject, says:—"From a review of the history of the epidemics of puerperal fever, it appears that there is some remarkable connexion between them and the Lying-in Hospitals. I do not mean strictly to assert that the epidemics always originate in, and are kept up by these institutions, but I refer to the fact, that we have no record of any epidemic independent of them in early *times." The* members of the Academy of Medicine of Paris in their ever-*memorable debate* of 1858, have put it beyond a doubt that their Lying-in

Hospitals are hot-beds for the generation and propagation of puerperal fever. The very best of them is not exempt from the charge. The Beaujon Model Hospital itself lost one out of every twelve women delivered between the years 1852 and 1856. But, if we contrast the results of private practice in the poorest *arrondissement* in Paris (the twelfth) with the mortality occurring at the Hospital de la Maternité, the bare facts speak for themselves. In the first, where the patients are isolated and attended at their own homes, the mortality is only 1 in every 322 women delivered; whereas, in one of the first hospitals of the city the mortality is so high that one woman dies out of every nineteen delivered within its walls.

Every obstetrician in Paris is convinced of the close connexion between Lying-in Hospitals and the origin of puerperal fever, and the only reliable steps which have been proposed to prevent or ameliorate the evil, have been such as will tend the more effectually to isolate each individual patient. They are all agreed that for want of this the present hospitals are powerless to stop the occurrence of the disease or to reduce the mortality; and that, when the fever makes its appearance in the wards, for the sake of humanity the hospitals should be closed.

As we can no longer doubt that Lying-in Hospitals and the crowding of parturient women are fruitful sources of the origin of puerperal fever, both epidemic and sporadic, let us cursorily examine whether or not it can arise among cases which are isolated.

Dr. Simpson, than whom no obstetrician has more carefully investigated this important subject, has said, that "in a small ward, or small Hospital, one could almost, as it were, manufacture puerperal fever at will, by crowding a great number of patients together in the same ill-ventilated room." . . . This, no doubt, was true when this experiment was driven, as it sometimes accidentally had been, to an extreme. But it was true, also, in its lesser degree; for Dr. Simpson believed that one great cause of weed, ephemera, and febrile attacks, during puerperal convalescence, was the still too slight attention that was paid to the ventilation of the lying-in chamber. "He had repeatedly, he thought, seen more or less slight febrile action set up in a patient from the curtains being closely drawn around her bed for eight or ten hours during the night, being thus obliged to breathe an air loaded and affected with the morbid animal discharges from her own body." I believe that most accoucheurs will agree with these statements of Dr. Simpson.

Without prosecuting the inquiry further for the present, I think that I am justified in concluding—1. That the decomposition of the organic tissues and fluids thrown off by the skin and vagina of the parturient female is to be looked upon as a fruitful source of puerperal fever, and other *post partum* febrile and inflammatory actions. 2. That, if such decomposition can be regarded as an agent in the development of such conditions when patients are isolated, it is evident that it must be a much more active cause in Lying-in Hospitals, where the emanations are more likely to accumulate, and lead to the generation of a peculiar miasmal condition of the air of a ward. 3. That, as the decomposition of organic matter gives rise to fetid odours, such odours in turn become sufficient evidence of decomposition and its dangers. 4. If the premises be true, then deodorisation of the lochia by an agent which stops decomposition promises fair to become a prophylactic of no mean value in puerperal fever and such like *post partum* accidents.

Any practicable means which, without much trouble and at a cheap rate, will safely and effectually relieve the parturient female of all disagreeable smell about her apartment, bed, or person; which will make the patient more comfortable and less loathsome to herself and friends; and which will at the same time assist in preventing the generation of miasma, or lessen the danger from the poisonous influences associated with putrid or morbid effluvia, must be hailed as a great addition to the Materia Medica of Obstetric Medicine. Such an agent, I think, we have in the oil of tar.—*Med. Times and Gazette, Sept.* 8, 1860, *p.* 231.

# MISCELLANEOUS SUBJECTS.

## 113.—ON A NEW MALFORMATION OF THE LOWER LIP, WITH SEVERAL OTHER MALFORMATIONS OCCURRING IN THE SAME FAMILY.

By J. Jardine Murray, Esq., F.R.C.S.E., Brighton.

[It is interesting to observe the curious cases of what are improperly called monstrosities in some families. They are proved, however, to be nothing more than an illustration of one of the most interesting laws which regulate our original conformation. As the mason adds one stone after another to his fabric, according to a definite rule, so our own fabrics are built up, portion after portion, according to fixed rules. Sometimes, however, a bit is omitted, and this gives rise to monstrosity—called now-a-days *teratology*, or the doctrine of monsters. We give the following paper, by Mr. Jardine Murray, of Brighton, as one of the best we have lately seen on the subject. Mr. Murray has the merit of having introduced to the notice of physiologists a peculiar and hitherto unrecorded malformation of the lower lip, and he passes in review the various possible explanations of its occurrence. He says:]

Having lately found in four members of one family instances of a singular malformation of the lower lip, which does not seem to have been alluded to by any writer on teratology, I purpose in this paper first, to describe the malformation, and then to consider the various possible explanations which might account for its occurrence. And as in the same family there are other important defects of formation, which, viewed collectively and in their relations to each other, afford interesting illustrations of the fundamental laws of teratology, I shall notice the individual peculiarities in the order in which they came under my observation.

In May, 1859, Dr. Thomson, Physician to the Royal Maternity Hospital of Edinburgh, requested me to see J. H., whose parents were anxious that he should immediately undergo an operation for the cure of hare-lip. The little patient was a well-developed and vigorous child, aged eighteen months. The fissure in the lip was confined to the left side, but extended quite into the nostril; and on this side, also, the anterior part of the hard palate was cleft from want of union between the intermaxillary bone and the left superior maxilla. The intermaxillary bone was twisted on itself, so that the incisor teeth were placed at an angle with the line of their ordinary direction, and partly bridged over the gap in the alveolar ridge. This twisting caused the portion of the alveolar ridge formed by the intermaxillary bone to overlap that portion formed by the inferior maxilla. The little patient had been brought up at the breast; but, in consequence of the above malformation, he had only been able to apply himself to the left nipple.

I believe that if the surgeon can choose his own time in hare-lip, he ought either to operate soon after birth, or wait till the child is two or three years old. But in this case there did not appear to be any reason to fear constitutional disturbance from dentition, which had been somewhat irregular. Both parents were extremely anxious that the operation should not be delayed; for the mother was again pregnant, and both she and her husband firmly believed that the daily contemplation of the infant's hare-lip would *have such* an effect on the mother as to produce a similar malformation in *her yet unborn* child. With the assistance of my friends Drs. Pow and *Messer, I* accordingly performed the operation on the 14th May, 1859.

Fig. 1.—J. H. (the eighth child), before operation. The condition of the Mouth is well shown in this wood-cut from a careful sketch. The marks on the lower lip indicate the openings of sacculi to be hereafter described.

Fig. 2.—The same, some months after operation. As nearly the entire intermaxillary bone has been preserved, the upper lip is normally prominent.

After extracting the incisor teeth, I pared off a small portion of the prominent angle of the alveolar ridge, hemorrhage from the gum being readily controlled by means of a hot wire. I next dissected the lip from the gum for some little distance on each side of the fissure. Then, having put the lip on the stretch by grasping the angle with catch forceps, I removed a crescentic portion from each margin of the fissure, and brought the edges together by means of two needles of soft steel and two sutures of silver wire. On the fourth day the needles were removed, and the cicatrix protected by a layer of collodion. During the following week, the cheeks were supported by a strip of adhesive plaster placed across the face.

The operation was entirely successful, and afforded the father so much satisfaction that he brought me another of his children, aged three years, who had been born with webbed fingers, and requested me to perform any operation I might think proper for the removal of the deformity.

On the left hand, the middle and ring fingers were closely united as far as their utmost tips; while, on the right hand, the like fingers were joined together by a web which did not extend much beyond the middle joint. It appeared that the bones and tendons of each finger were perfect, and that two fingers were merely wrapped in a common envelope of skin.

Fig. 3—*Left hand.* Webbed fingers. Right hand.
*This Boy (the seventh child)* was not the subject of any other malformation.

This malformation really consists in absence of fission, and is properly an arrest of development; for in the embryo the rudiments of hands and feet do not at first present any division of the fingers and toes.

I dissected the fingers from each other, and dressed each hand on a Y-shaped splint of gutta percha, the wrist being bandaged to that portion of the splint corresponding with the stem of the letter, and the middle and ring-fingers being respectively placed on the portions representing its arms. Thus, the undesirable re-formation of the web by cicatrization at the angle of divertication of the fingers, which has such a strong tendency to occur in these cases, was effectually prevented. A month afterwards the wounds were entirely healed, the fingers were almost free from stiffness, and the little fellow was using his hands as freely as any child of his age. Neither he nor any of his relatives have webbed toes.

Being now much interested in the peculiarities of this family, I was permitted to examine each of its members in turn; and I am thus enabled to construct the following table, which exhibits in a very striking manner the hereditary nature of physical malformations:—

| | |
|---|---|
| Father's mother, aged 65 . | . Palate very narrow and much arched. |
| Father, aged 41 . . | . Double hare lip.<br>Two sacculi in lower lip. |
| *Mother, aged 37. | |
| First child (girl), aged 17 | . Two sacculi in lower lip. |
| *Second child (girl), aged 15 + | . |
| Third child (girl), aged 13 + | . Double hare-lip.<br>Two sacculi in lower lip. |
| *Fourth child (girl), aged 11— | . |
| Fifth child (girl), aged .+ [Miscarriage.] | . Palate very narrow and much arched. |
| *Sixth child (boy), aged 5. | |
| Seventh child (boy), aged 3 | . Webbed fingers, both hands. |
| Eighth child (boy), aged 1½ | . Hare-lip (see description and figs. above.)<br>Two sacculi in lower lip. |

All the individuals alluded to in the above table are free from constitutional disease, and enjoy excellent health.

The father was born with double hare-lip, and was operated on by the late Mr. Liston. None of his brothers or sisters were thus affected. His mother's palate is, however, decidedly narrow and unusually arched in formation; and while she was somewhat eagerly accounting for the existence of hare-lip in her son, by the circumstance that shortly before his birth, she had been frightened by an elephant in Wombwell's menagerie, I was forcibly reminded of Mr. Fergusson's remark that—'Often, while listening to a mother's story about some conjectural cause for her infant's deformity of face, he has thought that a glance at her own features in the looking-glass might have given her a more plausible reason for the condition of her offspring.'

In the contracted formation of the palate, the fifth child exactly resembles her paternal grandmother.

The seventh child, as already mentioned, was born with the middle and ring-finger of each hand closely united by skin.

The eldest child, aged seventeen, is quite womanly in appearance. She is in every respect well-formed, save that in the lower lip there are present two pouches or sacculi, which constitute the interesting congenital peculiarity now to be described.

The under lip is full and fleshy. About a quarter of an inch from the external edge of the pink mucous membrane of the prolabium are two crescent openings, exactly similar in appearance and symmetrical in position, one

* *The mother* is in every respect well formed, and her second, fourth, and sixth children are also *quite normal in development*. She does not inherit any tendency to malformations such as are *found among her* children.

placed on either side of the mesial line. The horns of the crescent are directed forwards, and a little outwards. A probe inserted into one of these openings passes downwards on the inner side of the lip under a considerable thickness of mucous membrane, to the depth of half-an-inch. In each pouch a split pea might readily be introduced. The pouches do not communicate with each other; they secrete glairy mucous, and their presence does not occasion any inconvenience.

Sacculi in every respect precisely similar to these are present in the under-lips of the father of this family, and of his third and eighth children, all of whom were, however, born with the additional malformation of hare-lip.

By referring to the portraits of the eighth child (Figs. 1 and 2), the indications of the crescentic mouths of the sacculi may be seen on the prolabium of the lower lip. The two following woodcuts are carefully made from photographs of the father and his third daughter.

Fig. 4—The lower portion of the face of the Father, aged forty-one. He was born with double harelip, and at the age of thirteen was operated on by the late Mr. Liston. The prominence of the under-lip is due to the scantiness of the upper lip, which is drawn tightly over the gap caused by the removal of the intermaxillary bone. The crescentic openings of the sacculi on the prolabium of the under-lip are faithfully represented.

How, then, are we to account for this malformation in the lower lip?

1. The malformation is evidently not consequent on the occurrence of hare-lip; for although the father and the third and eighth children were born with hare-lip, and also sacculi in the lower lip, the eldest child has the sacculi in the lower lip very large, while her mouth is in every other respect well-formed.

Fig. 5—The lower portion of the face of the third child, aged thirteen. She was born with double hare-lip, and was operated on in infancy. The intermaxillary bone has been removed, and the upper lip is tightly stretched over the gap in the jaw thus created. The openings of the sacculi are well shown. The mucous membrane round the opening of the sacculus to the right of the mesial line is represented as it appeared when put on the stretch by means of a probe.

2. The sacculi might possibly be ascribed to the erratic development of tooth-sacs in the *under-lip*. *But in* all so-called "freaks of Nature" in the *human subject, there appears to be* an approximation to the normal condition

of some one of the lower divisions of the vertebrate animal kingdom; and I am not aware that true labial teeth are present even among fishes.

3. The sacculi cannot be ascribed to arrested formation, for in all the four persons in whom they occur there is complete union in the mesial line of the lower lip. And in all mammals, the under lip and lower jaw are developed from two lateral pieces, though the formation is more complex in some reptiles and fishes.

4. Dr. Ormerod has suggested to me that the malformation may possibly be explained by some peculiarity in the development of the frænum. The crescentic form of the mouths of the sacculi, and their perfectly symmetrical character, render the suggestion well worthy of consideration. But, in other respects, the formation and position of the sacculi do not appear to accord with this explanation.

5. Are the sacculi the result of intra-uterine disease of the labial glands? Some considerations favour this view. Surgeons are aware that the labial, like other mucous glands, are frequently the seat of cystic disease. In the 'Guy's Hospital Reports' for 1859, Mr. Birkett has recorded an interesting case of labial cyst, occupying the middle of the prolabium of the lower lip, a little to the left of the mesial line.

The peculiarly wide and crescentic openings of the sacculi, however, and the fact that in all four individuals they are so perfectly uniform in appearance and symmetrical in position, seem opposed to such an explanation.

Congenital malformations are sometimes evidently due to disease of the ovum or fœtus, and probably are often to be attributed to the male and female reproductive elements having been affected prior to the act of conception; but assuredly the most common explanation is to be found in impeded or arrested development of the fœtus, from some remote or unknown cause. Now, if we suppose the sacculi to be consequent on intra-uterine disease of the labial glands, it is to be noted that in each of three members of the family which forms the subject of this paper, we have two malformations—one (hare-lip) due to arrest of development, and the other (sacculi in lower lip) caused by intra-uterine disease. But there is ample proof that intra-uterine disease may co-exist with arrested development of the fœtus.

At my request Professor Goodsir very carefully examined the sacculi; and while he concurred with me in thinking them extremely interesting and remarkable, he confessed himself unable to account for their existence. I have since communicated with Professor Owen, Dr. Simpson, Mr. Paget, Mr. Green, Mr. Quekett, and others whom I thought likely to throw light on the subject. But I believe that a careful record of facts and description of appearances will better fulfil the requirements of science, than a painstaking advocacy of any one possible explanation of this interesting malformation.—*British and Foreign Medico-Chirurgical Review*, *Oct.* 1860, *p.* 502.

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## 114.—ON THE PRESERVATION OF URINARY DEPOSITS.

By Dr. Lionel Beale, F.R.S., Physician to King's College Hospital.

The different characters of urinary deposits render necessary different plans of preservation. It is, therefore, desirable to consider the nature of the deposit before we attempt to preserve it. Some deposits may be preserved *dry*, others may be mounted in *Canada balsam*. A certain number exhibit their characters very well if preserved in *glycerine*, while many can only be kept in certain *aqueous fluids*.

*Of placing the Deposit in the Preservative Fluid.*—After the deposit has been allowed to settle in a conical glass, the supernatant fluid is to be poured off; and if it is to be mounted *in fluid*, a quantity of the preservative solution, equal in bulk to the urine and deposit that remain, is to be added. After the deposit has again settled, the fluid is to be poured off and replaced with an equal portion of fresh preservative solution. In this way the deposit is washed clean, and properly impregnated with the preservative fluid.

When preparations are to be preserved in a fluid medium, a small shallow water-tight cell is to be used. The specimen and its preservative fluid being placed in the cell, the thin glass is applied, and the cover cemented in its place with the aid of Brunswick black or other cement. (*How to work with the Microscope.*) In washing urinary deposits prior to mounting them, it is often necessary to add some compound to the water used for this purpose, in which they are known to be insoluble; and sometimes it is desirable to add some substance to increase the density of the fluid; for which purpose certain salts, syrup or glycerine, may be employed, according to circumstances. Many deposits, although soluble to some extent in pure water, are quite insoluble in a weak acid; others are insoluble in a weak alkali or in certain saline solutions. Again, it is sometimes desirable to separate certain substances in the deposit from others, and this may be effected by special chemical solutions which have the power of acting on the one and not upon the other; or, in cases where one is more dense than the other, by agitating the deposit with water, and, after allowing time for the heavier one to settle, pouring off the lighter one into another vessel, to subside there. From this it may be collected in the usual way.

If the preparation is to be preserved as a *dry* object, water is to be added in the first place; and a portion of the deposit, which has thus been carefully washed, is to be removed with the aid of a pipette to the glass-slide, and the fluid allowed to evaporate, the whole being covered by a bell-jar, and placed over a dish of strong sulphuric acid. When dry, it is to be protected from dust by a thin glass cover.

The glass cover is easily prevented from pressing upon the preparation by interposing a thin piece of paper or cardboard; or a thin India-rubber ring, which may be easily fixed to the glass slide and thin glass cover, by a little gum made into a thick paste with whiting, may be used.

If the specimen is to be mounted in *Canada balsam* or turpentine, it is to be dried in the manner just described, warmed slightly, wetted with the balsam and mounted with the usual precautions. (*How to work with the Microscope.*)

*Refractive Power of the Medium in which Deposits are Mounted.*—The appearance of objects in the microscope depends very much upon the medium in which they are immersed; and many structures are so altered in their character by different media, that they would hardly be recognised as the same object. It may be said generally, that the darker the object, and the more dense its structure, the higher should be the refractive power of the medium in which it is mounted—thus the dark-coloured uric acid, or the thick spherical crystals of carbonate of lime, and the dumb-bells of oxalate of lime, exhibit their structure to the greatest advantage when mounted in the highly refracting *Canada balsam*, or in *strong syrup* or *glycerine*, while the beautifully transparent octohedra of oxalate of lime would be scarcely visible in these media, and require to be mounted in an aqueous fluid which possesses a lower degree of refractive power. Many of these objects, when mounted dry, appear quite dark, and scarcely exhibit any structure at all, in consequence of great difference in the refracting power of their substance, and the air by which they are surrounded. From what I have said, it will be evident how important it is to examine the same object in different media—in fact, it is quite impossible to form an idea of the real structure of many specimens, without proceeding in this manner. (*How to work with the Microscope*, p. 59; and *The Microscope in its Application to Practical Medicine*, second edit., sects. 74, 80, and 90.)

*Media in which Urinary Deposits may be preserved.*—Urinary deposits may be mounted in *air*, in *turpentine*, *oil*, or *Canada balsam*; in *glycerine*, in *gelatine and glycerine*, in *solution of naptha and creasote*, in *certain saline solutions*, in *weak spirit*, and in some other aqueous solutions, which will be alluded to. The "*glycerine*" which I use is *Price's patent glycerine*, diluted with water.

The composition of the naphtha and creasote fluid, above referred to, is as follows:—

*Solution of Naptha and Creasote.*

| | |
|---|---|
| Creasote . . . | 3 drachms. |
| Wood naptha . . | 6 ounces. |
| Distilled water . . | 64 ounces. |

Chalk, as much as may be necessary.

Mix first the naptha and creasote, then add as much prepared chalk as may be sufficient to form a smooth thick paste; afterwards add, very gradually, a small quantity of the water, which must be well mixed with the other ingredients in a mortar. Add two or three small lumps of camphor, and allow the mixture to stand in a lightly covered vessel for a fortnight or three weeks, with occasional stirring. The almost clear supernatant fluid may then be poured off and filtered, if necessary. It should be kept in well corked or stoppered bottles.

*Of keeping the Urinary Deposit for subsequent inquiries.*—In cases where it is desirable to retain a certain quantity of the deposit in the preservative solution for subsequent examination, or for the purpose of making more preparations, it should be kept in a small glass tube, with a tight fitting cork, and carefully labelled. Most urinary deposits may be kept for a longer time in this manner, than mounted in thin cells. I propose now to describe briefly the various plans adapted for the preservation of urinary deposits which I have found to succeed best.

### First Class Urinary Deposits.

*Mucus.*—It is very difficult to preserve the character of the so-called "mucus corpuscles," or imperfectly formed epithelial cells, nuclei, and granules, which constitute the slight flocculent deposit met with in healthy urine, and termed "mucus." The naphtha and creasote solution is best adapted for the purpose, and it is desirable to place the specimen in a cell about the twentieth of an inch deep.

*Epithelium.*—The different varieties of epithelium are easily preserved, although, after the lapse of some time, minute oil globules make their appearance in them. They may be kept in naphtha and creasote fluid, to which one fourth of its bulk of glycerine has been added. It is well to put up specimens of epithelium from the urethra, bladder, ureter, and pelvis of the kidney, removed from the organs of a healthy man who has been killed accidentally. They should be mounted in very thin cells. Specimens of the epithelium from the vagina, which can generally be obtained from the urine of females, should also be preserved.

*Vegetable Growths: Fungi.*—I have found that fungi may be preserved most satisfactorily in glycerine, for although they appear somewhat more transparent in this fluid than in urine, they preserve their general character better than when immersed in other preservative fluids. It is necessary to add weak glycerine in the first instance, and to increase the strength gradually, otherwise the fungi become collapsed, owing to the great density of the strong solution. A solution composed of equal parts of water and Price's glycerine is sufficiently strong to preserve fungi. I have not been able to preserve specimens of sarcinæ which I have met with on two or three occasions in the urine, probably in consequence of their extreme delicacy. The sarcinæ which are from time to time met with in vomit keep perfectly well, and preserve their recent characters in glycerine.

*Spermatozoa* are sometimes mounted in the dry way; but although their general form is preserved, their refractive power and transparent appearance are so different from what is observed when they are immersed in urine, that little is gained from such preparations. Spermatozoa keep very well in glycerine, although they appear rather more faint than in an aqueous fluid. They should be examined with the *eighth of an inch object glass* (× about 400); but when the eye of the observer has become familiar with the general appearances, they may be readily recognised with a quarter of an inch *object-glass* (× about 200).

*Casts.*—It is not difficult to preserve the character of some varieties of

casts. The transparents casts often become covered with numerous minute granules and oil globules, and their character much altered. Granular casts and epithelial casts often keep very well in the naphtha and creasote solution; but altogether I prefer glycerine, with one-third part of water. Although in many instances the cells they contain are altered, and oil globules appear much more transparent than when in urine, this alteration in character may be easily allowed for. I have some specimens of large waxy casts and epithelial casts which have been kept in the naphtha and creasote solution for upwards of seven years, and still preserve their characters well. The specimens in glycerine, of course, keep admirably. Some casts may also be preserved in gelatine and glycerine, care being taken that the mixture is not made too hot.

### Second Class of Urinary Deposits.

*Pus.*—Recent specimens of pus may be so readily obtained that it is hardly necessary to attempt to preserve the corpuscles permanently. Their characters alter so much in all the preservative fluids that I have tried, that after they have been put up for some time, it would be difficnlt to recognise the nature of the preparation.*

*Phosphates.*—The phosphate of lime, in its amorphous form, in globules, and minute dumb-bells, is easily preserved in weak spirit, naphtha and creasote fluid, or glycerine; but the character of the crystals of the triple, or ammoniaco-magnesian phosphate, could not be retained in this solution. As is well known, this salt is quite insoluble in solutions of ammoniacal salts, and these make the best preservative solutions for it. Crystals of triple phosphate may be kept for any length of time, with their smooth surfaces and their lustre unimpaired, in distilled water, to which a little chloride of ammonia has been added. Phosphate of lime and the stellar form of triple phosphate may be dried carefully, and mounted in Canada balsam; but, of course, the appearance of the crystals is a good deal altered.

*Urates.*—As the urates are so commonly met with, and as they aregenerally deposited in the form of granules, there is scarcely any need of mounting them as permanent objects. If desired, however, deposits of this kind may be preserved by adding a little naphtha and creasote fluid to the deposit, which should be left in it for a considerable time before it is put up. Urates which crystallise in small spherical masses, as often occur in the urine of children, and more rarely in irregularly branched processes, may be preserved very well in Canada balsam, or, if preferred, they may be kept in the naphtha and creasote fluid.

### Third Class of Urinary Deposits.

*Blood Corpuscles* become more or less altered in most preservative fluids. I think that those which I have mounted in glycerine (one part glycerine to two parts of water) have undergone the least change.

*Uric Acid Crystals* are easily preserved as permanent objects. The usual plan is to mount them in Canada balsam. They should be washed, in the first instance, with little water, to which a few drops of acetic acid have been added. When pretty clean, they may be placed upon a glass slide, with the aid of a pipette, and the greater quantity of the fluid absorbed with a small piece of bibulous paper. After the crystals have been properly arranged on the slide with a needle, they may be dried, by exposure under a bell jar over a dish containing sulphuric acid. When quite dry, they may be moistened with a drop of turpentine, and mounted in Canada balsam. In this operation, a very slight heat should be employed, otherwise the crystals will become cracked in all directions, and more or less opaque. Uric acid crystals, as a general rule, do not keep well in glycerine. In cases where we wish to preserve other substances in the deposit as well as uric acid crys-

* Dr. Andrew Clarke speaks highly of some fluids, containing bichloride of mercury and arsenious acid, which he has prepared. (*The Microscope in its Application to Practical Medicine*, second edition, p. 237, note.)

tals, the naphtha and creasote fluid will be found to answer very well. I have some preparations mounted in this manner, which were put up six or seven years ago.

*Cystine.*—Crystals of cystine may be preserved in Canada balsam, the same care being taken in mounting them as mentioned under uric acid, or they may be kept very well in distilled water, or in the naphtha and creasote fluid, to which a little acetic acid has been added.

*Oxalate of Lime.*—Both the octohedra and dumb-bells may be preserved in glycerine; but the former look very transparent in that fluid. The dumb-bells may also be mounted in Canada balsam, but in this medium the octohedra are almost invisible. When required for polarising, they should be put up in balsam. The dumb-bells keep very well in glycerine.

*On Preserving Crystalline Compounds obtained from Urine.*—It is exceedingly difficult to preserve many of the crystalline substances obtained from urine in a moist state: but several of them form beautiful microscopic objects when carefully dried. *Urea, nitrate of urea, oxalate of urea, creatine, creatinine alloxan, hippuric acid, murexid* and many others, may be kept as permanent objects in this manner. In order to prepare them, it is better to cause them to crystallise upon a glass slide; allow the mother liquor to drain off, and immediately, place the slide under a bell-jar over sulphuric acid. Sometimes the crystals may be made in a small evaporating basin, and when drained and dried, a portion of them may be removed to a glass cell, and covered with a piece of thin glass to exclude the dust. Many crystals may be examined and preserved for a considerable time in their own mother liquor, especially when they are very slightly soluble in fluid; but, as a general rule, this plan does not answer very satisfactorily, for, independently of the escape of the fluid from the edges of the cell, a few of the largest crystals grow still larger at the expense of the smaller ones, and the beauty of the specimen is destroyed.—*Brit. Med. Journal, Sept.* 22, 1860, *p.* 736.

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115.—*Chloride of Zinc Moulded into Sticks for the purpose of Cauterization.*—Soften gutta-percha in boiling alcohol, and incorporate it with finely-pulverized chloride of lime in a warm porcelain mortar, taking equal parts of each. Then roll rapidly on a porphyry slab, to the diameter of a quill, and divide in fragments, each of which shall be pointed at one end. Keep these in a wide-mouthed bottle in powdered lime. These sticks remain perfectly hard, are easily handled, cauterize with great regularity, and act as a sponge through which the chloride will slowly exude, becoming liquid by the action of the air and the skin.—*Lancet, July* 21, 1860, *p.* 64.

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116.—*Preservation of Bodies for Anatomical Purposes.*—Professor BUDGE has found that bodies may be admirably preserved for a long period of time, whether for anatomical purposes, or for courses of operative surgery, by injecting into the carotid preservative fluid composed of pyroligneous acid and sulphate of zinc, of each from eight to twelve drachms to seven pounds of water. Bodies thus injected have kept well during eight weeks of intense summer heat, without giving rise to any putrefactive smell, the muscles retaining their red colour, and though a little softened, admitting of good dissection. The injection does not prevent the subsequent injection of coloured matters; and the knives used in dissection scarcely suffer at all.—*London Med. Review, July* 1860, *p.* 48.

## 117.—CASES OF BRONCHOCELE SUCCESSFULLY TREATED BY STRYCHNIA.

By Dr. Murney.

The case which first suggested to me the use of strychnia was that of a lady, who stated that up to four years previously her health was rather good, but about that time the catamenia became irregular, and attended with great pain on each periodic return. She assigned as the cause of the thyroid tumour great distress of mind and disappointments. When I saw her the disease was of six months' duration; the right lobe of the thyroid gland was fully as large as a turkey's egg, extending laterally so as to press on the right carotid and jugular vein. A strong pulsation was felt in the tumour, and a purring thrill was communicated to the hand. She had no palpitation of heart. The action of the heart was quiet and regular. She complained of headache, and of a buzzing noise in the right ear. She imagined there was a slight enlargement of the right eye, and that she felt a sensation of fulness in it. The tumour at times impeded her deglutition, and occasionally her breathing also. I attended her from time to time for a year and a half; during that period I gave her everything I believe that has been recommended for this disease. In addition, by my advice she resided at the sea-side, and used sea-bathing; still the tumour continued undiminished. About this time I ceased to attend her. Three months ago, however, she presented herself again. She told me she got relief for some time from her more distressing symptoms, but the tumour never reduced in size. Latterly all the old symptoms returned in an aggravated degree; the tumour was larger than at first, and the left lobe was now nearly as much enlarged as the right. The difficulty of swallowing was increased; pulse quick and small, ranging from 100 to 120. I prescribed strychnia, in the dose of the sixteenth of a grain three times a-day. At the end of a fortnight the improvement was great, the headache was completely relieved, the bowels became quite regular, and the right lobe of the thyroid had shrunk considerably. She continued to take the strychnia for some time longer, when the gland had almost assumed its natural size. She has now no disagreeable symptom, and her general health is greatly improved. This lady is now married for two years. She is about thirty years of age.

The second case occurred in a housemaid, unmarried, about thirty-five years of age. When I saw her the disease had lasted for twelve months, and the thyroid gland had acquired a very great size, both lobes being equally enlarged. There was a very distinct remissement all over the tumour. She complained of great palpitation of the heart; pulse 130; great prominence of the eyes; catamenia irrregular; tongue large and flabby, of a whitish pale colour, and covered with a yellow fur in centre; bowels constipated, only acting about once a week. She stated that about eight years ago she was in a situation where she was very hard worked, and in consequence her health began to decline, and the menses disappeared for two years. Somewhere about this time she perceived the swelling in her neck, which soon acquired a very formidable size. Four months after the bronchocele began, palpitation of the heart set in; this was succeeded shortly by the prominence of the eyes. This case baffled and resisted all treatment. Iodine, internally and externally, iron, zinc, digitalis, and biniodide of mercury proved ineffectual. She was subject occasionally for the last year and a half to paroxysms of violent palpitations of the heart. I gave her the strychnia in a fluid form, as she could not swallow pills; I ordered a grain of strychnia, a drachm of dilute sulphuric acid, water seven drachms, to begin with five drops three times a day, and increase a drop each dose up to twenty, three times a day. When she was ten days taking this medicine a great flow of the menstrual discharge came on, lasted a few days, and returned in a week. She persevered in taking the medicine. Soon after the gland began to shrink, and is now so much reduced that one would scarcely imagine, if *they were not told so, that it* was ever diseased. The prominence

of the eyes is less; she is quite aware of this herself. She is now able to close the lids without any effort on her part; this she could not do before the reduction took place in the gland.

The third case is that of a young lady of fifteen years of age. She states that the swelling in her neck began three years ago, shortly after the death of her only brother, to whom she was very much attached, that it gradually increased, but was not of any inconvenience to her more than the deformity that it produced. Both lobes were enlarged, the right more so than the left. There was no fremissement in it; the general circulation was quiet, except that the heart became excited on making the least exertion. She had a dry, husky cough, attended with a ringing sound, a pallid look, not the greenish yellow of chlorosis, but the blanched appearance that is seen in cases of hemorrhage. Notwithstanding all this she was not emaciated, but, on the contrary, quite plump. The appetite was rather good, the catamenia regular but scanty in quantity; bowels sluggish, never moved except by medicine. Enlargement of the thyroid gland is very common in anæmic females, independent of any disease of the heart, functional or otherwise. Considering this case to be more or less a case of anæmia, I gave her iron combined with the strychnia in solution, and the result was very satisfactory. In the course of ten days the improvement was great, the bowels became regular, the headache left her, and the languid and prostrate state had completely disappeared, as the gland began to shrink. She is progressing most satisfactorily; the gland is now become quite soft, but it has not the shrunk appearance or contracted feel of the two other cases yet, but I am quite hopeful that it will ultimately become reduced to its natural dimensions.

As to the pathology of this very curious and interesting disease or its cure, I do not intend to do more than merely glance at it; my object is gained when I have brought forward those cases, and detailed their history and treatment; and as strychnia has never been used before (as far as I know in similar cases, I shall be disposed to consider this treatment of bronchocele as unique unless I hear something to the contrary; and if this treatment proves successful on a more extensive use of it, I think we shall have gained a considerable auxiliary to our therapeutic remedies. A word or two as to the cause of this singular disease; consulting my own experience of the cases that I have seen, I would say by far the greater number, indeed I may say all, had their origin in mental impressions of some kind. Hence I deduce that the most of our cases of bronchocele are the result of impaired innervation or a perverted action of the nervous functions, at least in this country, and not of climate, locality, or the peculiarity of snow water, as is generally alleged to be the cause in other countries. I look on the thyroid gland to be analogous to the spleen, first, in a mechanical view in acting as a diverticulum to the brain, as we know the spleen does to the stomach; and, secondly, in its pathology; enlargement and congestion of the spleen are often the result in certain localities of malaria; very often it is caused by affections of the mind, and the remedy for this affection of the spleen is either tonics or stringents, or both.—*Dublin Hospital Gazette, June* 1, 1860, *p.* 164.

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## 118.—ON VENTILATION: McKINNELL'S SYSTEM.

In arranging his plans for ventilation, McKinnell takes advantage of two simple and well-known facts. If an aperture is made, or a shaft carried through the ceiling of any room, or roof of any building, two currents of air are at once established; and these two currents, instead of jostling each other with human perversity, pass uniformly in definite courses according to fixed laws. The centre of the opening is occupied by an out-draught of the warm exhausted air of the building, while the sides are lined with an in-*setting* current of the colder and purer atmosphere. One shifting column of *air is contained* within the other, and the two have a relative movement some*what like that* of the pieces of a telescope. Any one may illustrate this fact

of the regular self-inclosed movements of fluids, of differing temperatures and densities, by warming a little water suspending a few floating coloured particles, in a test-tube, over a spirit-lamp. Directly the heat is applied, the coloured matter will indicate the presence of two opposite currents, in the positions and directions just mentioned. McKinnell carries two funnels or shafts, of different sizes and lengths, through the roof of the building he wishes to ventilate. The smaller and longer tube is placed in the centre of the larger one, and thus the two currents of air naturally seeking to pass into and out of the apartment, are separated one from the other; and this concentric arrangement of the two tubes is so devised that the capacity of the central tube shall be equal to the annular space enclosed between its outer circumference and the inner aspect of the external concentric tube. The lower end of the inner tube is moveable, and has an expanding mouthpiece or flange. This trumpet-shaped expansion serves two purposes; if drawn down away from the outer tube, it spreads the incoming cold air over the upper part of the room, and so causes its descent to be more uniform and dispersed; it may also by being raised to the level of the inner opening of the outer tube, be made to act as a valve shutting out altogether the external air, or limiting the currents to the area of the central tube. There is no mystery in this. The contrivance is simplicity itself; as, indeed, are most of the mechanisms adapted to natural laws. As applied to the forms of buildings and to the various compartments of compound structures, it acquires a little appearance of complexity, from the course and partial separation of the tubes, and the addition of appendages, to meet architectural requirements. But the principle remains the same, and the slightest infringement of it of course mars the scheme, and leads to disappointment. It, however, admits of infinite modifications, and can be fitted with equal ease to hats, carriages, and ship-cabins, houses, coal-mines, schools, churches, and dungeons. The system, too, is cheap. At small cost it may be introduced into old houses or parts of houses. Of course, in new buildings, the outlay is proportionately much less. But, under any circumstances, no one need be deterred by fear of expense, especially if contrasted with the money required for rearing a certificated master or mistress. This is a point of the argument which we are almost ashamed to introduce, though with a certain class of minds there is much to be gained by a sort of profit and loss reckoning, in which the tallies are human beings like ourselves. They can be made to act rightly and advantageously in the right direction, by an appeal to a standard with which they are familiar; whereas you would be met with a stolid stare of wonderment if you took your ground for reformation and money-spending upon the natural claim for care and consideration, in the matter of health, of your officials and the little untrained victims of "education." This system meets their case effectually. No matter what the building is, how mean, how close, how ill-contrived, the best air that is outside of it can be got in, and the worst got out, for a mere trifle. The direct outlay would be thought nothing of even by a small higgling committee; and if universally made, the raw material once manufactured into certificated teachers would last a long while, and the subscriptions to the factory might be in a measure reduced. We actually meet with people of this stamp, and needs must we act upon their intellects and moral sense with what would be caustics and searing-irons to better natures. In conclusion, this scheme of ventilation brought forward by Mr. McKinnell is unquestionably good, easy, and economical, and completely does that which is the great object in keeping buildings healthy and habitable. It rids them of the foul air, and provides for the free inlet of a fresh supply from the outside. —*Med. Times and Gazette, July* 7, 1860, *p.* 11.

## 119.—FATAL DISEASE PRODUCED BY THE TRICHINA SPIRALIS; ARTIFICIAL PROPAGATION OF THE PARASITE.

Trichina spiralis, known formerly only in a capsulated state, was considered more as a zoological curiosity than as a subject of pathological interest. No symptoms were known to betray its presence; and it was, perhaps, more frequently discovered in the dissecting-room of the students than at the *post-mortems* conducted by the pathologist. Since its discovery by Owen, numerous conjectures have been formed relative to the nature, origin, and propagation of this singular parasite. Herbst believed it to be identical with filaria, Meissner and Davaine regarded it as a larva of trichosoma, and Kuchenmeister considered it to represent an undeveloped, juvenile stage of tricocephalus dispar. The latter idea seemed to be confirmed by some experiments of Leuckart, who found trichinæ in the muscles of an animal fed with trichocephali; these experiments could, however, not be considered as conclusive, as the examination of the muscles before the experiment was commenced had been neglected. Herbst was the first to institute feeding experiments with trichinatous muscles. He found trichinæ in the muscles of the animals experimented upon; but the connective links between the parasites introduced into the stomach and those found in the muscles being deficient, no perfect light was thrown on the subject by these observations. The first experiments of Virchow, instituted last summer, tended materially to supply these deficiencies. In the intestinal canal of an animal fed with trichinatous muscle, he found the villi crowded with psorospermia; and free in the intestinal mucus numerous thread-like worms of the form of trichinæ, of both sexes, the sexual utricle of the male filled with sperm-cells, that o the female densely stocked with ovules. Trichinæ were thus proved to be bisexual. Their non-identity with tricocephali was also placed beyond doubt. The numerous points still requiring elucidation were being reserved for further inquiry, when, in January of the present year, the following case was observed by Professor Zenker, of Dresden, which, in conjunction with the experiments to which it gave the impetus, and for which it supplied the material, not only served to bring about a final settlement of the zoological part of the question, but disclosed the startling and alarming pathological fact, that trichina spiralis, hitherto considered to be an innocent parasite, is in reality the most terrible and dangerous of its kind,—that it can actually kill a healthy adult in a few weeks, under the most distressing symptoms:—

On January 12, 1860, a robust maid-servant, 24 years of age, was admitted into the Dresden Hospital. She had been indisposed since Christmas, and confined to bed since New Year's-day; complaining of depression, lassitude, sleeplessness, loss of appetite, heat, and thirst. These symptoms persisted on her admission; there was considerable pyrexia; the abdomen painful and tympanitic; and although neither splenic tumour nor roseola were present, the case was put down as one of typhoid fever. A remarkable affection of the whole muscular system now rapidly supervened, consisting in extreme painfulness of the extremities, with contractions of knee and elbow-joints, and œdematous swelling, particularly of the legs. The pain was so severe that the patient was continually moaning. Pneumonic symptoms supervened, and death took place on the 27th inst., preceded for twenty-four hours by an apathetic condition. The *post-mortem* examination showed in the internal organs merely an atelectatic condition of the left lung, with numerous small lobular infiltrations, bronchitis and hyperæmia of the mucous lining of the ileum. The muscles, however, which showed a greyish-red colour and a slightly freckled appearance, were found, on microscopic examination, to harbour vast numbers of non-capsulated trichinæ. The parasites were living, some coiled in spirals, others with extended *bodies*; and all (as Professor Virchow was the first to show, in a fragment *of muscle which* was forwarded to him for examination) living within the *sarcolemma of* the primitive fibrils. They showed various stages of devel-

opment; they were diffused over all the striated muscles of the body, with exception of the heart, and that in such vast numbers, that, under a small magnifying power, as many as twenty were in the field of vision simultaneously. The muscular substance was otherwise fragile, homogeneous, non-striated, and showed numerous transverse fissures. The intestinal mucus was found to be swarming with mature trichinæ of both sexes; and the remarkable fact was elicited, that female trichinæ are viviparous; the central portion of the bodies being observed to be full of well-developed embryos.

Inquiry being directed to the probable source of the trichinatous infection, it was ascertained that on December 21, four days before the patient was taken ill, two pigs and an ox had been slaughtered in the establishment of her master. Some smoked ham, and sausage prepared from the meat of one of the pigs, were fortunately obtained, and on examination proved to be full of trichinæ. The parasites had a shruken appearance; otherwise unchanged: reassumed a normal appearance on addition of water, but showed no signs of vitality. It is particularly worthy of remark, that to the naked eye the ham appeared perfectly healthy. It is very likely that the deceased had partaken of some of the raw meat. The butcher of the establishment (butchers notoriously indulge in raw meat) had also been taken seriously ill a short time afterwards, and was confined to his bed for three weeks with severe muscular pains, his whole body being semi-paralytic, &c. This complaint was ascribed to rheumatism at the time, but Professor Zenker correctly surmises that an immigration of trichinæ, not sufficiently extensive to prove fatal may have been the cause of the attack; and that capsulated trichinæ would very likely be discoverable in his muscle. Professor Virchow immediately commenced a series of feeding experiments with the pieces of human muscle forwarded to him by Professor Zenker.

The following is a brief statement of the results, as published in the last, number of Virchow's Archiv. :—

Rabbits fed with trichinæ die in about a month under symptoms of general muscular paralysis.

The trichinæ, which, as long as they reside in muscle, have no perfect sexual organs, become perfectly developed in the ileum. They are found free in the duodenum about six hours after a piece of trichinatous muscle has been introduced into the stomach. In about a month they attain a length of four lines, and during that period not only mature eggs and sperm-cells, but numerous embryos, resembling small filariæ, are developed, which leave the maternal body through the anterior sexual orifice, are found in the mesenteric glands, and rapidly invade the whole muscular system, dwelling within the sarcolemma, and feeding upon the contractile substance of the muscular fibres. They are found in all the striated muscles of the body, with the exception of the heart (Zenker states that he found a few in the heart of a rabbit fed with trichinæ); liver, lungs, kidney, &c., are free. In case the immigration is not sufficiently extensive to cause a fatal result, the trichinæ become enclosed in a capsule, which consist originally merely in a thickening of the sarcolemma, and this is the only condition in which they were formerly known. The trichinæ now shows the highest development it is able to attain within the muscle, into which it originally penetrated in an embryonic stage. It still retains its vitality, and quietly waits for an opportunity to find its way back into the intestinal canal, where, as Virchow's observations have shown, the two sexes attain the stage of puberty, and a wonderful productiveness, so pernicious to the individual who is unfortunate enough to harbour such terrible guests, is displayed. The same applies to the non-capsulated trichinæ. The flesh of the rabbit had to the naked eye a perfectly normal appearance. The trichinæ are not near as numerous in this case as they were on former occasions. The intestinal mucus contained mature trichinæ of both sexes.

Professor Zenker made a feeding experiment with a piece of the ham. No trichinæ were found in *the rabbit* a week after. These experiments will no

doubt be repeated, as the time elapsed was far too short to enable a definite conclusion to be formed.—*Edin. Medical Journal, Aug.* 1860, *p.* 173.

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120.—*Influence of Animal Food on the Colour of the Skin.*—M. d'Abbadie, the African traveller, writes to M. Quatrefages, that the flesh-eating blacks south of Nubia have a much lighter complexion than those nourished on an exclusively vegetable diet. It is also pointed out that the negroes in Kabylie who eat flesh have a very light complexion, while preserving the crisped hair and all the other characteristics of the negro race.—*Dublin Hospital Gazette, June* 15, 1860, *p.* 178.

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121.—*On Periodicity as a Character of Disease.*—By Dr. E. J. Tilt.—Believing that the periodical phenomena occurring in the system are to be referred to the ganglionic nerves, and that each menstrual process is analogous to a fit of ague, I was induced, many years ago, to try the best tonor of the vaso-motor nerves—sulphate of quinine, whenever the menstrual period recurred every second or third week instead of at the normal period, the fourth week. I have very seldom found the plan to fail, unless the change in the menstrual type was caused by severe inflammation of the neck of the womb. I have given the sulphate of quinine alone, in doses of from two to three grains every, or every other night, immediately after the subsidence of menstruation. The same dose of sulphate of quinine may be combined with two grains of extract of hyoscyamus, or a quarter of a grain of extract of opium, when nervous symptoms predominate, or with sulphate of iron if the patient be anæmic, or with extract of aloes in sufficient quantities to prevent constipation.—*Lancet, Aug.* 11, 1860, *p.* 149.

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## 122.—WHAT IS DISEASE?

By M. Claude Bernard, Member of the French Institute; Professor of General Physiology at the Faculty of Sciences.

[This celebrated Physiologist asks this interesting question in his 25th lecture published in the 'Medical Times and Gazette,' and endeavours to answer it by showing the effects of some poisons on the system.]

When we examine the lesions which result from the injection of toxic substances, we invariably discover that one peculiar histological element, one given tissue has been invaded in all the parts which compose the animal economy; and the direct consequence of such modifications is a total derangement of the physiological functions which, in the healthy state, devolve upon the injured elements. A general disturbance, therefore, arises from the suppression of their vital properties; and in this respect, the action of poisons is strictly parallel to that of internal diseases. Nor is this all; we are fully aware that the characteristic symptoms which attend the introduction of each particular substance into the system, enable the physician to establish as precise a diagnosis as in the case of any other affection. Are not the effects of arsenic, or those produced by the salts of lead, as easily distinguished from those of other poisons, as typhus fever or pneumonia from all other diseases? Now this is a property which has at all times been viewed as an essential characteristic of well-defined diseases; we give, in fact, this name to a definite succession of morbid phenomena, which arising from a given anatomical modification of tissue, undergoes a regular evolution, and terminates either in death, when the more important phenomena of life have been suspended, or in the patient's recovery when they have resumed their natural course.

*But a highly* plausible objection might now be raised against us; the effects of poison, it may be said, can be reproduced at all times upon a healthy

subject; while morbid agents are entirely beyond our control. Our reply to this argument is, that a variety of diseases may be created by artificial means, especially when a peculiar virus exists; the inoculation of small-pox and of venereal diseases sufficiently prove this; and we have previously shown that mere surgical lesions frequently enable us to create at pleasure in sound animals, a series of well-known disorders.

The astonishing rapidity with which the symptoms of poisoning often succeed the introduction of the toxic agent into the body, ought not to prevent us from assimilating them to the ordinary effects of disease; for the duration of the morbid series entirely depends upon the nature of the poison, and the state of concentration in which it is employed; if prussic acid, when pure, produces instant death, its destructive powers may be reduced by successive dilutions to the very lowest degree; and a large number of other poisons (metallic salts, for instance) act with comparative slowness, and may be assimilated to chronic affections, when taken in small doses.

The absence of all visible lesions after death, which not unfrequently occurs in cases where poison has been administered: and the possibility of an entire recovery, when the noxious substance has been completely expelled, may be adduced as additional proofs in favour of our views; and lastly, as a further resemblance to certain diseases, an incubation of a certain extent often takes place. The deleterious agent does not always exhibit at once its destructive properties, but remains during a certain lapse of time in a latent state (so to speak) within the economy. In short, the phenomena of life, whether viewed in connexion with physiology, pathology, or toxicology, obey the same natural laws, and ought not to be studied apart from each other. But in the case of poisons, the observer is placed in the most favourable conditions that can possibly be imagined: he administers definite substances in given quantities; he notes the effects produced from the very commencement of the consecutive disorder,—a condition which can never be realised in clinical practice; and lastly, he selects according to his convenience, the age, size, constitution, and species of the animal on which the experiment is performed: he therefore enjoys every possible security for the perfect success of the operation.—*Med. Times and Gazette, Sept.* 8, 1860. *p.* 225.

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## 123.—ON THE GENERAL EFFECTS OF MEDICINAL SUBSTANCES.

By M. Claude Bernard.

[In order to come to a rational explanation of the effects produced by foreign substances when introduced into the system, the first thing necessary is to regard the phenomena observed as ordinary physiological effects.]

In examining the various tissues which enter into the composition of the living frame, it is easily ascertained that peculiar and distinct properties are separately enjoyed by each of them; and that, in this manner, they are enabled to fulfill the various functions which devolve upon them. At the same time we discover that certain general systems exist, which by their distribution are brought into constant communication with all the various elementary tissues of the body; we have already named the vascular and nervous systems. The blood is brought through the capillaries to every part of the body, and its presence being indispensable to the physiological existence of all living tissues, they rapidly perish when accidentally deprived of this all-important and vivifying fluid. The blood, therefore, must be looked upon as the medium in which all our tissues have their being; it brings them in contact with an immense number of substances, derived from various sources, some of them drawn from the atmosphere through the process of respiration, others *contained* in the animal's food, and assimilated by the *digestive organs; others again* introduced into the economy through va-

rious absorbent surfaces, and all of them equally necessary to the continuance of the physiological conditions of life.

Such, however, are not the only bodies which the circulating fluid brings into contact with the primitive elements of which our organs are composed; the blood is, at the same time, the vehicle through which medicinal agents, intended to modify their properties, are conveyed into living tissues, and penetrate into their substance. We must, in consequence, consider medicines as physiological agents of an unusual nature; introduced into the system through the same channel as the bodies with which it is naturally supplied, they reveal their presence exactly in the same manner, viz., by giving birth to a series of phenomena, peculiar to each substance, and widely different from those which exist in the physiological state. But another question now presents itself; *how* are these foreign substances enabled to act upon our tissues? Not through their physico-chemical properties, as has been sufficiently proved in our last lecture; a *physiological* property must therefore be had recourse to, as in the case of all the powers which support life. When oxygen, for instance, is brought into contact with elementary, nervous, or muscular fibres, it quickens their activity, and keeps alive all their properties within them; and, when deprived of so powerful a stimulus, both nervous and muscular fibres quickly die. Similar actions are, no doubt, in their intimate essence, of a physical or chemical nature; but in the present state of our knowledge, the observer can only point to a physiological agency.

Histological elements, as we have already shown, are in every case endowed with special properties; and the wider is the difference which separates one tissue from another the higher is the animal's rank in the scale; in the nobler species peculiar organs are adapted to each individual function; and in no other animal is their number so great as in man. We are, therefore, justified in supposing that the primitive muscular fibre, for instance, is widely different, in every respect, from the nervous filament and the glandular cell; and, in consequence, that agents entirely powerless as regards all other tissues, may be found to exercise, over the muscles, an almost unbounded influence.

It is therefore our opinion that medicines, as well as poisons, exert their power exclusively upon certain histological elements, even in cases in which they appear to produce a general perturbation of the whole system. Thus, for instance, we find that strychnia localises its action entirely upon the sensitive nerves; and that wherever the torrent of the circulation conveys it, all the properties of the recurrent nervous fibres are destroyed: so that even before reaching the spinal cord it has already paralysed the extremities of the nerves which revert towards their central axis. On the other hand, woorara concentrates upon the motor nerves alone its noxious influence, and wherever it meets them, paralyses at once their properties; and in this manner, without doing the slightest injury to glands, muscles, vessels, or other tissues, it arrests at one blow the most indispensable functions of life. But the action of poisons introduced into the blood is not invariably directed towards the nervous system. All the other tissues, and even the blood itself, have their own peculiar poisons, capable of modifying their vital properties. Both digitalis, and the juice of the upas antiar, enjoy the power of destroying muscular contractility throughout the entire system. We find, however, a difference in the intensity with which these poisons act on the various orders of muscles; a difference with which the stimulating properties of electricity, which acts so powerfully upon certain systems of muscles, and are comparatively so feeble as regards others, has already rendered us familiar. But the nervous system, after the introduction of these poisons into the economy, remains wholly unimpaired; and through the suspended action of the heart alone is it in their power to produce death. A substance of a very different kind, the oxyde of carbon, neither attacks muscular nor nervous fibres. It acts exclusively on the blood-globules, which of course are to be viewed in the light of a living tissue, as well as other histological elements. Introduced into the lungs, the oxyde of carbon

fixes upon the globules which circulate in the pulmonary vessels; and such is its affinity for the blood-cells, that a perfectly stable chemical compound appears to be produced, which the presence of oxygen is no longer able to destroy. In this manner is the important function which devolves upon the globules abolished at once. How, in fact, would it be possible any longer for them to imbibe carbonic acid, and exchange it for oxygen in the capillaries of the lungs? The purposes of respiration being in this manner eluded, the blood-globules circulate in the vessels side by side with carbonic acid gas held in solution in the blood, without contributing in the slightest degree to the natural process of elimination which takes place in the lungs; and the animal at last dies in a peculiar state of asphyxia, the blood still retaining as before its ruddy arterial colour. Here, then, we find an influence of the most extensive and general kind, exerted over the whole economy through exclusively local means; a peculiar histological element having been modified, all the other tissues are deprived of an indispensable stimulus,—a fact which amply illustrates the mechanism through which the effects of poison ultimately pervade the whole frame: the toxic substance, circulating with the blood, fixes upon its favourite tissue wherever it exists. To adduce one more instance of this: the action of arsenic entirely resembles that of the substance we have just mentioned. It fixes also upon the blood-globules, but does not entirely suppress their action; it only tends to diminish the activity of the perpetual exchange carried on by their means between the oxygen and carbonic acid; a state of things which, when confined within proper limits, is not wholly incompatible with health, and, under such circumstances, produces, as a natural result, a certain degree of obesity.—(Schmidt.)

The action of medicinal substances must not be viewed in a different light; it resembles, in every respect, that of poisons. Nerves, muscles, blood-globules, and other tissues, enjoy peculiar vital properties, the intensity of which is modified through medical interference, sometimes for the purposes of increasing, sometimes for that of lowering, their activity; and the action of foreign bodies, whether medicinal or poisonous, can be rendered general only through the nervous or vascular system. But, in this respect, a wide distinction arises between sensitive and motor fibres, which experiments on living animals have placed in a strong light. Let woorara be injected into an animal's veins, or simply introduced under the skin after the vessels of one of its extremities have been tied, the motor nerves are paralysed throughout the system, and it becomes impossible to produce muscular contraction by acting upon them; but the nerves of that single limb, the vessels of which have been tied, alone retain their physiological power, and provoke contraction when galvanized or otherwise excited. It is therefore clear that motor nerves give merely birth to local phenomena, and are incapable of acting reciprocally upon each other through the central axis, in which they all unite. Far different is the mode of action of the sensitive fibres; let strychnia, or any other poison which holds them under its influence, be introduced into the circulating fluid, after tying, as before, the principal vessels of one limb, and it will be found that the operation has been performed in vain, convulsions being produced in that very limb, through mere nervous agency, as well as on other points. Supposing *all* the posterior roots which arise from the spinal cord to have been divided, no convulsions, of course, would be produced; but let a single root be left untouched, and through this single channel the necessary impulsion will be given; the blood which circulates throughout the system, and conveys the toxic substance into every part, brings it in contact with the extremities of that sensitive root, which alone remains uninjured, and still communicates with the spinal cord; and general effects are at once produced. How different, therefore, do we find the part played by sensitive and motor nerves to be!

It would therefore appear that the various substances which the physician employs exert their influence, not upon organs, but upon tissues; not upon the apparatus itself, taken as a whole, but upon *some* of the histological elements of *which it is composed.* It would be an entirely mistaken notion to

suppose that digitalis paralyses the heart; not upon the heart itself, but upon the muscular fibres which enter into its structure, is the action of that body directed, the nerves, vessels, &c. remaining, in the meantime, perfectly sound.

There is, besides, no room whatever for astonishment in the extraordinary energy which poisons display, when administered to the higher animals, while the lower classes remain infinitely less sensible to their action. A bird is struck dead, as if by lightning, by a dose of digitaline, which slowly destroys a mammiferous animal of an equal size, and is not sufficiently strong to kill a reptile. The fact is averred with regard to other toxic substances: histological elements are submitted, in fact, to the influence of external agents, in direct proportion to their vital energy.

But not only do the tissues of various animals differ with respect to the degree of sensibility which they exhibit under the action of foreign bodies; the proposition is equally true, when applied to homologous tissues, in the very same animal. Thus digitaline, long before it acts upon the contractile powers of other muscles, arrests the motions of the heart; a result which may, no doubt, be attributed to the ceaseless activity of the cardiac fibres: being therefore, of all muscles, the most energetic in the performance of their functions, they are naturally found to be, at the same time, those which experience long before the rest, the noxious influence of poisons. We are aware, besides, that a striking inequality is observed in the results obtained by stimulating various systems of muscles through different means: and that muscular fibres which act in obedience to the will, are infinitely more sensible to the galvanic stimulus than those which it is not in our power to contract at will.

The general effects obtained by the use of medicinal agents, are to be explained by the modifications which they produce in the primitive elements of which our tissues are composed.—*Medical Times and Gazette, May* 26, 1860, *p.* 515.

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## 124.—ON FERMENTED AND AERATED BREAD, AND THEIR COMPARATIVE DIETETIC VALUE.

By Dr. J. Dauglish.

[Since the introduction of the new process of preparing bread, by imparting to it a most perfect vesicular structure by artificial means, many have doubted whether the changes which the constituents of the flour, especially the starch, undergo during the ordinary fermentive process, are not essential to healthy digestion.]

In order to dispose of the assertion that starch requires to be prepared by the fermentive changes to render it fit for human food, it is but necessary to remark, that the proportion which the inhabitants of the earth, who thus prepare their starchy food, bear to those who do not, is quite insignificant. Indeed it would appear that the practice of fermenting the flour or meal of the cereal grains is followed chiefly by those nations who use a mixed animal and vegetable diet, while those who are wholly fed on the products of the vegetable kingdom reject the process of fermentation entirely. Thus, the millions of India and China, who feed chiefly on rice, take it for the most part simply boiled; and that large portion of the human race who feed on maize, prepare it in many ways, but they never ferment it. The same is true with the potato-eater of Ireland, and with the oatmeal-eater of Scotland. Nor do we find that even wheat is always subjected to fermentation; but the peculiar physical properties of this grain appear to have tasked man's ingenuity more than any other, to devise methods of preparing from it food which shall be both palatable and digestible. In the less civilized states, a favourite mode of dressing wheat grain has been, by first roasting and then grinding it. On the *borders of* the Mediterranean it is prepared in the form of maccaroni and *vermicelli, while in* the East it is made into hard thin cakes for the more delicate, *and for the* hardworking and robust into thicker and more dense masses of

baked flour and water. Even in our own nurseries wheaten flour is baked before it is prepared with milk for infants' food. The necessity of subjecting wheaten grain to these manipulations arises from its richness in gluten, and from the peculiar properties of that gluten. If a few wheaten grains are taken whole and thoroughly masticated, the starchy portions will be easily separated, mixed with the saliva and swallowed, whilst nearly the whole of the gluten will remain in the mouth in the form of a tough tenacious pellet, on which scarcely any impression can be made. A similar state of things will follow the mastication of flour. In this condition the gluten is extremely indigestible, since it cannot be penetrated by the digestive solvents, and they can only act upon its small external surface; hence the necessity to prepare food from wheat in such a manner as shall counteract this tendency to cohere and form tenacious masses. This is the object of baking the grain and the flour as before mentioned, of making it into maccaroni, and of raising it into soft spongy bread; by which latter means the gluten assumes a form somewhat analogous to the texture of the lungs, so that an enormous surface is secured for the action of the digestive juices; and this I believe is the sole object to be sought in the preparation of bread from wheaten flour.

Wheat is said to be the type of adult human food. It supplies, in just proportions, every element essential to the perfect nutrition of the human organism. And yet in practice we find that the food which we prepare from it, and furnish to the inhabitants of our large towns and cities, is quite incapable alone of sustaining the health and strength of any individual. This is the more remarkable, since in Scotland we find that the food prepared from the oat, a grain possessing the same elements of nutrition as wheat, though in a coarser form,—furnishes almost the exclusive diet of a very large number of the hardiest and finest portion of the population.

In the large towns of France wheaten bread certainly forms a very large proportion of the diet of the labouring classes, but not so large as oatmeal does in Scotland, And yet it has been remarked by contractors for public works on the Continent, that the chief reason why the Englishman is capable of accomplishing double the work of a Frenchman is, that the one consumes a very large proportion of meat, while the diet of the other is chiefly bread. In Scotland, however, the labouring man is capable of sustaining immense fatigue upon the nourishment afforded by oatmeal porridge.

The deficiency of wheaten bread in affording the nourishment due to the constituents of the grain, is to be attributed solely to the mode of preparing the flour, and the process followed for making that flour into porous bread.

The great object sought after both by the miller and the baker, is the production of a white and a light loaf. Experience has taught the miller that the flour which makes the whitest loaf is obtained from the centre of the grain; but that the flour which is the most economical, and contains the largest proportion of sound gluten, is that which is obtained from the external portions of the grain. But while he endeavours to secure both these portions for his flour, he takes the greatest care to avoid as much as possible, by fine dressing, &c., the mixture with them of any part of the true external coat which forms the bran, knowing that it will cause a most serious deficiency in the colour of the bread after fermentation.

It is generally supposed that the dark colour of brown bread—that is of bread made from whole wheated meal, is attributable to the coloured particles of the husk or outer covering of the grain. But such is not really the case. The coloured particles of the bran are of themselves only capable of imparting a somewhat orange colour to bread, which is shown to be the fact when whole wheaten meal is made into bread by a process where no fermentation or any chemical changes whatever are allowed to take place. Some few years since a process was invented in America for removing the outer seed coat of the wheat grain without injuring the grain itself, by which it was proposed to save that highly nutritious portion which is torn away, adhering to the bran in the ordinary process of grinding, and lost to human consumption. *The invention* was brought under the notice of the French

emperor, who caused some experiments to be made in one of the government bakeries to test its value. The experiments were perfectly satisfactory so far as the making of an extra quantity of white flour was concerned, but when this flour was subjected to the ordinary process of fermentation and made into bread, much to the astonishment of the parties conducting the experiments, and of the inventor himself, the bread was brown instead of white. The consequence, of course, has been that the invention has never been brought into practical operation.

It has been estimated that as much as ten or twelve per cent. of nutritious matter is separated adhering to the bran, which is torn away in the process of grinding, and until very lately this matter has been considered by chemists to be gluten. It has, however, been shown by M. Mège Mouries to be chiefly a vegetable ferment or metamorphic nitrogenous body, which he has named Cerealin, and another body, named vegetable Caseine.

Cerealin is soluble in water, and insoluble in alcohol. It may be obtained by washing bran, as procured from the miller, with cold water, in which it dissolves, and it may be precipitated from the aqueous solution by means of alcohol; but like pepsine, when thus precipitated it loses its activity as a solvent or ferment.

In its native state or in aqueous solution it acts as the most energetic ferment on starch, dextrine, and glucose, producing the lactic and even the butyric changes, but not the alcoholic.

It acts remarkably on gluten, especially when in presence of starch, dextrine, or glucose. The gluten is slightly decomposed at first, giving ammonia, a brown matter, and another production which causes the lactic acid change to take place in the starch and glucose. The lactic acid thus produced immediately combines its activity with that of the cerealin and the gluten is rapidly reduced to solution.

The activity of the cerealin is destroyed at a temperature of 140° Fah., according to M. Mouries, but my own experiments show that it is simply suspended even by the heat required to cook bread thoroughly; thus bread made without fermentation, of whole wheaten flour, or of flour in which there is a large proportion of cerealin, will, if kept at a temperature of about 75° to 85° Fah., pass rapidly into a state of solution, if the smallest exciting cause be present, such as ptyaline or pepsin, or even that very small amount of organic matter which is found in impure water—while the same material, when it has been subjected to the alcoholic fermentation, will not be affected in a like manner.

The activity of cerealin is very easily destroyed by most acids, also by the presence of alum; and while it is the most active agent known in producing the earlier changes in the constituents of the flour, it cannot produce the alcoholic, but as soon as the alcoholic is superinduced the cerealin becomes neutralised and ceases to act any longer as a solvent. M. Mouries, taking advantage of this effect of the alcoholic fermentation, has adopted a process by which he is enabled to separate from the bran all the cerealin and caseine which are attached to it. He subjects the bran to active alcoholic fermentation, which neutralises the activity of the cerealin, and at the same time separates the nutritious matter; and then having strained this through a fine sieve, he adds it to the white flour in the preparation of white bread, by which an economy of ten per cent. is effected, and the colour of the bread is not injured.

The peculiar action of cerealin as a special digestive solvent of the constituents of the flour—gluten, and starch—has been practically tested by Mr. Stephen Darby, of Leadenhall-street, in a series of careful experiments. He found that when two grains of dry cerealin were added to 500 grains of white flour, and the whole digested in half-an-ounce of water at a temperature of 90° for several hours, ten per cent. more of the gluten, and about five per cent. more of the starch, were dissolved than when the same quantity of flour was subjected to digestion without the addition of the cerealin, *but in which* of course there was the small amount of cerealin that is present *in all flours*. The action of cerealin upon the gluten of wheat is pre-

cisely similar to that of pepsine on the fibrine of meat. Pepsine acting alone on fibrine dissolves it, but very slowly, but if lactic acid be added solution takes place very rapidly. In like manner the starch present with the gluten of wheat is acted upon by the cerealin, and produces the necessary lactic acid to assist in the solution of the gluten by the cerealin.

With the knowledge thus obtained of the properties of this substance cerealin, it is not difficult to understand why the administration of bran-tea with the food of badly-nourished children, produces the remarkable results attributed to it by men both experienced and eminent in the medical profession; and why, also, bread made from whole wheaten meal, which contains all the cerealin of the grain, should prove so beneficial in some forms of malassimilation, notwithstanding the presence of the peculiarly indigestible and irritating substance forming the outer covering of the grain.

It will be seen that in all the methods of bread-making hitherto adopted, the peculiar solvent properties of this body, cerealin has been sought to be neutralised simply because it destroys the white colour of the bread during the earlier stages of panary fermentation. It is by thus destroying the activity of the special digestive ferment which nature has supplied for the due assimilation by the economy of the constituents of the wheaten grain, that wheaten bread is rendered incapable of affording that sustenance to the labouring man which the Scotchman obtains from his oatmeal porridge. Although the new bread has been as yet but little more than experimentally introduced to public consumption, I have already received from members of my own profession, who have recommended it in their practice, as well as from non-professional persons, accounts of the really astonishing results that have followed its use in cases of deranged digestion and assimilation. Private gentlemen have sought interviews with me to record the history of their recovery to health, after years of suffering and misery, by the simple use of the bread as a diet. Children that have been liable to convulsive attacks from an irritable condition of the alimentary canal and nervous system, have been perfectly free from them immediately the new bread was substituted for fermented bread. And cases are now numerous that have been communicated to me by Medical men of position, in which certain distressing forms of dyspepsia, which had remained intractable under every kind of treatment, have yielded as if by magic almost immediately after adopting the use of the aerated bread.

The delicate flavour of the new bread renders it peculiarly grateful to the stomachs of invalids and children, as well as of those whose tastes have not become vitiated by the habitual use of baker's bread, which is slightly sour, and tastes of yeast. The new bread was supplied to two wards in Guy's Hospital in place of the ordinary bread (which is of a very fine quality, made on the premises,) for two months, and in no case were there any pieces left in the wards unconsumed, while of the fermented bread large quantities of scraps are collected daily, for the consumption of which the appetites of the patients have been deficient.

That persons who have been long used to the strong yeasty-flavoured bakers' bread should consider the new bread tasteless at first is not to be wondered at, since the delicate sense of taste is of all other senses the most easily lost by rough usage. Hence the argument put forth in defence of adulteration by some London tradesmen, especially the beer sellers, that the public will not buy the pure article, as it is wanting in the flavour to which they have been accustomed; and hence, also, the dislike of the Viennese of the fresh oysters supplied to them when the railway was completed, as they deemed them insipid, after the habitual use of oysters slightly decomposed, with which they had been supplied when it required a lengthened period to transport them from the sea.

I am disposed to attribute the beneficial effects of the new bread to two causes. The one to the *absence* of the prejudicial matters imparted to ordinary bread by the process of fermentation, and the other to the presence in the bread, unchanged, of that most essential agent of digestion and assimilation, *cerealin*.

I believe the prejudicial matters imparted to bread by fermentation to be chiefly two—acetic acid and the yeast-plant. The first is produced in large quantities, especially in hot weather, by the oxydation, by atmospheric contact, of the alcohol produced. The second is added when the baker forms his spunge, and is also rapidly propagated during the alcoholic fermentation, and cannot of course be afterwards separated from the other materials in the manner that the yeast and the other *débris* of fermentation separate themselves from wine and beer by precipitation in the process of fining. Nor is the life of the yeast-plant generally destroyed in baking, because it requires to be retained at the boiling point for some time before it is thoroughly destroyed; and bread is generally withdrawn from the oven, for economical reasons, even before the centre of the loaf has reached the temperature of 212°. It is not difficult to understand how the most painful and distressing symptoms and derangements may follow the use of bread in which the yeast-plant is not thoroughly destroyed previous to ingestion, in those cases of impaired function in which the peculiar antiseptic influence of the stomachal secretions is deficient, and is incapable of preventing the development of the yeast-plant in the stomach, and the setting up of the alcoholic fermentation to derange the whole process of digestion and assimilation.

The presence of cerealin in bread is as beneficial as that of acetic acid and the yeast-plant is prejudicial. Digestion, or the reduction of food, is evidently essentially dependent on the action of a class of substances which chemists, for want of a better term, have called ferments,—to these substances belong pepsine, ptyaline, emulsion, diastase, and cerealin; these are evidently types of a very numerous class, which act by producing those molecular changes in organic substances in which digestion consists; and since the purpose of digestion or solution is to prepare from the heterogeneous substances taken as food a chyle, which shall not only when absorbed present all the elements of healthy blood, but shall previous to absorption, possess the properties which will constitute it the proper stimulus to the functional activity of the lacteals, it would appear to be necessary that each distinct substance taken as food, should be furnished, not with its simple chemical solvent, but with that peculiar form of solvent or ferment which alone can carry it through those molecular changes which shall terminate in the production of healthy chyle. Hence, we should infer that a substance is digestible or indigestible just in proportion to the provision that is made for its reduction to the standard of healthy chyle, and that substances which have hitherto been incapable of affording any nutrition whatever, may at some future day be rendered highly nutritious, simply by adding to them suitable ferments, artificially obtained or otherwise, that shall secure their passing through the proper molecular changes. Indeed, I think this subject opens up to us that very wide field of inquiry, as to whether the cause and prevention of disease, and the beneficial administration of remedies may not, for the most part, if not entirely, be dependent on the action of substances analagous to such bodies as ptyaline, pepsine, cerealin, &c., acting in concord with, or retarding and opposing the vital functions of tissues; and that by more profound inquiry in this field of research, the physiologist and the pathologist may not at a future day lay the foundation of true scientific Medicine.—*Med. Times and Gazette, May* 12, 1860, p. 468.

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## 125.—ON THE NATURE OF DEATH FROM CHLOROFORM.

There have been about 125 deaths from anæsthetics in Europe up to the present date. A very large number, the only list of any such cases on the Continent, is one by M. Scoutetten, an Army Surgeon, who gives 40 deaths, but unfortunately does not specify the nature of the operation for which the *anæsthetic* was administered. Dr. Snow gives 50 deaths, and I have since *myself made out* 35 deaths in addition. All agree as to the vast preponderance, however, of deaths in male patients rather than in females; it also ap-

pears that while there have been in reality no deaths as the result of large amputations, resections, ovariotomy, &c., fully two-thirds of the deaths have been from operations on sphincters or tendinous sheaths, strabismus, tooth-drawing, &c.

| | Nature of operation causing Death. | Snow. | Kidd. |
|---|---|---|---|
| 17 | Deaths during operations on fistulæ, hemorrhoids, applications of caustics, &c. | 13 | 4 |
| 14 | Deaths in removal of toe-nail, dead finger, and such operations on phalanges | 9 | 5 |
| 10 | Dentists' cases of tooth-drawing, &c. | 6 | 4 |
| 11 | Deaths from removal of tumours | 7 | 4 |
| | Resections, large amputations, midwifery, ovariotomy, ligatures of large arteries, &c. | — | — |
| 9 | Deaths in minor amputations (but 6 occurring before the operation commenced) | 4 | 5 |
| 6 | Deaths in strabismus, operations on eye, &c., cysts of eyelids, &c. | 2 | 4 |
| 9 | Deaths in operations on testis, sounding, &c. (2 included doubtful in 218 lithotomy cases) | 6 | 3 |
| 5 | Deaths from reduction of dislocations | 2 | 3 |
| 1 | Death from strangulated hernia | 1 | — |
| 3 | Deaths from delirium tremens | — | 3 |
| | | 50 | 35 |

Scoutetten's cases (not specified) .. .. 40

This table explains itself; the general result is very curious, and I think very convincing, that in the hundreds of thousands of operations where chloroform has now been used, it has proved in the vast majority to be perfectly safe, and that it is safer in large than in small operations. Why it is so, I may not now stop to enquire. Again, of sixteen deaths under chloroform, where I was myself able to trace out the probable cause of the fatal issue, I found three where some remote disease of the heart might be suspected; but in thirteen there had been probable, and in some very serious derangement of the nervous system—viz., three deaths from delirium tremens and chloroform combined, two where chorea and meningitis were present, two from cysts in the brain, and four from other lesions of cerebral tissue. It seems too evident, I think, that if syncope or "fit" should occur in any such case, resuscitation is rendered difficult, as we have an imperfect brain and spinal apparatus to work with. Disease of the heart is a very rare cause of death from chloroform; the facts on which this hypothesis was built up are now shown to be all erroneous.

There is a sad want of uniformity in the mode of making post mortem examinations in our London hospitals. In two necropsies in the Borough hospitals, and one at the Royal Ophthalmic, Moorfields, I was present, and anxious to see the state of the heart and pericardium before the latter was opened; in two I was successful, but in the third the head was opened first, and a large drainage of fluid blood took place from the jugular veins, sinuses, &c., which left the heart flaccid. Even in one of the other cases, where in good routine fashion the hospital porter opened roughly all the three cavities at once, and left them all open for an hour, awaiting some "crowner's quest" or magisterial inquisition, it was not less difficult to come to any steady or useful decision on the case. I observed, however, while waiting this hour, that the right cavities of the heart were very much distended with fluid blood; the pericardial contents and pericardium bulging very prominently, "filled to bursting almost," till the examination of the lungs commenced, when the section of the large vessels of course emptied the heart; the heart being then examined was marked "perfectly healthy—and empty." It seems to be not a bad mode of proceeding to open all the cavities together, but our great want in the hospitals is some uniformity; and in a very painful degree some uniformity in the examination of dead bodies after accidents from chloroform. Some other remarkable facts have been discovered during the course of these investigations, as to death from chloroform, which it is of

interest to note. First, that a very large number of the twenty-five deaths from the use of anæsthetics after operations have been deaths from the effects of the slow admininstration of ether, or ether and chloroform mixed, but not from pure chloroform. I published nineteen deaths from ether two years ago, yet it is still suggested in America that there have been no deaths from this agent. Scoutetten gives five; and since then there have been at least four or five more from ether or amylene; so that, on the whole,

| | Snow. | Scoutetten. | Kidd. | Total. |
|---|---|---|---|---|
| In 121 deaths, the relative dangers of the chloroform "Stages" were: | | | | |
| Deaths when chloroform was given for intended operations, or immediately before operations..... | 18 | 22 | 14 | 54 |
| Deaths during the progress of operations..... | 22 | 6 | 14 | 42 |
| Deaths after operation—*i.e.* from chloroform immediately after, or the result of chloroform and the operation combined a short time after the operation had been completed..... ..... | 6 | 12 | 7 | 25 |
| In 133 deaths, the relative frequency of deaths as to Sex, was:— | | | | |
| Males..... | 30 | 32 | 28 | 90 |
| Females..... | 20 | 16 | 7 | 43 |

there appears at present, about a hundred deaths from pure chloroform, and one-fourth (or twenty-five) from ether or amylene! Next, as to the stage or degree of the anæsthetic process which appears most dangerous, this (contradictory though it seems) is decidedly the stage of excitement, or the early stage of violent plunging, before the patient is rendered anæsthetic and fit for the operation he is about to undergo. This escaped the knowledge of Dr. Snow.

I think it will be found that the mode of death by "fits," trachelismus, syncope, &c., observed before operations, occurring suddenly, and consequently the means to be adopted for resuscitating patients in such accidents, differ very widely from the mode of death observed and means to be adopted for resuscitation of patients who show a tendency to sink after operations (by asphyxia?). We have hitherto confounded the two, and have been looking for some one great secret cause of these most melancholy occurrences. One set of accidents seem to occur suddenly from chloroform; the other slowly, but from ether. It appears also very instructive and consoling that in all the large tedious operations,—such as resections, large amputations, midwifery, ovariotomy, ligatures of large arteries, &c.,—such is the "law of tolerance" of chloroform, that in these formidable operations it is almost free from danger; whereas, in trivial operations, (where, fortunately, we might use Dr. Arnott's congelation plan) the chances of accident are very serious indeed.—*Med. Times and Gazette, May* 12, 1860, *p.* 482.

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## 126.—ON THE INFLUENCE OF BELLADONNA ON THE PNEUMOGASTRIC NERVE.

By Richard Hughes, Esq., M.R.C.S., and L.R.C.P.Ed. (Exam.), Surgeon to the Brighton Orthopædic Hospital.

Dryness of the throat and dysphagia are two of the earliest and most invariable symptoms of the influence of belladonna. On what do they depend?

The pneumogastric nerve appears to preside over alike the muscular fibre and the secreting apparatus of the alimentary canal from the fauces to the *stomach*. Claude Bernard has ascertained the result of its division upon *the stomach*. "The section was made during the free flow of gastric juice *(though a canula previously introduced into the stomach)*, excited by the

presence of an alimentary bolus. The flow immediately ceased, and the mucous membrane, which had been tense and turgid the moment before, became withered and pale. On introducing the finger into the stomach itself, the walls were perceived to be perfectly flaccid, and there was no longer the gentle pressure which had been previously felt." (Carpenter's Human Physiology, 5th ed., p. 87.) It stands to reason that, if such be the effect of loss of pneumogastric influence on the stomach, similar phenomena—check of secretion and diminished muscular power—higher up in the alimentary canal, must be referred to depression of the influence of the same nerve. Consequently the dryness of throat and dysphagia caused by belladonna must result from a depressant influence exercised by it on the pneumogastric nerve.

That belladonna has such an influence will farther appear from a consideration of its effects on disease. Hooping-cough and asthma are admittedly spasmodic affections, in which irritation of the pneumogastric nerve is at the bottom of the phenomena; and both of these affections are singularly under the control of belladonna and its congeners. Stramonium is the favourite remedy in asthma; and the use of belladonna in hooping-cough is becoming more and more general. In the former case, we have the evidence of direct experiment for our theory. Galvanisation of the pneumogastric has been found by Valentin to produce constriction of the trachea and bronchial tubes; while on the other hand, in animals poisoned by belladonna, and stramonium, these tubes have been found lax, and have refused to contract under the strongest stimuli (Watson's Lectures on Physic, 4th ed., vol. ii, p. 358, 363). Lately, moreover, an enterprising French surgeon has attempted and (apparently) achieved the cure of asthma by injecting a solution of atropine upon the pneumogastric nerve in the neck (Medical Times and Gazette, Nov. 26, 1859). Another affection in which belladonna has been found very beneficial, and in which the pneumogastric is the seat of irritation, is obstinate spasmodic vomiting. A patient was suffering from cancer of the pylorus; the usual incessant vomiting took place. Belladonna was given, and it ceased. After death the stomach was found lax and enormously distended. Mr. Amesbury tells me he once had a case of obstinate vomiting in pregnancy. All the usual remedies failed; at length he tried belladonna, with complete success. Here again, as the pneumogastric is the motor nerve of the muscular coat of the stomach, a sedative influence exercised by the belladonna upon this nerve will explain the phenomena.

The practical result from the above facts will be, that we shall be led *rationally* to the use of belladonna and its congeners in all affections of the pneumogastric nerve. Laryngismus stridulus must therefore be added to the above mentioned morbid conditions. I have lately used belladonna (in combination with nitric acid) in every case of common cough which has come under my notice, and with far more marked success than I have obtained from any other remedy. I trust that a more extended trial may add fresh support to the view here advocated.—*Brit. Med. Journal, May* 26, 1860, *p.* 393.

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127.—*Opium and Belladonna Plasters.*—Mr. A. F. HASELDEN, having observed the inconveniences attending the use of these plasters of the Pharmacopœia, of which most medical practitioners are very sensible, (viz, that the preparation runs beyond the margin of the leather, or frequently penetrates it, and stains unpleasantly the clothing,) proposes, in a recent number of the 'Pharmaceutical Journal,' methods of obviating them. He recommends the use of resin plaster instead of gum thus, and morphia in place of opium. For the opium plaster, he proposes the following formula, which has been successfully employed:—Lead plaster, six ounces; resin plaster, four ounces; acetate of morphia, one drachm; acetic acid and water, of each four drachms. For Belladonna plaster, the following:—Lead plaster, six ounces; resin plaster, four ounces; atropine, two grains; acetic acid, two drachms; water, *six drachms.—Med. Times and Gazette*, May 12, 1860, p. 482.

## 128.—CASES ILLUSTRATIVE OF THE INFLUENCE OF BELLADONNA.

By Richard Hughes, Esq., M.R.C.S., L.R.C.P. Ed., (Exam.) Surgeon to the Brighton Orthopædic Hospital.

[Dr. Hughes in two previous papers, one in the 'British Medical Journal' for June 2, 1860, and the other in the 'London Medical Review' for August, 1860, enters into the physiological effects of this medicine. He thinks that it is an *excitant of the sympathetic* and a *depressant of the cerebro-spinal system.*]

In the following paper, says he, I propose to exhibit some results of the practical application of this theory, and to point out the farther indications for practice thereby afforded. And I will begin with cases where the action of this drug upon the sympathetic system appears to be the *rationale* of its curative influence.

*Case* 1.—*Enuresis.*—July 1859. George M., aged 14, had long suffered from nocturnal incontinence of urine. There was nothing particular about the general appearance of the patient, or that of his urine. I ordered small doses of tincture of cantharides, which relieved him temporarily; but immediate relapse followed its discontinuance. I now prescribed one-third of a grain of extract of belladonna twice a day, in cinnamon-water. In three days his complaint left him; a slight relapse, about a week subsequently, was immediately removed by a few more doses of the medicine; and he has since been entirely free of his ailment.

*Remarks.*—Incontinence of urine may obviously result from two immediate causes. 1. From over-action of the longitudinal fibres of the muscular coat of the bladder (detrusor urinæ.) This is usually excited by some irritating quality of the urine, and is best removed by alkaline medicines and opiate suppositories. 2. From weakness of paralysis of its sphincter. This second and far more common cause of enuresis, usually met with in delicate children, requires for its cure some agent capable of toning the sphincter muscle, either immediately, or through the medium of its nerves. Such an agent we have, according to my views, in belladonna; and, accordingly, the accounts of the influence of this drug over the disease from all parts of Great Britain and the Continent are highly encouraging. It rarely fails to overcome it, and in its use is subject to none of the evils occasionally produced by cantharides.

In prescribing this drug for children, the discovery of Dr. Fuller as to its comparatively slight effects on young subjects must be borne in mind; and what would seem to be large doses must be unhesitatingly used.

*Case* 2.—*Milk Abscess.*—July 1860, Emily H., aged 24, nine weeks after confinement caught cold. When I saw her, I found at the upper part of the left breast a hard swelling elevated above the surface, of about the size of a hen's egg; it was very painful, red, hot, and tender to the touch. There was also pain in the right side, constipation, headache, high fever; and, in the course of the preceding night, there had been some delirium. The fever had somewhat abated since vomiting a quantity of pure bile early in the morning. These symptoms pointed to concomitant derangement of the liver, and soon yielded to a mercurial pill and saline draught. The state of the breast I treated by suspending the mamma by a handkerchief tied over the shoulder, and keeping the swelling covered with the unguentum belladonnæ of the *Pharmacopœia.* In twelve hours, all pain had ceased, and there had been no increase of swelling. Next day, the redness and heat had gone, some tenderness only remaining. The swelling remained stationary and slightly tender for a few days, then slowly disappeared. The infant was not put to the breast during the mother's illness, and the general fever was sufficient to diminish the secretion of milk; but, as soon she became convalescent, the flow of milk in both breasts returned as freely as before.

*Remarks.*—The localised inflammation of the breast to which (from its great tendency to suppuration) this term "milk abscess" is commonly ap-

plied, has now been frequently treated by belladonna. This drug has been ascertained to possess galactifuge properties; and accordingly, being applied in the form of extract or ointment around the nipple in these cases it speedily checks the secretion of milk, and with it the inflammation.

The galactifuge properties of belladonna are easily explicable upon the theory of its action which I have advanced. Cl. Bernard has found that there are two ways of diminishing the secreting power of a gland—1, galvanising the sympathetic nerves which supply its bloodvessels; 2, dividing the cerebro-spinal nerves which supply its secreting cells. Belladonna, according to my view of it, both excites the sympathetic (as by galvanism), and depresses the cerebro-spinal influence (as by section) of the parts which it affects. The result of its application to a gland like the breast will therefore be to diminish alike its secreting power and the supply of the material on which that power is exercised. And this galactifuge property of the drug will be found invaluable in cases of still-birth, premature weaning, and others, where the check of secretion of milk is loudly called for.

But it will be noticed that, in the case I have recorded, there was no check of the secretion of milk. Not to speak of the superior practical advantages of a plan of treatment which avoids this unpleasant concomitant, it is obvious that the *rationale* of the cure must be a different one. It seems to me to resolve itself into the simple action of belladonna in exciting the vaso-motor nerves, and thereby contracting the capillaries. The essence of the inflammatory condition, to my thinking, is a morbidly dilated state of these vessels. The exudation from them, on which Professor Hughes Bennet and his school lay so much stress, is surely a consequence only of their preternatural dilatation and fulness. A substance which will contract them will save them the necessity of relieving themselves by exudation at all, and, even when this has taken place, will most surely promote its absorption. And this was the view of John Hunter. "If," he wrote in 1794, "we could discover an agent which would cause the contraction of the extreme vessels, we should have in this the true remedy for inflammation."

This case, then, of cure of threatened milk-abscess by the local application of belladonna, appears to open a wide field for the treatment of inflammation in general. The three indications which inflammation gives us for treatment are—1. To moderate the force and frequency of the heart's action: 2. To diminish the superfibrination of the blood; 3. To reduce the hyperæmia of the part affected. The old mode of effecting the first two objects—in other words of relieving the symptomatic inflammatory fever—was to bleed, and to give mercury; which may fairly be described as robbing a patient of a portion of his life, and poisoning the remainder. A more enlightened practice fulfils the former indication by antimony, aconite, and the other substances which have this physiological effect upon the heart; and the latter by low diet, diluents, and salines. The third indication—the reduction of the hyperæmia of the part—is commonly met by local bleeding and cold. But these are measures only applicable in their fullness to external inflammations, and even there are open to much improvement. Local bleeding, however efficacious, wastes blood, and leaves the capillaries weak and still morbidly dilated; and cold must be continuously applied, to be of avail. We should gain immensely, could we find a remedy capable of acting, whether locally or through the general system, upon the nerves of the inflamed part, so that its dilated capillaries might be aided to that contraction for which Nature is striving, and thus the hyperæmia of the part reduced without the loss of a drop of blood, or the inconveniences inseparable from leeches, cupping, and the continuous application of cold.

Such an agent I believe we have in belladonna; and I augur an immense advance in our control over inflammation as its use therein becomes extended within the limits which practical experience alone can define. (See the Lancet for Aug. 4, 1860, for a case of myelitis treated by belladonna applied along the spine.)

*Case* 3.—*Congestive Headache.*—August, 1860. Mrs. M.; aged 74, for four days *in succession had been* visited with severe headache, beginning

with blindness, and lasting the greater part of the day. Similar headaches, some years ago, had been relieved by leeching. The administration of belladonna was commenced at the first coming on of the blindness, on the fifth day. The attack this day passed off sooner than usual. On the sixth day, there was but a slight attack; and since then there have been none.

*Remarks.*—The remarks made upon the last case will in most respects apply here, substituting congestion for inflammation. The congestive character of the headaches seemed marked—1, by the accompanying blindness; 2, by the relief formerly afforded by leeches; 3, by the absence of good effect from a full dose of Hoffman's anodyne—a medicine which I find invaluable in cases of simple nervous headache. Here the belladonna accomplished the same good end as the leeches, without their expense, inconvenience, or spoliation of the vital fluid.

The result of this case would seem to indicate the use of belladonna to remove that habitual tendency to congestion of the brain which usually results in apoplexy.—*British Medical Journal, Sept.* 8, 1860, *p.* 706.

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129.—*Influence of Fatty Bodies on the Solubility of Arsenious Acid.* By M. BLONDLOT.—The remarkable fact, to which the author calls the attention of toxicologists, is the property which fatty bodies possess of presenting an obstacle to the solubility of arsenious acid, either in plain water, or in water slightly acid or alkaline. A large number of experiments have proved to him that the least contact of arsenious acid, in the concrete state, with a fatty body, is sufficient to reduce its solubility in these different menstrua to one-fifteenth or one-twentieth of what it would be, *cæteris paribus*, without the intervention of the fatty principle,—a fact which may easily be ascertained by testing the proportion of arsenic dissolved by means of starch and tincture of iodine. Since a mere trace of any fat whatever is sufficient to produce this effect, and since neither acids nor energetic bases oppose any obstacle to it, it is evident that there is not in this case any chemical combination between the arsenious acid and the fatty body; and that, therefore, the latter can only interfere mechanically by soaking the arsenious acid, so as to remove it from the action of the aqueous fluids, which would dissolve it. This fact, in itself so simple, is susceptible of numerous applications in toxicology. It explains, in the first place, how it has happened that, in medico-legal analyses, arsenic has sometimes been sought for in vain in the liquid portion of aliments which contained it, when they were more or less fatty, such as soup, milk, &c. It also gives the reason why arsenic, taken in the form of powder, becoming mixed in the stomach with fatty substances, which retard its solution, has occasionally remained a very long time without producing poisonous effects,—a circumstance which might, in certain cases, mislead the investigations of justice. In the same way we may account for a very significant fact related by Morgagni, that, in his time, it was not very uncommon to see jugglers swallow, with impunity, pinches of arsenic, because, he adds, they had taken the precaution of swallowing beforehand milk and fatty substances, which they brought up again by vomiting after the public had retired. Finally, these experiments show the benefits which may be derived, in this kind of poisoning, from the administration of fatty bodies, especially of milk, which not only have the advantage of acting as emolliments, according to the general belief, but which are veritable antidotes, capable of retarding considerably the solution, and consequently the absorption, of the arsenious acid, which as often happens, still remained in the concrete condition.—*Acad. des Sciences.*—*Edinburgh Medical Journal, Aug.* 1860, *p.* 176.

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130.—*On Peroxide of Hydrogen.* By Dr. RICHARDSON.—Dr. Richard-

son has been employed in this research for twelve months, and his observations in relation to the nature of the body in question, its formation for therapeutical uses, and its effects on animals, are singularly interesting. For instance, he showed by experiment that the oxidizing power of the solution of peroxide is suspended by the presence of all narcotics; thus establishing the great law advanced by Snow, that narcotism is suspended oxidation, and that every substance, which on being introduced into animals produces narcotization, has the property, either by a negative influence, or by catalysis, of preventing the union of oxygen with other substances with which it is in contact, and for which it has an affinity. Again, it was shown that some animal structures, to say nothing of certain inorganic bodies, on being brought into contact with the peroxide in solution, liberate the oxygen. Fibrine has this property, and carbonic acid. A fish placed in the solution evolved the oxygen with considerable action. Physiologically, the peroxide, on addition to venous blood, gives to the blood the arterial character; it stimulates the left side of the heart to contraction, but seems to stop the action of the right side. Injected into the arteries, it restores for a time, a condition of muscle during which contraction occurs on the application of an excitant. It also suspends cadaveric rigidity, and further, it prevents the spasms of muscle caused by such bodies as ammonia and hydrocyanic acid. Therapeutically, peroxide of hydrogen offers itself in all cases marked by deficient oxidation; in low fevers; as an antidote to various poisons; in tetanus; in diabetes, and in cancer. It is given compatibly with all the mineral acids; and the doses of a solution charged with ten volumes of the peroxide is from one increasing to four fluid drachms, in distilled water; externally, it forms a deodorizing lotion. Dr. Richardson took pains in his paper to explain the exact mode of making the solution; but we feel that the process as yet is too complicated for general introduction into the dispensary. To bring the substance into general use, some practical Pharmaceutical Chemist must take it in hand. We are informed, indeed, that Bullock and Reynolds are already preparing the peroxide solution, and as several practitioners are only waiting for the proposed remedy to test its effects, we may expect soon to see its virtues fully brought out in the treatment of disease. It is just to say that the introducer of the remedy specially guarded himself from offering any extreme views: he claimed simply that a substance possessing such singular properties, physiologically, should be used rationally as a medicine in extreme cases for which we now have, virtually, no means at command.—*Med. Times and Gazette, Oct.* 20, 1860, *p.* 382.

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## 131.—ON THE SIGNIFICANCE OF THE CONTRACTION AND DILATATION OF THE PUPIL PRODUCED BY OPIUM AND BELLADONNA RESPECTIVELY.

By Richard Hughes, Esq., M.R.C.S., L.R.C.P., Ed. (Exam.) Surgeon to the Brighton Orthopædic Hospital.

It is well known that the muscular fibres of the iris, by whose contractions the diameter of the pupil is enlarged or diminished, are arranged in two sets; a circular set, immediately around the margin of the aperture; and a more external longitudinal set, radiating from the centre to the circumference of the iris. It is obvious that while the contraction of the circular fibres will diminish the size of the pupil, that of the longitudinal fibres will enlarge it.

It is also now ascertained that these two sets of fibres are animated by two distinct nerves, emanating from different divisions of the nervous system. The circular fibres receive a branch of the third cranial nerve, and are therefore under the influence of the cerebro-spinal system; while the longitudinal fibres are supplied by filaments from the cervical ganglia of the sympathetic.

In consequence *of this duplex* muscular and nervous arrangement, the di-

ameter of the pupil is always under ordinary circumstances, the same. So long as the normal influence of both third and sympathetic nerves is kept up, an equilibrium is maintained. On the other hand, let either of these channels of nervous supply, or their sources be injured or depressed, and the balance is lost. The fibres animated by the unarmed nerve will obtain a predominance over the others, and passive contraction or dilatation will ensue, analogous to that which obtains in the lower extremities when, the flexors or extensors of the limb being paralysed, deformity is produced by the unbalanced influence of the opposing muscles. A similar loss of balance will follow the excitation of either of the animating nerves. The active supply of nerve force set on foot by the stimulus will overcome the influence of the mere passive conveyance kept up by the unexcited nerve, and active contraction of the fibres supplied by the other will be the result. Examples of the former mode of destruction of equilibrium are—

Contraction of the pupil from section of the sympathetic in the neck.

Dilatation of the pupil from diminished power of the third nerve in compression of the brain.

Of the latter mode—

Contraction of the pupil from reflex excitement of the third nerve produced by the stimulus of strong light acting through the optic nerve.

Dilatation of the pupil from galvanic stimulation of the sympathetic in the neck.

It follows from the above facts and considerations that the state of the pupil will at any time (except in cases of local injury) be an index to the condition of one or both of the two great divisions of the nervous system. The only question to be asked in each case is—have we here abnormal depression of one system, or abnormal excitation of the other? And the accompanying symptoms will generally enable us to answer this inquiry.

Reserving for a future paper the consideration of the states of the pupil in disease, I shall here inquire into the rationale of its appearances under the influence of certain drugs.

The state of the pupil is a marked feature of the influence of opium on the one hand and of belladonna and its congeners (hyoscyamus and stramonium) on the other. The latter produce dilatation of the pupil, whence they are called mydriatics: the former causes as decided contraction.

The question arises—Is this a casual difference, or is it an index to an essential diversity between the two classes of substances? By farther considering their respective actions, we shall see the latter to be the true statement of the facts.

1. Opium is a narcotic, *i.e.* produces cerebral unconsciousness going on, when large doses are taken, to coma. The mydriatics have no such effect; on the contrary, one of their poisonous results is delirium.

2. Both are anodyne; but this influence of belladonna is only seen in external neuralgiæ, while that of opium extends over the whole class of internal pains. For instance, we should not give belladonna in peritonitis, nor opium in tic-douloureux.

3. Opium checks all the secretions, save that of the skin, and constipates. The mydriatics in medicinal doses leave the secretions untouched, and hyoscyamus and belladonna (if not stramonium also) are gently aperient.

4. Young children, as is well known, are exceedingly intolerant of opium; while, as Dr. Fuller has shown (Medical Times and Gazette, July 23, 1859), the younger the patient, the larger in proportion is the dose of belladonna required to produce its physiological effects.

Actuated by such considerations as these, it has been lately recommended to give opium as an antidote in poisoning by belladonna, and *vice versa*. I have seen no account of the use of belladonna in opium poisoning; but of the former practice we have at least one most successful set of cases. In the 'Medical Times and Gazette' of December 3, 1859, Mr. Seaton, of Leeds, *records* the history of ten persons who partook of the berries of the atropa *belladonna*. In two of these cases the symptoms were slight, and an emetic *was the only* thing required. In the eight others, the full poisonous effects

of the drug were obtained, especially active delirium and wakefulness. Opium was given to all; and in seven of them the symptoms disappeared as soon as sleep and contraction of the pupil indicated its influence over the system. The eighth patient, who was in bad health at the time and took but little opium, died.

I believe that these facts ought to be more widely known, and put into practice, that facts may test what theory thus indicates, that opium and belladonna are directly antagonistic in their effects, and are, therefore, antidotes one to another.

Careful investigations and experiments are required to ascertain how far belladonna may be given to relieve symptoms which arise from opium poisoning, even cases where they are otherwise caused, and *vice versa*. We know that the cerebral excitement of delirium tremens, which is precisely analogous to that produced by belladonna (and in which the pupil is ordinarily dilated), is completely under the control of opium. May not congestive headache, and coma with contracted pupil—however arising—be indications for the use of belladonna?

But we must be cautious here, and not proceed to give these drugs in fevers and other cerebral affections, purely on the evidence of the state of the pupil. As Dr. Pereira has remarked, all practical experience is against giving opium in certain affections (*e.g.*, compression of the brain) where nevertheless the pupil is dilated.

An interesting physiological enquiry seems suggested by the question, as to what is the process by which opium contracts, and belladonna dilates, the pupil?

We have seen that contraction of the pupil may be produced either by excitation of the third nerve or its source, or by depression of the sympathetic nerve or its source. It is *a priori* very unlikely that opium, whose full effect is depressant to the whole cerebro-spinal system, should act as an excitement to one nerve proceeding from that system. It is most probable, therefore, that opium contracts the pupil by such a depressant influence on the sympathetic nerve as that the third, still excited by the stimulus of light through the optic nerve, or even without this, is more than a balance for it.

This *a priori* probability must now be brought to the test of the observed facts of the action of opium. Will depression of the sympathetic influence explain these? I believe it will in nearly every instance.

We may enumerate its effects as follows:—1. Contraction of the pupil. 2. Congestion of the brain and other internal organs; but pallor of the skin. 3. Diminution of all secretions save that of the skin. 4. Constipation. 5. Slight cerebral excitement, followed by heavy sleep, merging into coma. 6. Relaxation of involuntary muscles. 7. Relief from pain.

Let us consider the above phenomena one by one, with a view to enquiry how far they can be accounted for by depression of the influence of the sympathetic nerve.

1. It has already been shown how improbable it would be to suppose any excitant action of opium upon the third nerve, and how therefore its contracting influence upon the pupil can only result from a depressing influence exerted by it upon the sympathetic.

2. The sympathetic being the motor nerve of the muscular fibre of the blood-vessel, it is obvious that depression of its influence will allow of an increased afflux of blood through the vessels. Such is the invariable result of division of this nerve as practised by C. Bernard, Brown-Séquard, and others. Now increased afflux of blood, if kept up for any time, becomes congestion. The congestive effects of opium, therefore, are sufficiently explained by its depressing influence on the sympathetic nerve. The pallor of the surface, however, seems an exception to this result: yet is not really so. For it is obvious that the whole body cannot be congested, while the amount of blood in it continues the same. What is gained by one part must be lost by another. In this case (as in the cold stage of ague) while the internal parts are *congested, the external* are drained.

3. While increased active afflux of blood, (as in the case of division of the sympathetic in the neck) sets up at first increased secretion, the congestion into which it soon merges has a precisely opposite effect. The urinary secretion is diminished in congestion of the kidney; but little bile is formed by a congested liver. So that the congestion of the internal organs produced by the depressing influence of opium on the sympathetic, amply accounts for the diminution in the secreting functions of these organs.

The truth of this explanation seems demonstrated by the fact, that the only organ which is not congested by opium—the skin—is also the only one whose secretion is not checked. Indeed, it is considerably increased, and this probably from the call which the congested kidneys make upon its vicarious function of aqueous elimination.

4. The diminished secretions of the intestinal canal would, in their turn, account sufficiently for the well-known constipating effects of opium. But I am inclined to think (from the opposite effects of belladonna) that this result is largely contributed to by the diminution of nervous excitement sustained by the muscular fibre of the intestines in consequence of the depression of the sympathetic's influence.

5. The effect of opium upon the cerebral hemispheres will, if the view I have propounded of the generation of nerve-force, be accepted, viz.: that it is secreted by the cells of the gray matter from the blood—be only a particular instance of its general influence upon secretion. The slight temporary excitement, and the subsequent gradual loss of consciousness—going on, when large doses have been given, to stupor and coma—are precisely analgous to the temporary increase of secretion from the active afflux of blood, and its subsequent diminution as congestion is established in other glands, whose bloodvessels have lost their sympathetic influence. The relaxation of the cerebral vessels produced by the depression of the sympathetic will, therefore, account for these phenomena also.

This is a highly novel conclusion, but is, I think, well borne out by the facts of the case. Natural sleep is, according to the generally received opinion, the result of a slight congestion of the brain. Habitual congestion —as in apoplectic subjects—induces habitual sleepiness. While the great internal congestion produced by severe cold, causes—as is well known to travellers—profound sleep which is often fatal. On the other hand, the theory that the soporific effects of opium depend entirely upon a direct sedative influence exercised by it upon the gray matter of the cerebral hemispheres, will hardly account for all the facts of the case. I allude especially to the primary mental excitement which follows its use, which could not exist, were its action upon the cerebrum directly sedative. And the excitement is too slight and fleeting to allow us to suppose the subsequent sleep the result of exhaustion or reaction, and so to class opium with alcohol and the inebriants.

6. The relaxation of spasm of involuntary muscles is a well-known effect of opium; as in the passage of gall-stones through the ductus choledochus, retention of urine from spasmodic action of the urethra or sphincter vesicæ, colic, &c. Now, all these involuntary muscles are animated by the sympathetic nerve; and their relaxation by opium falls thus under the general head of its depression of sympathetic influence. On the other hand, in asthma, which is a spasmodic affection of muscular fibres supplied, not by the sympathetic, but by the pneumogastric nerve, opium is (according to our latest and best authority on this disease, Dr. Hyde Salter), of little or no use.

The accounts given by authors of the state of the voluntary muscular system under the influence of opium are so various and contradictory, that I have no inclination to attempt a rationale of facts themselves so uncertain.

7. There is one more effect of opium—its anodyne influence; and this alone, I think, cannot be accounted for by its depressant action on the sympathetic. We must here allow a similar depressing action upon the centres *of sensation*; the gray matter of the cord and the sensory ganglia at the base *of the brain.* And it is noticeable that in diseases accompanied by severe *pain, the whole* energy of the opium seems expended upon these sensational

centres. The relative demand conditions the direction of the supply, and its physiological results from depression of the sympathetic—even contraction of the pupil—are not seen. Dr. Todd remarks this in respect of acute rheumatism; of one case he writes, "Thus, in 48 hours she took 16 grains of opium, exclusive of a grain and a half of morphia on each night, yet her pupils were not at all contracted, nor was she in the least degree narcotized. The effect upon her has been most beneficial; her nervous excitement has been calmed down, and her pain materially relieved." ('Clinical Lectures on Acute Diseases,' p. 47). So Dr. Sibson writes, ('British Medical Journal,' Aug. 13, 1859,') of the effect of frequent and full doses of opium in this painful disease. "The first thing that suggests itself for notice is the remarkable absence of any symptoms, such as accompany the administration of opium in less painful disorders. We hear hardly anything of drowsiness, of tendency to coma, of constipation, or contracted pupils; only, when the remedy is pushed to its extreme point, delirium comes on, which speedily subsides under the diminution of the dose. In some cases, so far from constipation being produced by the opium, the patient has actually suffered from diarrhœa during the opium treatment. In most cases there has been improvement in the patient's sleep; but this has never proceeded to the extent of narcotism. The pain was in almost all cases much relieved."

It may appear strange to admit a sedative influence of opium upon the sensory portion of the gray nervous matter, and to deny it such an influence on the same tissue in the cerebral hemispheres. But a similar thing obtains with woorara, which affects the motor portion of gray matter only, leaving its sensational and ideational regions untouched.

The last evidence I have to bring forward as to the specific influence of opium on the sympathetic nerve is drawn from its use in the treatment of ague. I purpose shortly to publish facts and reasonings which tend to show that the "cold stage" of this disorder is the result of irritation of the sympathetic nerve by the malarious poison, and the "hot stage" the morbid re-action of the contracted capillaries. Now, opium, if given immediately before the cold stage, will often cut short the whole paroxysm; which would obviously be the case if its action be sedative upon the very nerve whose irritation causes the phenomena.

Farther, the same considerations have led me to reduce the curative influence of quinine in intermittent fever under the general head of its tonic action upon the vaso-motor (*i. e.* sympathetic) nerves. Quinine, therefore, is a tonic where opium is a sedative; and we should expect to find these two drugs antagonistic; and that such is the case would appear from a remarkable experiment by M. Gubler. He states that "while opium causes cerebral congestion, quinine possesses a diametrically opposite influence. In support of this theory, the ingenious author relates a case in which 30 grains of quinine and five grains of extract of opium simultaneously administered, failed to produce either the characteristic intoxication of quinine, or the somnolency to be expected from opium."—'Medical Times and Gazette,' July 3, 1858.

It has thus, I think, been established, that opium contracts the pupil, and produces well-nigh the whole of its physiological effects, by a depressant influence exerted by it upon the sympathetic nervous system. The next question is, do belladonna and its congeners dilate the pupil by an opposite action upon the same sympathetic nerve?

The first evidence that such is the case arises from the fact, that belladonna causes contraction of the small arteries. Dr. Brown-Séquard, in his work on Epilepsy (see also his Lecture on Paraplegia, Lancet, May 5, 1860,) states this to be an effect of its internal administration; and Mr. Wharton Jones (Prize Essay on Inflammation in 'Guy's Hospital Reports' for 1850, pp. 8, 9.) finds that the application of a solution of sulphate of atropia to the small arteries causes constriction of their calibre, while opium largely and at once dilates them. Now, as the sympathetic is the motor nerve of the blood-vessels, it is most probable that substances which cause their constriction, do *so by a stimulant influence* exerted upon this nerve. Constriction

of the capillaries is a well-known result of galvanic excitation of the sympathetic in the neck.

But let us consider the several results of the administration of belladonna, with a view to ascertain how far they may be accounted for on the hypothesis of a stimulant influence upon the sympathetic system. They may be enumerated as—1. Dilatation of the pupil, with dimness of vision and protrusion of the eyeball. 2. Dryness of the throat, with dysphagia. 3. No congestion of the brain or other internal parts; the skin often hot and red. 4. The secretions somewhat diminished; the skin usually dry. 5. Slight aperient action. 6. Wakefulness and restless excitement, going on in severe cases to delirium. 7. Toning of most involuntary muscles. 8. Relief from pain.

1. Of dilatation of the pupil I need say nothing: and the dimness of vision may result from this, or from the diminution of vascular supply to the retina from constriction of its blood-vessels. The protrusion of the eyeball (noticed in most of Mr. Seaton's cases) is interesting; as it is one of the effects of galvanization of the sympathetic in the neck, enumerated by Dr. Brown-Séquard.

2. The dryness of the throat and dysphagia caused by belladonna, I have discussed in a paper which appears in the 'British Medical Journal' of May 26, 1860. I have there shown it to depend upon a specific depressant action exerted by this drug upon the pneumo-gastric nerve, which presides over the secreting function and muscular power of the alimentary canal from the fauces to the stomach. This, of course, is a practical rather than a physiological conclusion; for the pneumogastric nerve can hardly be influenced by a substance which has no power over other nerves of the same nature. It will suffice for our present purpose to conclude that belladonna depresses that cerebro-spinal influence which presides over the function of secretion.

Will this help us in any way to get at its action upon the sympathetic? I think it will. The researches of Claude Bernard upon the salivary glands appear to establish the following as the law of the relation of the two divisions of the nervous system to the secreting glands:—That a branch from the cerebro-spinal system is distributed to the secreting cells, and a branch from the sympathetic system to the coats of the arteries; and, consequently, excitation of the former increases, of the latter diminishes secretion. The extension of this law to other glands, though probable, has yet to be established by direct experiment; but there can be little doubt that it obtains with that secreting surface over which the pneumogastric presides. For, as in the experiment cited in the paper I have referred to, division of the pneumogastric nerves has been found to produce immediate draining of the vessels and check of secretion in the parts which it supplies, while we have formerly seen that division of the sympathetic produces precisely opposite effects, viz.,—increased afflux of blood and secretion in the parts supplied by it.

If, then, the influence of the pneumogastric and sympathetic nerves be antagonistic, it seems to follow that belladonna, which exercises a depressant influence on the former, will have an excitant action on the sympathetic.

3. The absence of congestion will naturally follow from the contraction of the arteries produced by excitation of the sympathetic. In Mr. Seaton's cases, nothing peculiar was noticed about the skin; but in a more severe case reported by Mr. Holthouse (Medical Times and Gazette, Dec. 17, 1859), it was "pungently hot and dry." The same remark is made relative to a case of belladonna poisoning, recorded in the Edinburg Medical Journal, April, 1860; and to three cases of poisoning by stramonium, contained in the British Medical Journal of October 2, 1858. Here, again, the opposition to opium is noticeable, in poisoning by which the skin is *cool and pale*; and the explanation there given may apply to this phenomenon also.

4. The effect of galvanic excitation on the sympathetic is to check secre-

tion from the great diminution of the flow of blood to the parts. And such appears to have been the effect of belladonna in Mr. Holthouse's case, and of stramonium in the three cases of poisoning by this substance above referred to. In the former instance no urine was passed from 9 a.m. to 10 p.m., or 13 hours. In Mr. Seaton's cases the secretions are not mentioned. Medicinal doses rarely have any appreciable effect on the secretions; but in Dr. Fuller's cases of administration of belladonna the urine is said to have been scanty.

5. While opium is the most powerful agent we have for locking up the bowels—which we have seen that it accomplishes by diminishing the secretions and depressing the nervous powers of the intestinal canal—the mydriatics have on the other hand (in medicinal doses) an aperient action. This has not indeed been ascertained with regard to stramonium; but it is well known in the case of hyoscyamus; and I can quite confirm Dr. Fuller's statement of the aperient power of belladonna. I have known half-a-grain of the extract, taken twice a day, to produce in four or five days quite troublesome purging in an adult whose bowels were usually costive. Such a result would naturally follow from gentle stimulation of the sympathetic which animates the muscular fibres of the intestines.

6. The most marked feature of large doses of the mydriatics is the wakefulness and restless excitement going on to delirium, to which they give rise. In Mr. Holthouse's case the symptoms were precisely those (to use his own words) of delirium tremens. Now the *post mortem* investigation of this disease invariably shows, according to Mr. Solly (On the Brain, p. 352) a bloodless condition of the gray matter of the cerebral hemispheres. And just as a great diminution in the supply of blood to the motor centres of the brain cause—as shown by the beautiful experiments of Kussmaul and Tenner—convulsions, so it would seem probable that a similar sudden anæmia of the intellectual centres may cause their analogue—delirium. And such a sudden anæmia would be caused by the contraction of the cerebral arteries resulting from excitation of the sympathetic. The delirium produced by the mydriatics may thus be satisfactorily accounted for.

A state of coma may ultimately come on in cases of poisoning by belladonna and its congeners. In this case, it is of that form called by Mr. Solly "anæmic coma;" and has been frequently observed at the close of exhausting diseases (See Solly on the Brain, p. 361; Watson's Lectures on medicine, 4th ed., vol. 1, p. 456; Gooch on Diseases of Women and Children, New Sydenham Society, p. 179). And there, as here, the pupil is dilated; not, as in coma from opium poisoning, contracted.

7. The toning effects of belladonna upon involuntary muscles are best exemplified in the now well-known power it has of curing incontinence of urine. This it would do by increasing the tone of the sphincter vesicæ, just as opium relieves spasmodic retention of urine by relaxing that muscle. And it is not improbable that the beneficial effect of a belladonna plaster applied over the region of the heart in palpitation of that organ from debility, may result from its toning rather than sedative effects upon the muscular fibre. In both cases, excitation of the sympathetic nerve which animates these muscles will account for the phenomena.

I have said "*most* involuntary muscles," because some are supplied not from the sympathetic, but from the cerebro-spinal centres, I may instance the muscular walls of the respiratory tubes and the alimentary canal from the fauces to the stomach. And here belladonna is found to have a precisely opposite effect (as is shown in my paper on its influence upon the pneumogastric nerve), and relaxes spasm rather than increases tone.

8. But what shall we say of the anodyne influence of belladonna? can that be brought under the general head of sympathetic excitation, or must it be referred to that sedative power over the cerebro-spinal system which we have seen it—as in the case of the pneumogastric—to possess? There can be little doubt that the latter is the true explanation. But to distinguish it here from opium, we have the fact I have before noted, that belladonna is especially *beneficial in external* neuralgiæ, where particular nerves are af-

fected: in which cases we rarely give opium with advantage. I am inclined to think that this anodyne influence of belladonna is local acting upon the affected nerve or nerves, rather than general on the nervous centres. Its great value when subcutaneously injected over the course of the nerve in sciatica, has been well ascertained (British Medical Journal, Feb. 11, 1860;) while its internal administration in the same disease is productive of far less benefit. And as to its effects on the urinary system, I would quote one of Dr. Fuller's conclusions, derived from the result of the administration of belladonna in full doses to children. "That inasmuch as it exercises a remarkable power in affections of the urinary organs, and the experiments recorded in this paper prove that it is excreted with the urine, it is highly probable that its curative action in such cases may be due in a great measure to its topical effect, and if so, that it might be applied locally with advantage."

I have in the above paper pursued into some detail the thoughts which arise from the fact that opium contracts, and belladonna dilates the pupil. The theories advanced are in many cases novel, and require much discussion to establish them. I can only say, that at present they seem to be the best to account for the facts.

In a future paper I hope to investigate the rationale of the state of the pupil in various morbid conditions, and the indications derivable therefrom. *London Medical Review, Aug.* 1860, *p.* 92.

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132.—*On the Physiological Effects of Opium.* By GEORGE BODINGTON, L.R.C.P.E., Sutton Coldfield.—Of course, it is nothing new to remark, that in delirium tremens, very large doses of opiates may be given, not only with impunity, but with almost certain advantage. I will not say what quantity of morphia may have been given, as that is the more dangerous form of the drug, but that from two drachms up to six of laudanum per dose has been often attained in delirium tremens is well known to experienced practitioners. Under what circumstances, then, does it occur that the drug so administered becomes comparatively innocuous? It is when the system is, so to speak, saturated with alcohol. On the other hand, the smallest dose given to patients who are mere water or tea and coffee drinkers, exclusively of alcoholic beverages, has an almost instantaneous and very powerful effect. Here is an instance in point. A lady, entirely a teetotaller, was seized with a catarrh and with severe coughing. An eight-ounce almond emulsion was prescribed, containing only about half a grain of morphia, of which she had taken about a fourth or a third part, when she fell into a complete state of syncope, and was restored by the dashing of cold water in her face and by the exhibition of stimulants. These occurrences are, I believe, frequent in general practice, and well known to the profession. The point appears to be this. Had the lady in question been in the daily habit of drinking a moderate quantity of wine or of other alcoholic beverage, no such sudden effect from so small a dose of morphia would have ensued. Contrasting this case with the effects of opium in cases of delirium tremens, the controlling powers of alcohol over the physiological effects of the drug are obvious.

It seems, then, that the use of the drug should be guided especially by the previous habits of the patient. The modern salts of the drug seem to be the most dangerous form in which it can be given. The native opium contains constituent ingredients of a contrary character which are counteractive of each other. Laudanum, holding all these in solution, is the safest form; inasmuch as the alcohol, the solvent, in some measure acts as an antidote. In practice, contraindications to either of these forms of exhibiting the drug frequently are present. In the treatment of lunatic patients, with *whom the* use of sedatives is of the first importance, my practice is to give *Battley's sed*ative drop in preference to any other form of opiate, in a little *brandy* and water or sherry wine, this vehicle obviating the tendency to pros-

tration or syncope which in some constitutions and under certain conditions might ensue. I am not aware that morphia administered in a similar manner would not have as good an effect.—*British Medical Journal, June* 9, 1860, *p.* 445.

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## 133.—ON OPIUM AS A REMEDY IN POISONING BY DATURA.

By Dr. Thomas Anderson, Assistant Surgeon, Bengal Army.

[The antagonism of opium and belladonna has been insisted on by several writers within the last few years. Dr. Anderson considers that this doctrine was first propounded by himself.]

In Britain, poisoning by belladonna, or by henbane, is so exceedingly rare an accident, that it was vain for me ever to expect to meet with a case of either. However, in 1854, I proceeded to India, where poisoning by datura is of everyday occurrence; and I determined to test my theory by administering opium to the first suitable case I should have an opportunity of treating. From the unsettled state of the country during my residence in India, first from the Santhal rebellion, and lately from the annexation of Oude, followed by the mutiny of our army, my service has been entirely military; and thus I have been thrown among the class of natives least obnoxious to datura poisoning. Only one favourable case has occurred to me, though I have heard of several cases that recovered, and assisted Dr. Fayrer, of Lucknow, in making *post-mortem* examinations of three or four individuals, to whom a fatal dose had been administered.

As this solitary case, however, illustrates my theory of the impossibility of these two classes of poisons exerting their action simultaneously on the brain, and is moreover, the converse of my former experiments, and at the same time supplementary to Mr. Bell's, I am induced to relate it in detail.

The 43rd Bengal Native Infantry, of which I was then the medical officer, while on its passage from the Punjab to Calcutta by boats, on the River Ganges, halted for four or five days at Futteyghur, in the beginning of December 1855. The sepoys were of course allowed to go on shore, and many of them visited the large bazaar in the native city of Furrackabad, two or three miles from Futteyghur, the British cantonment. Two days after our arrival, one of the sepoys, a man well know to me, was found by his comrades lying by the roadside, near Furrackabad, in a state of high delirium, and was brought to the hospital boat. On inquiry, I learned that he had been seen partaking freely of sweetmeats (the Hindoo eats them by pounds weight at a time) in the Furrackabad bazaar, some hours before he was found; and the men at once ascribed his condition to poison administered in the sweetmeats. The many varieties of native sweetmeats poisoned with datura and bhang, one of the preparations of Indian hemp, are well known in all large Indian towns, and are used to cause death or stupefaction, so complete, as to allow of robbery or violence being committed; and a sepoy, when in undress, usually displays valuable ornaments, sufficient to tempt a thief or thug. Even without this knowledge, the symptoms were so marked, that I had no difficulty in forming my opinion of the cause of the patient's condition. I saw him immediately after he was brought to hospital, and found him in the following state: He was reclining on his back in bed, in a wakeful muttering delirium, unbroken by an interval of even transitory consciousness. His face was much flushed; the eyes were reddened and wandering, and the pupils were brilliant, widely dilated, and quite insensible to light. The pulse was much accelerated and small. He continually twitched his extremities, but especially his hands, and every now and then pinched and tugged at the bedding. Though utterly unconscious to real external objects, the brain was evidently active, with continual visions before the eyes, as his mutterings and frequent clutchings referred to imaginary objects. He was not in the least degree violent, and required no restraint, but merely an attendant to keep him covered during his restless tossings about.

He swallowed any liquid put into his mouth, but exactly as patients in delirium tremens do, in one convulsive mouthful.

In an hour after his arrival at the hospital, I began the administration of opiates, and prescribed one grain of the muriate of morphia in solution, to be given every hour, beginning the first dose at 2 p.m. I watched the effect most closely, taking the state of the pupil as my principal guide.

Eight doses were given before I could observe any result. After the eighth dose, about 11 p.m., I noticed that his attention could be fixed for a moment, and that the muttering could be arrested by loudly talking to and shaking him; that the hands were less tremulous.

Still no impression had been made on the widely-dilated pupils, and he was evidently as wakeful as ever. I therefore, before retiring for the night, directed the continuance of the morphia until the morning, with orders to discontinue it so soon as sleep threatened to come on. I did not see him until six in the morning, when I found all his delirium gone, the tremulousness much relieved, the pupils almost reduced to their natural state, and the patient surrounded by his delighted comrades, who were listening with wonder to the native doctor relating to them how "dhutona" poisoning had been cured by "apheem" (opium.)

Though he was able to talk, and was nearly well, I considered it advisable to persevere with the treatment until sleep had been obtained. This did not supervene until three doses more had been given.

After several hours' sleep, he awoke perfectly well; and after two days more detention in hospital, he was dismissed "fit for duty;" and while I remained with the regiment, was never again in hospital. In all, fifteen grains of the muriate of morphia had been administered in eighteen hours; and with reference to the largeness of the amount, I took care to inquire concerning the habits of this sepoy, and found that he used opium in no form whatever.

In this case, as well as in the experiments published in my former paper on this subject, the amount of the narcotic used as the antidote, whether belladonna or opium, to restore the normal condition of the brain, would, by itself, have been a poisonous dose. This tolerance of the one poison, produced by the presence of the other poison in the system, seems to me to be a strong additional argument in favour of my theory.

It also induces me to go further, and to believe, that all narcotic poisons with distinctly opposite actions, and destroying life by their effects on the nervous centres, will, when present in the body together, counteract each other, until eliminated from the system by the excretions.

For example, the distinct coma-producing narcotics, such as opium, will, I believe, in all cases be counteracted by the poisonous solanaceæ; and as a remedy to the action of strychnia, I would hope for success from the use of conia, or preparations of hemlock itself; and, of course, in all these cases the opposite would hold true.

Unfortunately, no correct conclusion can be drawn from experiments made on the lower animals, as they are variously affected by narcotics. To dogs and cats I have given morphia in enormous doses without producing almost any effect; and to them five and six grains of atropia prove quite harmless. Rabbits will feed for months on belladonna leaves, and have their pupils dilated all the time, but otherwise they are not affected. Blackbirds eat belladonna berries in large quantities, and do not die.

It is, therefore, only to experiments on man that any value can be attached.

In conclusion, I would request my medical brethren never to give up hope of the most desperate case of opium-poisoning; but after the failure of the usual means, such as emetics, the stomach-pump, electricity, &c., to endeavour to produce, as rapidly as possible, the first symptoms of the action of belladonna on the system, by the administration of atropia, either internally or by injection under the skin, as indicated by Mr. Bell. My firm conviction is, that, in all such desperate cases (desperate as regards the old *treatment*), *this* plan, when boldly adopted, will be invariably successful.

*Since writing* the above, I have just heard that Dr. Carnegie, medical

missionary in China, in a letter lately received from him, mentions a case of poisoning by opium which he had just cured by giving belladonna.—*Edinburgh Medical Journal, June* 1860, *p.* 1100.

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## 134.—TANNIN AS ANTIDOTE TO STRYCHNINE.

By Professor Kurzak, of Vienna.

From want of a reliable antidote, the treatment in cases of poisoning by strychnine hitherto consisted principally in endeavouring to evacuate the poison, to combat the frightful spasmodic symptoms by narcotics, and to re-establish respiration, when it finally ceased, by artificial means. Donné proposed iodine, chlorine, and bromine as antidotes to strychnine; Garrod, Rand, Morson, and Falck recommended prepared animal charcoal; but the efficacy of these substances has been neither tested sufficiently by experiment nor proved by experience. The same is true in regard to tannin, and the astringent vegetables containing it, their infusions, decoctions, &c. Although they recommended themselves by the fact that tannin forms chemical compounds, insoluble in water, with strychnine and other poisonous alkaloids, it seemed very probable that these products might be redissolved in the stomach and intestines, and thus be rendered capable of absorption; the virtue of tannin as antidote to strychnine was, therefore, considered very doubtful.

With a view to subject this matter to a thorough examination, and to ascertain the efficacy of tannin in preventing and allaying the symptoms of poisoning by strychnine, Professor Kurzak made a series of experiments on rabbits and dogs. At the end of his interesting and highly important memoir, he states that the results of his investigation permit him to draw the following conclusions:—

1. *Tannin*, if administered in time, *is an* excellent *chemical antidote to strychnine.*

2. The doubt, whether the precipitate formed by tannin in a solution of strychnine, although insoluble in water, would not be redissolved by the gastric and intestinal juice, and the strychnine thus reobtain its poisonous properties, is solved by these experiments on rabbits and dogs in a complete and highly gratifying manner.

3. The successful results in dogs and rabbits justify the expectation that *tannin would suspend the poisonous action of strychnine also in man*, even in cases where the evacuation of the tannate of strychnine, formed in the stomach, could not be accomplished.

4. These experiments show that *twenty to twenty-five times the quantity of tannin is required in order to suspend the poisonous action of strychnine.* In cases of poisoning it will be, however, advisable to administer a relatively larger proportion, as a part of the antidote will be absorbed by the usual contents of the stomach, particularly by gelatin.

5. As tannin has proved to be an antidote to nitrate of strychnia, which is much more soluble in water, there is so much greater reason to hope that it will be successful in poisoning by pure strychnia which dissolves in water with great difficulty.

6. *The same successful result* is to be expected from its administration in poisoning by the hard and tough *nux vomica*, which imparts the poison to aqueous fluids, but gradually and not very rapidly.

7. Tannin is a so much more valuable antidote in poisoning by strychnine, as *galls* in which it is contained can be readily procured, and thus be administered without much loss of time. They are easily reduced to a powder, which is given mixed with water. Another advantage is obtained by the vomiting which it is liable to produce. In the mean time, an *infusion or decoction* of powdered galls may be prepared.

On an average, Turkish galls contain fifty, the Illyrian galls twenty per cent. of tannin. *At least one drachm* of the former and two drachms and a

half of the latter are therefore required to neutralize one grain of strychnine introduced into the stomach, but in general, especially if there is vomiting, a much larger quantity should be administered.

8. Another readily obtained substance containing tannin is *Chinese tea*, the efficacy of which, in poisoning by strychnine, is confirmed by our experiments. But these experiments (VII. and VIII.) have also shown that, in a decoction of tea-leaves, we cannot count upon the whole amount of tannin contained in them. In poisoning by a larger dose, it would therefore be necessary to administer so large an amount of green tea that the antidote itself might produce poisonous effects. One decigramme (1·3 grain) of nitrate of strychnine requires, as our experiments prove, ten drachms (600 grains, 40 teaspoonfuls) of green tea, which, according to Peligot's analysis, contain about fifteen grains of caffein. Tea is therefore applicable only in poisoning by smaller doses, but may otherwise be useful as adjuvant.

9. The efficacy of *roasted coffee* as chemical antidote to strychnine seemed to be much inferior. The amount of *caffeo-tannic acid* contained in coffee is according to Payen, 3·5 to 5·0 per cent. But our experiments (IX., X., XI.) show that the decoction evidently contains a much smaller quantity of undecomposed tannic acid than this percentage would justify us in assuming. The decoction of 180 grains of roasted Cuba coffee, (being adequate to 200 grains of the raw coffee, which should contain at least six grains of tannic acid) produced, according to the ninth experiment, merely a delay and diminution of the poisonous effect of 0·13 grain of nitrate of strychnine. In the tenth and eleventh experiments, 300 grains of raw coffee, which weighed, after roasting, 267 and 264 grains, and should have contained at least nine grains of tannic acid, had furnished a decoction which, as antidote to 0·13 grain of strychnine, was nearly inert, only delaying the appearance of the symptoms for a little while.

10. From *unroasted coffee*, so inconsiderable an amount of tannin is extracted, by boiling, that the employment of its decoction for our purpose is out of question.

11. *Oak bark* (of Quercus robur and Q. pedunculata) contains, according to Gerber, 8·5 per cent. of tannic acid, and imparts it readily to aqueous fluids. It deserves attention in poisoning by strychnine so much the more, as it can be procured without much delay, especially in the country. What has been said about the administration of galls equally applies to the use of the powder and decoction of this bark.

12. On account of their frequent occurrence and the large amount of tannin they contain, we have to mention in this connection: *acorns* (from Quercus robur and Q. pedunculata) with 9 per cent., *the bark of the horse-chesnut* with 8 per cent., *willow bark* with 5½ per cent., and *the green hull of walnuts*. The *radix tormentillæ* (with 17 per cent.,) *rad. caryophyllatæ* (with 31 per cent.,) and *rad. bistortæ*, are still richer in tannin, but can rarely be procured without much loss of time.

13. The solubility of the precipitate, produced by tannin in a solution of strychnine, by *acetic*, *citric*, and *tartaric acid*, (vide experiments with the same,) show the necessity of avoiding vegetable acids during the treatment of poisoning by strychnine with tannic acid.

14. The same applies to the internal use of *alcohol* and *alcoholic remedies*.

15. The reported experiments on rabbits have sufficiently proved that more active voluntary movements excite the spasms usually produced by strychnia, even when they otherwise would not have made their appearance. In treating cases of poisoning by strychnine, *it is therefore highly important to prohibit as much as possible all voluntary movements, and to avoid violent excitement of any other kind.* *Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien*, March 12, 1860.)—*North American Medico-Chirurgical Review, July*, 1860, *p.* 744.

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*135.—Detection of Phosphorus* in Cases of Poisoning.—SCHERER has found that the smallest trace of phosphorus may be detected by its reaction with

a salt of silver. Phosphorus volatilises even under liquids, at the ordinary temperature, and a piece of paper moistened with nitrate of silver is blackened when suspended over it in a closed vessel.

In order to guard against error, it is necessary, when the fluid contains organic matter, to mix it with pure sulphuric acid, and suspend over it a piece of paper moistened with an alkaline solution of nitro-prussiate of sodium, which would detect the presence of phosphoretted hydrogen by the violet colour produced. If by long contact with air the phosphorus has passed into the state of phosphorous acid, it may still be detected by distilling the liquid with zinc and sulphuric acid, and passing the evolved gas into a solution of nitrate of silver. The phosphoretted hydrogen present produces a black precipitate, which may also be oxidized with nitric acid, and then examined for phosphoric acid in the usual way.—*London Medical Review, July*, 1860, *p.* 46.

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136.—*Examination of the Superficial portions of the Eye by means of Natural Light Laterally Reflected from a Double Convex Lens.*—M. Laborie lately read a paper before a medical society of Paris, wherein he speaks of the advantages of the opthalmoscope, and those of the lamplight reflected laterally. He, however, finds fault with the latter mode of illumination, and prefers the natural light, reflected from a double convex lens, held in the hand of the surgeon, an inch and a half from the patient's eye. In this manner he has been enabled to examine affections of the cornea, of the anterior chamber, the iris, the pupil, and the lens. The great comfort is, according to the author, that the examination may be made at once, without repairing to a darkened room, or having recourse to artificial light, &c.—*Union Médicale; Lancet, May*, 26, 1860, *p.* 525.

137.—*Recovery from Strychnine Poisoning.*—A very good paper on a case of recovery from poisoning by strychnine, was lately read by Dr. PART, of Camden Town, to the North London Medical Society. This respected physician succeeded in his case, by the use chiefly of emetics and cold affusion. "As the vomiting continued, so the spasms appeared to abate in their intensity," and in about eleven hours the effects of the poison had passed off.

Dr. Part, after reviewing the various antidotes which have been recommended, such as tobacco, woorara poison, chloroform, hydrocyanic acid, camphor, animal charcoal, tannin, tincture of iodine, and fat, recommends that we should first employ emetics, and then judiciously assist by chloroform. As soon as the vomiting has ceased, he strongly recommends camphor suspended in almond emulsion. In his case he considered that the cold affusion materially assisted in inducing inspiration. As emetics, he prefers sulphate of zinc with copious draughts of mustard and water.

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## 138.—TWO CASES OF NARCOTIC POISONING; SUGGESTING A MODIFICATION IN THE USE OF THE STOMACH PUMP.

By J. EVELYN CROOK, M.D., Northfleet.

[The following cases prove that it is not necessary to *pump* fluid from the stomach, any more than from the urinary bladder, as both viscera may be emptied merely by passing a tube.]

*Case* 1.—May 15th, 1857.—An infant only twenty-two days old, had about forty minims of laudanum poured down its throat. The occurence happened in this way:—The mother, after her confinement, was attacked with muco-enteritis; and, among other remedies, an opiate injection was sent to the house, part of which only was administered, as the patient quickly recovered. It appears that an elder child, seven years of age, was left alone with the infant, *and she deliberately took* the bottle containing the remainder of the

opiate injection from the mantel-piece, and poured its contents down the infant's throat. The accident was soon discovered, and I was sent for; but not being at home at the time, did not arrive till the poison had been taken about half an hour.

When I saw the child, two mustard emetics and some antimonial wine had been given (how much I could not ascertain), but no vomiting had taken place. The pupils were contracted; the skin was cold; indeed, the child appeared to be dying. I immediately sent for my elastic male catheters, and passed a large one (No. 12) down the œsophagus. I had no sooner done so, than the contents of the stomach flowed through the instrument, and were collected in a basin. I filled my mouth with warm water, and by applying my lips to the end of the catheter, forced the water into the stomach; the fluid was immediately returned through the tube. This was repeated several times. I afterwards injected some warm coffee, and withdrew the instrument, the child being all this time in the upright position. The little patient was then enveloped in flannels wrung out in water tolerably warm; and at frequent intervals cold water was dashed over its body. This treatment was continued many hours, the infant during the time being fed with milk and brandy. Twenty-four hours elapsed before it was again able to take the breast. It recovered, and is now alive and well.

*Case* 2.—On the 23d of last November, a discharged soldier from India had been lying at the railway station in a state of coma for seven hours. At first, he was supposed to be intoxicated, so that little notice was taken of him, until a medical man, a passenger by one of the evening trains, pronounced him to be dying, and advised the police to be sent for. He was then removed to a public-house, where I attended him. I found him perfectly insensible; the pupils were contracted; the pulse was scarcely to be felt; the extremities, and indeed the whole body, were very cold. Friction, hot bricks, hot bottles, hot blankets, &c., were applied: but as the police strongly suspected him to be drugged, I considered it my duty (notwithstanding the time that had elapsed) to empty the stomach. I therefore passed the œsophageal tube into the stomach, having the syringe attached, and injected nearly a pint of warm water, when the man vomited, the contents of the stomach passing *by the side* of the tube. I then injected about another pint, and removed the syringe, the fluid then flowed through the tube into a basin. This was repeated until the water was returned perfectly clear, when strong coffee was injected, and the tube withdrawn, the patient being the whole time in the horizontal position. He continued perfectly insensible until between seven and eight o'clock the next morning, making altogether a period of sixteen hours. He recovered, and left the place the same day.

[It seems quite easy to empty the stomach when full enough by means of a syphon in this way, and we see no reason why the stomach may not as easily be *filled* as *emptied*. When to be filled, lift the tube *up* and pour the liquid in at the top;—when to be emptied, depress the end and make a syphon.]—*Brit. Med. Journal, May* 5, 1860, *p.* 341.

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## 139.—ON THE UNIVERSITY OF ST. ANDREW.

### WITH REMARKS ON THE DEGREE OF M.D. OBTAINED THERE.

By William Braithwaite, M.D., (St. Andrews.)

"The University of St. Andrews has done good service in supporting this examination movement. We call attention to the papers of questions proposed to the Candidates, and feel assured that they would do credit to any body, however learned. It is one of the privileges of this country that she may feel proud of so many excellent schools of instruction."—*London Medical Review, Sept.*, 1860, *p.* 109.

*We were* agreeably surprised during last September with the ancient *University of* St. Andrews. We confess that we were attracted to this seat of

learning chiefly by the celebrity and high character of Dr. Day, its Professor of Medicine, and were not prepared to find that its importance as a seat of education had been so misunderstoood and undervalued by English medical practitioners. Instead of the small and rather mean city, which we anticipated, we found that, by means of modern additions and improvements, it was not only rapidly advancing to be an enjoyable watering place, where people from various parts of Scotland were resorting for its healthiness, its splendid sands, its golf-ground two miles long, and its picturesque rocks and antiquities; but that it was also much frequented for the sake of its educational establishments. It will generally be found that in such places as Rugby, Harrow, &c., where there are good schools, families of moderate incomes will congregate for the sake of educating their boys in a superior manner, and at small cost. This seems to be the case at St. Andrews. It is rising up to be an important place of education in Scotland, and has, under the superintendence of Dr. Day, a superior University for the examination of medical practitioners and students for the degree of Doctor of Medicine.

By the late regulations of the different Medical Boards in Great Britain and Ireland, there are now, we believe, only two Universities where medical men, already in practice, can obtain the degree of Doctor of Medicine, without residing more or less at the place of examination. These two universities are the London and St. Andrews. The London University admits to examination students from different schools in England, but renders it very difficult, owing to its stringent rules about the rudimentary knowledge which it requires from all its candidates, for medical practitioners of mature age and experience to obtain its degree. This, in our opinion, is a foolish and unnecessary restriction. Why compel a medical practitioner of 40, who is, perhaps, distinguished in his own town for character and success in practice, again to *rub up* the elements of knowledge, such as Greek and Latin, Anatomy, Physiology, Chemistry, and Materia Medica, which he knew well twenty years before, but which have now been succeeded by ripe experience in the knowledge and treatment of diseases? Why not examine such a candidate in his knowledge of Medicine, Surgery, and Midwifery, and on these subjects only be particular and exact, demanding that he shall be *well up* in every thing required at the bed-side. Now, this is just the practice at the University of St. Andrews, and hence its increasing popularity with all sensible men. At this University, a medical man of mature age and excellent character is examined most strictly in Medicine, Surgery, and Midwifery, including the diseases of Women and Children; but those branches of knowledge which he is supposed to have known twenty years before, are considered in his case, of secondary importance. The younger candidates, indeed, are expected to be thoroughly conversant with *all* the subjects of study, especially such as young men *ought* to know; but in them, the higher branches of Medicine (such a practical knowledge of disease as can only be learned by years of experience) will, no doubt, hardly be expected, so that the candidates are strictly examined according to what is expected of them. The younger men being examined in Celsus, Anatomy, Physiology, Chemistry, &c., and being expected to have also a good knowledge of Medicine, Surgery, and Midwifery; while in the older candidates, this is very properly reversed. Hence St. Andrews is yearly becoming the Alma Mater of English practitioners, while London will continue to be only the resort of remarkably few candidates, instead of being, what it ought to be, the liberal Alma Mater of both the younger and the older practitioners of Medicine in England. We would venture to recommend the London University to be as liberal and fair to men in practice, as is St. Andrews. Let the Examiners allow gentlemen of 40, who have worked their way up in their respective towns and localities, often to a much higher social and professional position than their own graduates, to be examined *only* in Medicine, Surgery, and Midwifery; and let them insist on a deep knowledge of these practical subjects, and they will have a very superior class of men at-

tempting to obtain the London Degree, which we consider now to be the best Medical Degree in the empire.

St. Andrews was one of the ancient cities of Scotland, the residence of an Archbishop for a great number of years, who possessed great power and wealth, and the ruins of whose Cathedral remain to this day a monument of his greatness. You enter the city in one direction by a gateway similar to what you find in other old towns, such as York. A new town of good houses and villas is gradually being added to the more ancient city. At the eastern extremity, on the abrupt rocky sea-shore, you come to the most interesting ruins of a large Cathedral, a very ancient square Tower of great height, and an old Castle of rather large dimensions, with its central court-yard, its rooms all round, and its *bottle-dungeon*, so called on account of its resemblance in shape to a bottle. These ancient edifices are built on commanding rocks, which must have a picturesque appearance from the sea. At a short distance are what are called the *Links*, but which in England would be called *Downs*, or fine grass land kept short by sheep feeding, and varied by sand-holes. These Links are about two miles long and constitute the play-ground of the inhabitants, both young and old, on which the Scotch game of *Golf* is nearly always going on. We never trod on firmer sands than at St. Andrews, extending for two or three miles, with a magnificent ocean foaming and roaring amongst the fine rocks which are there situated. Ins hort, we found St, Andrews to be a place where the intellect could be gratified by one of the finest College Libraries in the klmgdom, where the artist could sketch the most interesting ruins, and where the athletic man or boy could play golf to his heart's content. Besides these, you may *boat* and *fish* and *shoot ducks* for ever.—so that had it not been for a severe examination taking up most of our own time, we should have thoroughly enjoyed the place. We ought also to say, that you have the most talented preachers the worthy successors of John Knox, who often preached here, and whose pulpit is still preserved in one of the Colleges.

But it is with respect to the University of St. Andrews that we chiefly wish to enlighten the English reader, but not the Scotch reader, as he already knows and duly appreciates this seat of learning.

The University includes two Colleges, the United College of St. Leonard and St. Salvator now commonly termed the "United College," and St. Mary's College. In the United College there are Professorships of Latin, Greek, Mathematics, Natural Philosophy, Logic, Moral Philosophy, Civil History, Chemistry, and Medicine. This is the College of the Faculty of Arts: and after three years' attendance upon the prescribed course of classes, students can offer themselves for examination for the B.A. degree, a fourth year's study being required for the degree of M.A.* No student can commence his studies at the Theological College until he has attended lectures on Latin, Greek, Mathematics, Logic, Moral Philosophy, snd Natural Philosophy, during four sessions. The session extends from the beginning of November to the end of April. St. Mary's College is devoted entirely to the study of Theology, and contains Chairs of Hebrew, Biblical Criticism, Church History, and Theology. In addition to the two Colleges which have been already mentioned, there is an establishment termed "The Madras College."

The Madras College was founded but a short iime ago, by a bequest of about £60,000 by the late Dr. Bell, and is named the Madras College, because it was in Madras that *Dr. Bell himself* first tried the system, which is essentially the Monitorial one. It has nearly 1000 scholars, some commence ing early and continuing there till 15 or 16, when they are ready, if they think fit, to finish their education at the United College, obtaining the usual degrees of B.A. and M.A. The rest of the scholars, who may be intended for commerce or other industrial pursuits, do not enter College but go forth to all

* We may mention that the Latin, Greek and Mathematical classes are taught by gentlemen who *have* obtained the highest honours in their respective departments in the English Universities, and *that the United College* consequently affords an admirable education (on extremely moderate terms), *for young men preparing* for Oxford, Cambridge, and the India Civil Service, &c. The Professor of *Medicine gives* popular courses on Physiology and Comparative Anatomy.

parts of Scotland to earn their livlihood as they best can: and this kind of education gives an intelligence to this class which we often find deficient in England.

We have only room here to dwell on that department of the University which has reference to medicine. We may just say, however, that the Divinity College of St. Mary's is presided over by Dr. Tulloch, a man universally beloved and respected, not only for his great learning but for his urbanity towards every one; and that the Arts College, called the United College, is presided over by Professor Forbes, late of Edinburgh, whose name alone is a guarantee of every thing that is good; and that Dr. Day is in this College the Professor of Medicine—a man well known and appreciated by the whole medical profession, not only for his talents as the author of several original works, and as a writer in various medical publications, but for the zeal that he has exhibited, ever since his appointment, in endeavouring to raise the character of the St. Andrews degree. The Assistant Examiners have evidently been selected by the University with great care, as men whose reputation would add to the value of the degree conferred. For, example we have most of us heard of Dr. Alexander Wood and Dr. W. T. Gairdner of Edinburgh, of Dr. G. Buchanan and Dr. Struthers, of Dr. A. Anderson and Dr. J. M. Duncan. They are all men of note, and distinguished in their different departments for their acquirements and reputation. All these men must considerably add to the reputation of St. Andrew's, and make its degree rank high in medical and general estimation.

New regulations for students commencing their studies on or after Oct. 1, 1861, have lately been issued by the University of St. Andrew's, but these will not interest the great body of medical practitioners, and old students at the various medical schools whom we are now addressing, and who will be examined according to the old regulations, a copy of which we here insert.

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# UNIVERSITY OF ST. ANDREWS.

## REGULATIONS OF THE SENATUS ACADEMICUS.

RESPECTING THE

## EDUCATION OF CANDIDATES FOR THE DEGREE OF DOCTOR OF MEDICINE.

I. Every Candidate for a Diploma in Medicine, upon presenting himself for examination, shall produce satisfactory evidence—
   1. Of unexceptionable moral character.
   2. Of having had a liberal and classical education.
   3. Of having completed the 22nd year of his age.

II. Fellows, Members, and Licentiates of the Royal College of Surgeons of England, Edinburgh, and Dublin—of the Royal College of Physicians of London—of the Faculty of Physicians and Surgeons of Glasgow—and of the London Apothecaries' Company—are eligible as Candidates for the Degree of Doctor of Medicine, on producing their Diploma or Licence.

*N.B.— Notice to Students.*—New regulations (copies of which may be obtained from the Secretary) have been published for students commencing their attendance on Medical Classes in or after October. 1861.

III. Candidates not holding any of the qualifications enumeraetd in the above clause, must produce satisfactory proof that they have regulary attended Lectures for four complete Winter and two Summer Sessions, on the following *branches:*

| | | | |
|---|---|---|---|
| 1. Anatomy | 2 | courses of six months each. | |
| 2. Practical Anatomy or Dissections | 12 | months. | |
| 3. Physiology | 1 | course of six months. | |
| 4. Chemistry | 1 | do. | do. |
| 5. Practical Chemistry | 1 | do. | of three months. |
| 6. Botany | 1 | do. | do. |
| 7. Natural History or Comparative Anatomy | 1 | do. | do. |
| 8. Materia Medica and Pharmacy | 1 | do. | do. |
| 9. Midwifery and Diseases of Women and Children | 1 | do. | do. |
| 10. Medical Jurisprudence | 1 | do. | do. |
| 11. Surgery | 1 | do. | of six months. |
| 12. Clinical Surgery | 1 | do. | do. |
| 13. Practice of Medicine | 1 | do. | do. |
| 14. Clinical Medicine | 1 | do. | do. |

and that they have diligently attended Medical Hospital Practice for at least two entire years. The Lectures and Hospital Practice may be attended at any of the medical Schools and Hospitals recognised by the Universities of London or Edinburgh, or by the College of Surgeons of England.

# REGULATIONS RESPECTING THE EXAMINATIONS.

### EXAMINERS FOR DEGREES IN MEDICINE.

George E. Day, M.D., F.R.S., Professor of Anatomy and Medicine.
Arthur Connell, F.R.SS.L. and E., Professor of Chemistry (assisted by M. Foster Heddle, M.D.)

### ASSISTANT EXAMINERS FOR THE YEAR 1861.

*In Therapeutics and Practice of Medicine.*

William T. Gairdner, M.D., F.R.C.P., Lecturer on the Practice of Medicine, and on Clinical Medicine, Edinburgh.
Alexander Wood, M.D., President of the Royal College of Physicians, Edinburgh.

*In Surgery.*

George Buchanan, M.D., Surgeon to the Royal Infirmary, Glasgow.
John Struthers, M.D., Assistant-Surgeon to the Royal Infirmary, Edinburgh.

*In Midwifery and the Diseases of Women and Children.*

Andrew Anderson, M.D., Professor of Medicine in the Andersonian University, Glasgow.
J. Matthews Duncan, M.D., Lecturer on Midwifery, Edinburgh.

The examinations take place three times in the year, commencing on the first Monday in May; on the last Monday in September; and on Thursday, the 27th of December. The graduation fee is Twenty-five Guineas. In the event of a Candidate being found unqualified, he shall forfeit one-third of the graduation fee; which, however, will be accounted for to him when he passes his examination at a subsequent trial.

The examination by printed papers extends over three days, after which *each Candidate* is submitted to an oral examination.

*All Candidates* are required to give a written translation of a passage from

the first four Books of Celsus, to write Prescriptions in Latin with accuracy, and to be so far acquainted with Greek as to be able to give the meanings of Scientific and Medical Terms derived from that language.

During the first three days of the examination, the Candidates answer printed questions on (1) Chemistry and Materia Medica; (2) Anatomy and Physiology; (3) The Practice of Medicine; (4) The Principles of Surgery; and (5) Midwifery and the Diseases of Women and Children. They are likewise required to write a short Commentary on a Medical, on a Surgical, and on a Midwifery case.

The Degree is conferred, at the conclusion of the oral examinations, by the Rector, in the Hall of the Public Library of the University, and the Diplomas are signed by the Professors of the University.

Every Candidate is required to present himself for Registration to the Secretary, on or before the Saturday preceding the May and September examinations, and on or before the day preceding the December examination, and to communicate by Letter with the Professor of Medicine, at least a fortnight previously, stating what Diploma or certificates he intends to produce.

JAMES M'BEAN, A.M., *Sec.*

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As the Examiners receive very frequent applications respecting the course of reading to be pursued, they beg to recommend the following works as especially deserving of perusal:—

Fownes's Manual of Chemistry. (Candidates who have been long engaged in practice are expected to possess, at least, a knowledge of the general principles of the Science, and an acquaintance with the ordinary Chemical Compounds used in Medicine.)

Christison's Dispensatory, or Pereira's Materia Medica (especially the sections treating of the mode of action, uses, and administration of Medicines), and the London, Edinburgh, or Dublin Pharmacopœia.

Gray's Anatomy, Quain's Elements of Anatomy, or Wilson's Anatomist's Vade-Mecum.

Carpenter's Principles of Human Physiology, or Kirkes' Handbook of Physiology.

Williams's Principles of Medicine, and either Watson's Lectures on the Principles and Practice of Physic, or Wood's Treatise on the Practice of Medicine (American).

Miller's Principles and Practice of Surgery, or Fergusson's System of Practical Surgery.

Ramsbotham's Principles and Practice of Obstetric Medicine, or Churchill on the Theory and Practice of Midwifery.

Candidates who have acquitted themselves creditably in the written examination are allowed to compete for Honours.

Candidates for Honours are additionally examined in Comparative Anatomy and Physiology, in the higher departments of Human Physiology and Pathology, and in Medical Jurisprudence; and their practical knowledge of Medicine is tested at the bedside.

ST. ANDREWS, *November*, 1860.

All Medical Practitioners possessing the diploma of the College of Surgeons, or the Licence of the Hall, will come under the old regulations, and can at once proceed to St. Andrews. The best plan for Students at present, is to first obtain the diploma of the College of Surgeons, and then to go to St. Andrews. They will then possess a good degree, and also the College diploma in surgery, thus fitting them for any office or position.

We will now proceed to explain what the examination is, and what is the best way for our professional brethren to pass it. The examination, last September, began on Monday morning. For two days we were occupied in a written examination. The hours, were, in the morning of Monday, from 10 to 1 o'clock, and in the afternoon, from 3 to 5; on the Tuesday, from 10 to

12 in the morning, and from 2 to 5 in the afternoon. The two following days of Wednesday and Thursday were occupied by the different examiners in *viva voce* examinations of the respective candidates, each candidate going in according to his name standing alphabetically. On the Friday, the successful candidates had the degree of M. D. conferred on them, in the University library, by Dr. Tulloch, the Vice Chancellor, in presence of a goodly number of ladies and gentlemen of the city.

The following were the questions asked at this examination :—

## UNIVERSITY OF ST. ANDREWS.

### Medical Examination Papers.—September, 1860.

### First Examination,—First Part.

*To be Translated into English.*

Cognitis indiciis, quæ nos vel spe consolentur, vel metu terreant, ad curationes morborum transeundum est. Ex his quædam communes sunt, quædam propriæ : communes, quæ pluribus morbis opitulanter : propriæ, quæ singulis. Ante de communibus dicam : ex quibus tamen quædam non ægros solum, sed sanos quoque sustinent; quædam in adversa tantum valetudine adhibentur. Omne vero auxilium corporis, aut demit aliquam materiam, aut adjicit, aut evocat, aut reprimit, aut refrigerat, aut calefacit; simulque aut durat, aut mollit. Quædam non uno modo tantum, sed etiam duobus inter se non contrariis adjuvant. Demitur materia, sanguinis detractione, cucurbitula, dejectione, vomitu, frictione, gestatione, omnique exercitatione corporis, abstinentia, sudore. De quibus protinus dicam.

Give the origins or primary meanings of the following words : Astragalus, Blastema, Condyle, Cysticercus, Hemiplegia, Lithotrity, Peroneal, and Scaphoid.

### Second Part.

*Chemistry.*—1. Water boils at 212° F. and freezes at 32°. Is this statement universally true, or only true under certain conditions? What are the corresponding boiling and freezing points in the Centigrade Thermometer? Explain the method of converting degrees from one of these scales to the other. Reduce 100° F. to the Centigrade Scale and 256° C. to the Fahrenheit scale. In the Arctic Regions would you prefer mercurial or spirit thermometers?

2. What compounds does oxygen form with carbon, hydrogen, sulphur, and phosphorus, and what are their respective formulæ?

3. Write down the formulæ expressing the composition of nitre, borax, alum, and corrosive sublimate, and explain the method of obtaining any two of these substances in a state of purity.

### Third Part.

*Materia Medica and Therapeutics.*—1. Name the principal medicines which are commonly regarded as diuretics, and describe the modes in which they respectively act. Do these substances increase the aqueous portion or the solid constituents of the urine, or both? What effect upon the solid constituents of the urine is produced by copious water-drinking?

2. Are you acquainted with any substances which have the power of checking the general metamorphosis or disintegration of the tissues? If so, describe them; and explain their mode of action, and the cases in which their administration may be of service.

3. Quinine is sometimes adulterated with one or more more of the following substances : gypsum, chalk, boracic acid, sugar, starch, and salicine. How would you detect these impurities?

Supposing that any other acid had been mixed with hydrocyanic acid, how would you detect the adulteration?

*How would you determine the strength of any given specimen of hydrocyanic acid?*

4. What is the preparation of bismuth that is commonly used in medicine? How is it prepared? What are its uses? Describe the symptoms which would lead you to prescribe it, and write a Latin prescription (without symbols or abbreviations) for a draught containing it.

### Second Examination.

*Anatomy and Physiology.*—1. Describe the muscles which flex the leg upon the thigh.

2. What parts must be removed to expose the pterygoid muscles? Describe these muscles, noticing their relations to adjacent structures, the source from whence they derive their nerves, and their actions.

3. Describe the boundaries and contents of the axilla.

(N. B. Candidates who prefer answering this question fully, may neglect the two preceding questions.)

4. What are the respective numbers of the temporary and permanent teeth? State, as nearly as you can, the period at which you would expect the different teeth to appear. Describe the structures which occur in the composition of a tooth. If you have studied comparative anatomy, you may describe the peculiarities of dentition in the ruminants and in the rodents.

5. Describe the appearance and structure of the villi, and explain the part which they are supposed to take in the process of intestinal absorption.

6. What are the different refracting media of the eye through which the rays of light must pass before they reach the retina? Explain how these different media modify the direction of the rays. What are the conditions giving rise to myopia and presbyopia? Explain the way in which glasses remedy these defects.

### Third Examination.

N. B.—In answering the practical questions, the examiners require every candidate to specify the mode of treatment which he is in the habit of adopting, and the doses of the medicines which he prescribes.

*Medicine.*—1. State what you know of the general doctrines of the hemorrhagic affections, especially as to their origin, causes, nature, and varieties; and name the special hemorrhagic affections.

2. Describe the general principles applicable in the detection of valvular diseases of the heart, the diseases to which the valves of that organ are liable, and the means of detecting the particular valves affected, and the nature of the affection.

3. What advantages are gained by a selection of climate for invalids, what affections are likely to be benefitted by such selection, and what climates would you select, 1st, for incipient phthisis—2d, for chronic bronchitis, in this country and abroad?

4. What are the different forms of lithiasis, and what treatment would you adopt in each?

5. A robust young man was seized four days ago with headache and shivering. He went to bed and slept well all night, but on the following morning was seized with pain (severe and stabbing) below right nipple; his pulse then became rapid and skin hot. At the period of observation pulse 108, skin pungently hot, tongue covered with a thick white fur, rubbing sound with fine crepitation below right nipple.

What disease does the patient labor under? What would your prognosis be? How would you treat such a case?

6. A patient, aged fifty, is losing flesh and weight rapidly, subject to headache and palpitation; appetite voracious; urine plentiful, sp. grav. 1·040; sample of urine turned claret colour by liquor potassæ.

What is the disease, and how would you treat it?

### Fourth Examination.

*Surgery.*—1. What is iritis? What are its symptoms and diagnostic marks, the results if it proceeds unchecked, and the treatment?

2. What is *hydrocele? Mention* its varieties. With what other tumours

of the scrotum might it be confounded, and state how it may be distinguished from them?

3. Describe the various forms of the dislocation of the hip-joint.

4. Describe the process of formation of an aneurism. Mention the methods of treatment.

5. A man falls from a height on the top of his head, and is taken up insensible. On examination, no depression or other sign of fracture is found on the surface of the cranium. His breathing is laboured and his eye is insensible to light. He remains unconscious, and his urine and fæces are passed involuntarily. At first blood, then watery fluid flows from the ear. Ultimately he dies comatose.

What injury has been received? Explain how it has been produced, account for some of the most prominent symptoms.

What appearances will probably be found on post-mortem examination?

FIFTH EXAMINATION.

*Midwifery.*—1. Mention the various circumstances which may make it proper to induce premature labour; and describe the different ways in which this may be effected, stating which you would prefer, and why so.

2. Recount the symptoms and signs of pregnancy, stating the circumstances which may in particular cases obscure the diagnosis.

3. What are the points in the history, the symptoms, and the physical signs of a case, which enable us to distinguish ovarian dropsy from ascites?

4. A woman, on the day after a very severe labor, in which turning had to be performed, had a rigor, which returned from time to time during the next week, being always followed by perspiration, and accompanied by gradually increasing asthenic fever. Her pulse was extremely rapid and feeble, her tongue dry and brown, her skin of a dusky yellowish colour; the lochia were fœtid, and the milk suppressed. She became very restless and sleepless, and died before the end of the fortnight, the region of the uterus having been only slightly tender. What was the nature of her disease, what the morbid appearances to be expected in the dead body, and what treatment ought to have been employed?

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Now this is the examination which we underwent last September, and we maintain that a better set of questions could hardly be asked by any examining board in the kingdom; and we will also venture to add that we believe, from the conversations which we had with several of the candidates, these questions were in most instances well answered. The writer of this pamphlet can only say for himself that it occupied him the best part of two days to do justice to these questions, and to answer some of them he had to *ransack* his old brains severely for some hours.

In short, we consider that the examination at St. Andrews is one of the best in the kingdom, and one of the fairest too: not exacting too much of that knowlege from young candidates which can only be obtained by years of experience at the bedside, nor too much of that elementary knowledge from the older candidates which they knew in bygone days but have forgotten. Every candidate has fair play. If his written answers are comparatively defective he is examined with more strictness orally, while if they are quite satisfactory to the examiners, he has little to dread from his oral examination.

Let us now recommend all our professional brethren to go to St. Andrews without loss of time. It is just possible that under some future new regulations the Commissioners for the Scotch Universities might insist on residence at St. Andrews, as they have done at other Universities, although we think that this would be a great injustice to St. Andrews, seeing that there is no hospital, and considering that it almost strictly resembles the London University, both being chiefly examining boards, although St. Andrews has the *advantage* of colleges attached to it. But, for medical students, what is a *college* without an hospital? If the Commissioners close St. Andrews to all

but resident candidates, we can only say that practitioners will apply to Foreign Universities for degrees, as has been the case already to a disgraceful extent, or perhaps to the Archbishop of Canterbury, whose degrees (called Lambeth degrees) have now become a laughing stock to the medical profession, on account of their profusion amongst the ranks of Homœpathy, and obtained apparently simply by purchase, without that examination which confers honour on those who undergo it. What a pity that so high a dignitary of the English Church should allow such disgrace to fall on his honorary degrees, that they are despised by all respectable medical practitioners who know anything about the matter?

In preparing for this examination we would recommend the younger candidates to be *well up* in all the books mentioned at page 319. To the older practitioners we would say,—go up *well read* in Physiology, Medicine, Surgery, and Midwifery, especially in the books recommended: and in Celsus, Chemistry, Materia Medica, Anatomy, it will be sufficient to be *pretty well up*. It would be unreasonable to expect a man who had been hard at work in practice for perhaps fifteen or twenty years, to get up his Chemistry, Materia Medica, and Anatomy again; and we know that this is not expected of him; what *is* expected of such candidate is, that he should be *well up* in everything belonging to *Bed-side practice*,—the use of the stethoscope, tests for urine, Toxicology, the diagnosis and treatment of diseases of the Nervous System, Heart, Liver, Kidney, Lungs, &c., and such other diseases as every good Physician ought to know. He should also be well up in the Principles of Surgery, in the Practice of Midwifery, and the diseases of Women and Children. If he be well informed on these practical subjects he need not fear being rejected at St. Andrews. It is the most *sensible* and fair examination of any of our Universities, and must rank high with every one who knows anything about the matter.

# A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOLLOWING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

## AFFECTIONS OF THE SYSTEM GENERALLY.

AGUE.—M. Eissen states that quinic ether, when inhaled during a paroxysm of ague, arrests the attack, and prevents the recurrence of future attacks. This, however, is open to doubt, as the ether only contains kinic acid, which is known not to possess the tonic and antiperiodic properties of quinine. (p. 25.)

BRONCHOCELE.—The author considers this disease as the result of a perverted action of the nervous functions, at least in this country. Acting on this theory, he administers strychnia in these cases, giving at first a dose of the sixteenth of a grain three times a day: if accompanied by anæmia, he combines iron with the strychnia in solution. This treatement, from the cases reported, seems to be followed by good results; the enlarged organ shrinks, the improvement commencing after taking the remedy a few weeks. (Dr. Murney, of Dublin, p. 281.)

CHRONIC RHEUMATISM.—*Congelation.*—It is well known that in many cases the whole list of drugs, likely to be of any use in cases of subacute and chronic rheumatic inflammation of joints, may be gone through, without any good effect whatever. Dr. Arnott affirms that extensive experience in the employment of congelation in these cases, shows that cold thus employed is capable, in the great majority of instances, of immediately and permanently relieving the pain, and subduing the inflammatory condition of the part to which it is applied. Of course, this agent can have no influence on any constitutional affection coexisting with that which is local. The mode of employing congelation is as follows:—By means of a flat iron, break into a fine powder half or three-quarters of a pound of ice, enclosed in a canvas bag, then rapidly mix this with half its weight of common salt, pour the mixture into a piece of gauze, and apply to the part for about six minutes. (Dr. J. Arnott, p. 26.)

TYPHUS FEVER.—The cardiac sounds are said by Dr. Stokes to form the only certain guide for the proper administration of wine in fever. This is a practical rule, and whenever we find the systolic or first sound so weak as scarcely to be distinguished from the second, and the pulse at the wrist soft, compressible, and rapid, wine or brandy must be administered freely. It is especially necessary to enjoin that nourishment and wine be given at proper intervals during the night, when the depression is usually most marked, and that the patient be not allowed to sleep too long without being awakened for the purpose of taking his wine. Many a patient has thus been allowed to pass into hopeless collapse. It is very common, in cases of typhus, for the patient to be restless and wakeful; do not hesitate to give opiates freely if necessary. (Dr. A. Tweedie, p. 21.)

TYPHOID FEVER.—*Emetics.*—You will in some cases of typhoid fever positively succeed in cutting short the malady by the early administration of emetics. Ipecacuan is most suited to these cases, and the wine, in doses of an ounce or two ounces, may be given once or twice a day, plentifully diluted with warm water. This treatment is only suitable for the first four *days, dated from* the inaugural rigors. During this period there seems in *many or most* cases to be a natural determination of the fever poison to the *gastric mucous* membrane. (Dr. Brinton, p. 24.)

*Typhus and Typhoid Fever.*—Contrary to the opinion of some of our best writers, Dr. Kennedy thinks that these are the result of a common poison. In cases of typhoid, the general impression is that, when stimulants are required, it matters little what kind is employed—hence brandy, wine, beef-tea, &c., are often given to the same patient, and often on the same day. This is not good practice: beef-tea and wine ought not to be used indiscriminately; their effects are very different. Wine may be given in *typhoid* much more safely than beef-tea, which often causes secondary inflammation. The nitrogenous material in beef-tea is much greater than in wine. On the other hand, the latest analysis of wine (by Mulder) shows that it contains very small quantities of albumen, iron, &c. Hence the opinion of Cheyne and Barker, advanced forty years ago, appears to be correct, that whiskey and pure alcohol will not supply the place of wine. Carbonate of ammonia is not suitable to cases of typhoid; it causes purging. Dilute sulphuric acid and water, with three or four drops of laudanum in each dose, will often be very useful, for example: ℞. Acid. sulph. dil. ʒ ij.; aquæ lbss; tinct. opii guttæ 30. Ft. mist., capiat ℥j, tertia vel quarta quaque hora. (Dr. H. Kennedy, p. 17.)

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## AFFECTIONS OF THE NERVOUS SYSTEM.

Chorea.—Faradization of the skin in cases of chorea occasions a very rapid and marked diminution of the movements, and frequently effects a prompt cure of the malady. It should be applied chiefly to the parts most convulsed, every day, or every other day. (M. Briquet, p. 70.)

Delirium Tremens.—Give *half-an-ounce* of the tincture of digitalis in a little water. This will generally suffice, but a second dose of half-an-ounce may be required in four hours, and even a third dose. The action is on the brain and not on the heart; the pulse is not lowered but becomes fuller and stronger, and more regular. Smaller doses do more harm than good; doses of half-a-drachm or a drachm do no good, but harm—the pulse becomes intermitting instead of full. This practice of Mr. Jones is a bold one, but his respectability and knowledge deserve confidence. (Mr. G. M. Jones, p. 59.)

Mr. Jones is supported by Dr. A. Wynn Williams who has also given half-ounce doses of tincture of digitalis in cases where there was great excitement of the nervous centre. He also has given it to avert *epilepsy*, and recommends it to be tried in *puerperal convulsions*. One or two good doses are less hurtful than the continued use of small doses. (p. 62.)

Dr. Ballard thinks that digitalis is a real counter-poison to alcohol, as belladonna is to opium, and *vice versa*. (p. 61.)

Mr. Wood, of Heene, gives a teaspoonful of the tincture of digitalis every hour for three or four hours, and then half this dose every few hours. (p. 62.)

When arising from hard and recent drinking, leave off the stimulants and do not give opium; treat the case more as inflammatory and congestive. When arising in cases long addicted to drinking, and when from some cause the drink has been left off, you may cautiously give opium, after moderate purgation; and should this not succeed, give "a hair of the dog that bit him," in the shape of porter or brandy and water; a little wine and broth may also be judiciously given. (Dr. J. A. Marston, p. 53.)

Headache, *Congestive.*—In severe headache, owing apparently to congestion, give the usual doses of the extract of belladonna. This drug *excites* the ganglionic nerves, *tones* and *strengthens* them, but depresses the cerebro-spinal system. Hence its value in congestive headache. (Dr. R. Hughes, p. 300.)

[We should rather doubt this being the true explanation. There is just as much reason to think that the belladonna is valuable as a *sedative* to both the ganglionic and cerebro-spinal nerves.—Ed.]

Neuralgia.—*Hypodermic Injection of Morphia.*—The hypodermic injection of morphia *is of no use* whatever in cases where active inflammatory

disease (caries of bone) produces the pain. It was of temporary benefit in a case of severe pain originating in inflammation of the nerve itself. The cases best adapted to this mode of treatment are those where the pain is of a purely neuralgic character, depending upon general anæmia. (Dr. J. K, Barton, p. 46.)

*Obstinate Neuralgia.—Electricity.*—It is well known that if an intense current of electricity be applied in the course of a nerve, its sensory power is benumbed for a time, and the same result follows if the nerve is the seat of neuralgic pain. After a time the pain returns, but on repeating the electrisation the recurrence each time at longer and longer intervals, until finally a cure is effected. Sciatica, lumbar, intercostal, crural, and trifacial neuralgia may be thus treated successfully. Nineteen cases of sciatica, many of them old standing and obstinate cases, have been cured by this plan of treatment. (M. Becquerel, p. 49.)

Reflex Paraplegia.—Dr. Brown-Séquard relates a number of interesting cases, showing that paraplegia is frequently produced by irritations starting from very different parts, as the various abdominal viscera, the skin, mucous membranes, and the trunks of nerves. That these irritations are really the cause of the accompanying paraplegia is proved by the following facts:—that cure of the paralysis quickly follows the cure of the irritating cause, whether spontaneous or as the effects of treatment, and that no treatment directed solely to the paralysis has the least effect, until the cause of irritation is removed. The principal diseases which are liable to act thus secondarily upon the spinal cord are affections of the womb, urethra, bladder, kidneys, and bowels.

The various kinds of paraplegia may be classed into two general groups: one in which the amount of blood circulating in the spinal cord is too considerable; the other in which the opposite condition exists. Most cases of reflex paraplegia, and those accompanied by or depending on general debility, belong to the latter; those cases depending on inflammation belong to the former class. Of the various remedies used in these diseases, some diminish the quantity of blood in the spinal cord, such as mercury, ergot of rye, and belladonna; others, such as strychnine and brucine, increase that quantity. These remedies are often indiscriminately employed with positive harm to the patient, whereas the two classes of remedies are adapted respectively to the two classes of diseases.

The urine in an ordinary case of paraplegia from myelitis, is, as is well known, almost invariably alkaline; but in cases of reflex paraplegia, unless depending on disease of the urinary organs, it is usually acid as in healthy people. Paraplegia depending upon inflammation of the membranes of the spinal cord will be chiefly distinguished from the reflex paraplegia by a rigid spasm of the muscles of the back, by the intense pain caused by any motion of the lower limbs or of the spine. Hysterical paraplegia is preceded by hysterical symptoms, but it is in most cases, if not in all, pathologically similar to a reflex paraplegia, both depending upon, or being excited by some uterine or ovarian lesion. There are many other forms of paraplegia which it is necessary to diagnose from reflex paraplegia.

Having ascertained from what organ starts the nervous influence which causes the reflex paraplegia, it is of the greatest importance to try to prevent or diminish the transmission of any nervous influence from the diseased nerve or organ to the spinal cord. If the bladder be primarily diseased, employ narcotic injections into that organ; if the stomach, small intestines, or kidneys be the seat of disease, give the narcotics by the mouth. By applying great cold to the spine we excite the vessels to contraction, but this contraction is always followed by dilatation, and an actual increase of the amount of blood circulated in the cord; and a temporary congestion of the cord may be produced by the patient lying upon his back with his head and legs on high pillows. Those remedies must be employed which will augment the *vital properties* of the spinal cord: the food must be very nutritious and *easily assimilated*, and the paralyzed muscles must be regulary exercised (*Dr. Brown-Séquard*, pp. 29–42.)

*Paraplegia due to White Softening of the Spinal Cord.*—Give 5 grains of iodide of potassium twice a day, mixed with nearly the same dose of sesquicarbonate of ammonia, in a decoction of cinchona bark, or in infusion of calumba rhubarb. Be sure to take this before food in the morning, and an hour before dinner, to avoid the decomposition of the iodide by the gastric juice and the setting free of the iodine. In some cases we may give strychnine, as where the affection is slight, but strychnine produces congestion of the vessels, and if pushed much might cause rupture, and thus increase the evil. Belladonna on the other hand, decreases the quantity of blood in the cord, and ought therefore not to be used unless the physician has that object in view. The patient should sleep on his back and live as well as he can. (Dr. Brown-Séquard, p. 41.)

TETANUS.—*Aconite.*—It is worth while to try the effect of aconite in tetanus, as from some cases related its employment seems to have been followed by some amelioration of the symptoms. In one case five minims of the tincture were given every two hours at first, then increased to eight minims. The improvement in the symptoms dated from the employment of the aconite. The patient was well sustained by beef-tea and brandy. This remedy must be administered very cautiously however, as though it certainly is a powerful nervine sedative, it acts much more powerfully on the heart. Death from aconite usually occurs from syncope. It is cumulative in its action. (Ed. of Lancet, p 45.)

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## AFFECTIONS OF THE CIRCULATORY ORGANS.

ANEURISM.—*Popliteal.*—For a novel method of making pressure in these cases, see Dr. Moffatt's paper at page 157.

*Aneurism by Anastomosis.*—The means usually adopted in the treatment of aneurism by anastomosis frequently disappoint the operator; especially the ligature of the arteries feeding the part. In all cases when it is possible, exercise the part at once, taking care to make the incision only in sound parts. When the disease is situated in the lips, this proceeding may be adopted to the exclusion of all other modes. If this cannot be done, owing to the situation of the tumour, the most successful treatment will be to ligature the supplying arteries, and at the same time to introduce numerous double threads through the substance of the tumour in various directions. Inflammatory exudation into the substance of the tumour will be excited. In some cases it may be well to strangulate the tumour by means of ligatures, and in small tumours astringent injections or caustics may suffice. (Mr. O. Pemberton, p. 158.)

INTERNAL HEMORRHAGE.—*Larch-Bark.*—Dr. Hardy, of Dublin, last year showed the efficacy of larch-bark in cases of purpura hemorrhage (see Vol. xxxix. Synopsis p. 335.) It is a pleasant form of administering a terebinthinate, and is, it seems, not only of use in cases of purpura, but in most cases of internal hemorrhages, or hæmoptysis, hæmaturia, and metrorrhagia. The tincture may now be obtained of most respectable chemists. (Dr. J. M. O'Ferrall, p. 77.)

*Pulmonary Hemorrhage.*—In the intercurrent hæmoptysis of phthisis, the tincture of larch-bark, in doses of half-a-drachm to a drachm every second or third hour is a very effectual remedy, succeeding in some cases when the usual remedies (ipecacuanha, lead, tannin, and gallic acid) had failed. It is now the remedy principally used in these cases, in the wards of St. Vincent's Hospital. (Dr. J. M. O'Ferrall, p. 77.)

VARICOSE VEINS.—Mr. Startin describes a mode of obliterating varicose veins, similar in principle to Mr. Erichsen's plan, by passing pins beneath the vein, differing however in the manner effecting the pressure. A long needle, made of ordinary steel wire, ground to a triangular point, and bent into a curved form in its anterior two inches, is passed behind the vein (which is very easily and safely done owing to its form) and then drawn through, so as to leave *half an inch of the* posterior end projecting. The anterior por-

tion must now be broken off, leaving a small piece projecting, and over the two ends, a clasp is placed so as effectually to compress the vein. In many cases the wire itself will effect this sufficiently without any clasp. A pair of forceps is also made specially for passing the needle under the vein. These instruments may be obtained of Savigny & Co., but any workman can convert steel wire (sizes 18 to 21) into the required needles. A reference to the woodcut p. 161 will render the above description clearer. (Mr. J. Startin, p. 160.)

## AFFECTIONS OF THE RESPIRATORY ORGANS.

CHRONIC BRONCHITIS.—In chronic bronchitis, attended with much viscid bronchial secretion, give the following pills:—R. Ammoniaci, gr. ij; pulv. ipecac. gr. ½; morphiæ mur. gr. ¼; ammoniæ carb. gr. ij; mucilaginis acaciæ, q. s. Misce. Fiat pilula. Coat the pills with a varnish of balsam of tolu dissolved in chloroform, and keep in a bottle. (Dr. J. Williams, p. 100.)

HAY ASTHMA.—*Stramonium.*—The ordinary way of using stramonium by smoking it, is of little value; unless very long continued it will produce little effect. To obtain an immediate effect, the proper and only way is to fill an inverted glass with the fumes of the plant, and inspire the fumes, taking one long and deep inspiration. A momentary sense of suffocation is induced, then copious expectoration of ropy mucus, and immediate relief. If properly used, i. e., brought into absolute contact with the air-cells of the lungs, stramonium is a direct, powerful, and instantaneous expectorant. (Dr. E. Lawford, p. 93.)

HOOPING COUGH.—*Belladonna and Zinc.*—It is a singular fact that children are very tolerant of the action of belladonna. If begun in the smallest doses and given four times daily, it may be gradually increasedup to a scruple or half a drachm daily, without the production of any unpleasant symptoms. If this remedy be combined with sulphate of zinc, and the two given in gradually-increasing doses, an attack of hooping-cough may be very much mitigated. Dr. Fuller, from whose paper the above is taken, states that he had obtained "results exceeding his most sanguine expectations." The whoop rarely lasted more than twenty-one days, and in some instances it had subsided in ten days. If the attack is accompanied by much febrile excitement and bronchial irritation, a cough medicine may be given, containing a drachm of antimonial wine and the same quantity of ipecacuanha wine in two ounces of water; and, if necessary, a blister may be applied to the chest. To children under three years of age, a sixth of a grain of extract of belladonna and half a grain of sulphate of zinc four times daily may be given at first. This must be given as soon as the whoop declares itself, and perseveringly administered in all cases. (Dr. Fuller, p. 98.)

LARYNGEAL DISEASES.—*The Laryngoscope.*—By means of a small metallic mirror attached to a slightly flexible handle, a very good view of the interior of the upper part of the larynx may be obtained. A perforated concave mirror (that used with the ophthalmoscope will answer the purpose) is required also. The laryngoscope being introduced over the tongue, the light of a candle or lamp may be reflected into the larynx by a proper adaptation of the two mirrors, and at the same time, of course, the image of the interior of the larynx will be reflected into the laryngoscope. By means of this instrument, (re-invented by Garcia and Dr. Levin, of Berlin,) we shall probably be able to treat laryngeal diseases with much greater discrimination and success than formerly. (Dr. Czermak, p. 77.)

PARALYSIS OF THE BRONCHIAL MUSCLES.—Spasm of the bronchial muscles is a well recognized pathological condition, of which we constantly *hear; but* paralysis of the same muscles is never mentioned by authors, *and it is ignored* by the practical physician. Yet it undoubtedly occurs, *generally* as a sequel to acute inflammation of the bronchial mucous mem-

brane, in those cases, so often seen, where wheezing respiration and nocturnal dyspnœa are prominent symptoms. In such cases give the extract of nux vomica in half-grain doses, combined with a grain of ipecacuanha three times a day, along with tonic medicines. The effect will often be surprising. (Dr. J. F. Duncan, p. 95.)

## AFFECTIONS OF THE DIGESTIVE ORGANS.

DYSENTERY.—*Creosote.*—In the great majority of cases of dysentery occurring in this country, opium, given by mouth and injection, speedily effects a cure. But in some cases it fails; and it is not, moreover, a drug which can be given in large doses for any length of time. In those cases attended by great irritation and by fœtor of the evacuations, the use of creosote is attended by the best possible effect. The creosote should be given in solution, so as to be applied uniformly and certainly to the whole surface of the diseased mucous membrane. It may be dissolved in glycerine in the proportion of twenty minims to an ounce of the solvent. The creosote may be also used as an injection. Under this treatment the stools will be found to lose their offensive character. Creosote was first recommended in these cases by Dr. Wilmot, of Pembury, who, however, administered it in large doses (℥i.) merely suspended in gruel. (Dr. W. T. Gairdner, p. 101.)

FISSURE OF THE RECTUM.—A very large number of fissures of the rectum are produced by little polypoid bodies, on the opposite side of the gut to the fissure, and fitting into the fissure. "They will be found in almost every case if carefully sought for." The polypus must be removed, and the fissure divided freely with the knife. If the oiled lint be left in the wound for forty-eight hours it is unnecessary to repeat the painful process of reintroducing it. (Mr. I. B. Brown, p. 265.)

*Fissure of the Anus.*—Under the head of fissure of the rectum are included at least three distinct forms of the disease. The first is the true fissure of the anus, a tear or crack in the mucous membrane lining the sphincter ani. The second is invariably coexistent with an enlarged and varicose state of the veins about the rectum. It is originally a small slough, which leaves an ulcer, this increases in size and the edges become hardened and everted. It is usually situated immediately in front of the coccyx. The third form of ulcer is rather a deep excavation at the upper margin of the sphincter. It appears to be caused by the lodgment of a small portion of hardened fæces, or a small polypus may cause this form of ulceration. In this third form there is constant irritation of the adjacent parts, and a certain amount of pus and blood is passed at each relief of the bowels. (Mr. J. Rouse, p. 170.)

HARE-LIP.—Instead of using the ordinary hare-lip pins and twisted silk, it is a better plan to pass silver wire through the parts, fastening the projecting ends by small perforated splints, precisely as the quill suture is used in operations on the vagina for prolapsus, or ruptured perineum. The edges may be brought together much more accurately, three or four very fine silk ligatures only are required to be inserted along the edges of the wound, very superficially. The chief advantages of this mode of operating are that the scar is less than when the pins are used, and the ulceration caused by the pins is avoided. (Mr. E. Woakes, jun., p. 163.)

Mr. Price, of the Great Northern Hospital, has invented a new form of needle, by which metallic sutures may be inserted with the greatest facility through the lips of the deep-seated wounds made in operations for the cure of cleft palate and vesico-vaginal fistulæ. It will be found described and figured at p. 164. It may be obtained of Matthews, Portugal Street.

HYDATIDS IN THE LIVER.—Dr. Budd proposes that, in a case of hydatid tumour of the liver, the cyst shall be tapped by introducing a fine exploring trocar, withdrawing the fluid by means of an exhausting pump, taking the precaution that not a particle of air shall enter the cyst. An instrument for this *purpose is made by Weiss.* Insert the trocar downwards, with the

outer extremity of the instrument at a higher level than the body, so that as the liquid rises in the canula it will expel the air from it. (Dr. Garrod, p. 103.)

LIVER DISEASES.—The healthy action of the liver-cells consists in the absorption of certain albuminous, fatty, starchy, and saccharine matters. When this becomes impaired the cells assume a fermentative tendency, leading to the same description of decay in the albuminous matter as that which is induced by putrefaction, or by treatment by acids or alkalies. Hence some valuable hints respecting the treatment of liver complaints. In jaundice, give nitric or nitro-hydrochloric acid, or a watery solution of nitrous acid made to resemble weak lemonade. The acid destroys and *lixiviates* the deposits of cholochrome and other amido-acids. It also tones the digestive powers. We still want a medicine to *stay* the abnormal action of the liver cells. Creasote is useful, and especially when the disorder proceeds from the bowels; also charcoal, gallic acid, and iron. Nitric acid is often not borne because it is not sufficiently diluted: the more it is diluted, the quicker it is assimilated. Pay great attention in all liver diseases to the bowels: give mild purgatives, of salts, Pullna water, water containing one per cent. of ordinary chloride of sodium and a little sulphate. All mercurials are *bad*. This, we know, is against all medical opinion, but there is much truth in Dr. Thudichum's assertion. Bile contains a good deal of phosphate of soda. Without this salt the assimilation of fats is imperfect, therefore try phosphate of soda in food with chloride of sodium; half a drachm or one drachm per day of phosphate of soda may be given. (Dr. Thudichum, p. 105.)

LUMBRICI.—*Santonine.*—The success following the use of santonine for the expulsion of lumbrici much depends on the method of administration. When given by itself it is scarcely more effectual than Dr. Watson's treatment by steel and purgatives alternately. From two to eight grains stirred in from a tea to a table-spoonful of castor oil, according to age should be administered at night, and a dose of senna the following morning. The effect will be almost specific. When children have passed worms already I have never known the santonine and castor oil, when administered together, fail to eject more of them. (Dr. J. Brisbane, p. 102.)

STRANGULATED HERNIA.—*Chloroform.*—Unless chloroform be given to a very free extent it does not produce perfect relaxation of spasmodically contracted muscle, hence in cases of strangulated hernia it must be given so as to produce a state of perfect coma, as indicated by stertor. If this be produced there are but few cases of inguinal hernia which will not yield to efforts at reduction, if the case is seen sufficiently early. Success to the same extent cannot be hoped for in femoral hernia, as statistics show that this form of hernia requires operation 25 per cent. more frequently than inguinal. (Mr. T. Bryant, p. 167.)

When you are refused an operation, and other means of reduction fail, turn the patient as much as possible *topsy turvy*, as follows: pull the patient to the edge of the bed, place the legs over the shoulders of an attendant, and raise the patient as much as possible on his head and shoulders on the bed, thus throwing the intestines back upon the diaphragm, and to some extent necessarily making traction behind and directly *from* the seat of strangulation. [We should like to read more accounts of this kind of treatment having been tried in such cases before an operation is proceeded with. Where it can be done it certainly seems more reasonable to pull the strangulated intestine away by the weight of the bowels themselves, than to *push* the strangulated portion *back* with the force which has sometimes to be used. Ed.] (Mr. W. Jessop, p. 166.)

STRICTURE OF THE RECTUM.—*Tubular Bougie.*—The instruments usually employed in the treatment of stricture of the rectum are solid bougies. These are much more usual as hollow tubes, so made as to pass one over *another, in* the same manner as Wakley's urethra tubes, for not only are *they more* readily passed, but are less likely to injure the parts. After their *introduction* the rectum above may be washed out with warm water, and

any accumulations so removed without pain to the patient from passage of hardened fæces over the sensitive contracted portion of intestine. (Mr. Tufnell, Dub. Quarterly Journal, Aug., 1860, p. 53.)

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## AFFECTIONS OF THE BONES AND JOINTS, ETC.

Excision of the Knee Joint.—Excluding malignant diseases, there is scarcely any disease of the knee-joint for which excision may not be practised. Some recent experiments of M. Oilier, of Lyons, prove, that if the periosteum be left entire, in the vicinity of joints, it will reproduce bone. Hence the operation may be done for even very extensive necrosis. Recent experience has tended to show that it is better to remove the patella, still this is a subject for consideration. In very young children, it must be remembered that the growth of the limb will be retarded or arrested by the operation. The great inducement to perform the operation, is the fact that the mortality is less than that attending amputation. (Ed. of Lancet, p. 136.)

Operations on the Pelvic Bones.—In operations in which it is necessary to remove a portion of the pelvic bones, as in excision of the hip, with extensive caries of the acetabulum, the operator need be under no fear of exposing the pelvic cavity, as the fascia, muscles, &c., lining the pelvis, in cases of disease of long-standing, becomes so infiltrated with plastic matter, that they form an effectual barrier which protects the pelvic cavity. (Mr. J. Erichsen, p. 140.)

Lateral Curvature of the Spine.—The disease commences as a yielding of ligaments, but very soon the bones themselves become altered in form, the bodies and articulating processes of the vertebra adapting themselves to the curvature. It is obvious that mechanical means alone will suffice to remedy the disease when this has taken place. Steel, and tonic remedies, will not replace a twisted vertebra. Tamplin's apparatus, after many alterations, is the instrument now commonly used at the Orthopœdic hospital. The great success in its use depends on its exercising a gradual, unremitting, but gentle pressure upon the displaced vertebra. It is found that by the use of this instrument the spine can be again brought to a straight line, and this accomplished, it can be so held by stays not differing in appearance from those usually worn. The apparatus will be found described and figured at p. 152. (Mr. H. Coote.)

Loose Cartilages in Joints.—Some surgeons remove loose cartilages at once, while others leave them in the areolar tissues. Experience shows that it is better to leave them in the areolar tissues, or under the skin. The operation for their removal is as follows:—Bring the loose cartilage to the skin as much as possible, straighten the leg, then make a *valvular incision* over or near to the cartilage, and extract it by a scoop, next close the wound, and put the limb in a splint. (Prof. Fergusson, p. 148.)

New Trephine.—Dr. Galt, of Virginia, United States, has invented a new trephine which will readily cut through the cranial bones, but the instant the internal table is divided all action ceases, and it is consequently impossible to wound the membranes. It will be found described at p. 157.

Strumous Synovitis.—There is no doubt but that, in the very first stages of all cases, true inflammation exists, and if seen so early, treatment must be accordingly. But where the disease is established, by far the most successful plan of treatment is by a combination of the use of the actual cautery, followed by pressure. The iron used for the purpose should be that known as Rust's; it is simply a three-sided prism with blunt edges, mounted by one of its oblong facets to a curved handle. When the patient is under chloroform, the iron, heated to a white heat, must be passed slowly and with gentle pressure in lines an inch apart, across the joint, if possible, from *above to below*. *The skin* must not be too deeply destroyed; in prop-

erly chosen cases the effect of this is marvellous, the narrow charred lines made by the cautery came away between the fourteenth and the twenty-first day; they should then be healed as quickly as possible. These lines are afterwards marked by cicatrices, the contraction of which, if aided by strapping the joint, will much favour resolution. Issues, potassa-fusa, mercurials, blisters, and poultices, all favour the growth of the very tissue upon which the disease depends. (Mr. R. Barwell, p. 145.)

STUMPS, *Dressing of.*—In dressing stumps after amputation bandaging is not necessary; the strips of plaster should not be above half an inch broad at the part which is to be in contact with the wound. The *position* of the strips should be varied occasionally, else the parts between the plasters become too prominent. (Mr. F. Symonds, p. 152.)

SUTURES.—Which of the two kinds of metallic sutures (the common wire, and the needle or twisted suture) is the best in amputations, &c.? Union will take place more rapidly when the needle (or twisted) suture is used; it is firmer, and holds the edges together better than the other. Before twisting it round the needle, oil the thread well, and keep it oiled every day, with a camel's hair brush. When the time comes for its removal, the needle will be removed much more easily. (Mr. F. Symonds, p. 152.)

## AFFECTIONS OF THE URINARY ORGANS.

ABSCESS IN THE PROSTATE GLAND.—Surgeons have generally been afraid of making any communication between the urethra and rectum, as in abscess of the prostate gland, &c. They have often preferred making a deep incision in the perineum. Mr. Meade disproves this necessity; you may make a free incision into the prostate from the rectum, when it is necessary. (Mr. R. H. Meade, p. 174.)

ACUTE ALBUMINURIA.—Dr. Corfe states that he has repeatedly seen the injudicious practice of giving stimulating saline diuretics in the *later* stages of the disease, followed by the voiding of hematine and red corpuscles, and final suppression of urine; whilst in the commencement of the case a discreet use of diuretics will be followed by a complete alleviation of the symptoms. In this stage, after a small bleeding so as to affect the pulse, give half an ounce of fresh infusion of digitalis every hour, for three doses; this dose may be repeated at intervals of twenty-four hours, each time followed by copious draughts of nitre-drink (nitrate of potash dissolved in barley-water.) If the disease does not yield, the loins may be dry-cupped daily, and elaterium administered. A good form for exhibiting this drastic is the following fresh extract of elaterium, half a grain; powdered capsicum, two grains; gamboge, three grains: soft extract of jalap. four grains; make two pills. One to be taken at six in the morning, the other at eight; or a grain of the extract of elaterium may be dissolved in an ounce of spirit of nitric ether, and a small spoonful be taken every hour, until the bowels have been well opened: this latter is Dr. Watson's plan. (Dr. G. Corfe, p. 116.)

DIABETIC URINE.—*New Test.*—Add an equal quantity of concentrated sulphuric acid to the urine, and boil. The mixture will froth up, and after the ebullition has ceased the froth will remain permanent. The mixture itself will turn of an inky blackness in colour, and on dilution with water it will be seen that this is owing to the production of small particles of carbon, which will subside as a precipitate. The only objection to this test is that when albumen is present a dark colour will also be produced, but then the colour is a deep brown in this case, and on the addition of water it becomes lighter, and there is no black precipitate. (Dr. J. M'Ghie, p. 123.)

INCONTINENCE OF URINE.—In a boy of fourteen, give one third of a grain of extract of belladonna twice a day in cinnamon water, in those cases where *the affection* is suspected to arise from weakness or paralysis of the sphincter *of the bladder*. Belladonna acts by toning or exciting the ganglionic system, *and depressing* the cerebro-spinal system. (Dr. R. Hughes, p. 395.)

LITHOTOMY.—Professor Smith, of Maryland, describes his operation for lithotomy at page 188. It seems an extremely simple operation, and has been performed with great success. (See an engraving of his instruments at page 188.)

*Allarton's Operation.*—Large calculi and enlarged prostates are no real objections to this method of operating. It simplifies the proceeding to introduce the finger between the director and the sound. It is generally said to be best in the ordinary operation for stone to have a large external wound. It is not necessary in Allarton's operation, a moderate orifice is best. Altogether, this is a most valuable operation; the wound is less, the hemorrhage less, the constitutional disturbance less, and there is no chance of infiltration of urine. (Mr. B. Holt, p. 183.)

PHOSPHATE OF LIME IN THE URINE.—Phosphate of lime is always described by writers as occurring only in the granular form, and along with the ammoniaco-magnesian phosphate, the two possessing a precisely similar pathological import. But phosphate of lime occurs frequently in the crystalline form, and the crystals present well-marked and highly characteristic forms, whereby their identification, by means of the microscope, is rendered easy and certain. The crystals are generally aggregated, forming glomeruli, or rosettes. The figure given by Dr. Golding Bird, in his work on 'Urinary Deposits,' of some "penniform" crystals, which he considered as consisting of a variety of the magnesian phosphate, are undoubtedly phosphate of lime. Other forms also occur. Deposits of phosphate of lime have a much deeper significance than those of the triple phosphate, for the phosphoric acid of this last phosphate, and all the magnesia are derived from *without*, being contained in the various articles consumed as food; while for the phosphate of lime we have in the system—in the teeth and bones, and also in the nitrogenous tissues—sources containing some pounds weight of this phosphate. From direct pathological observation it is found that this deposit, when it occurs, is very apt to be persistent; and when it has disappeared, to return whenever the health is reduced from any cause. It is generally associated with marked impairment of the health, and this where organic disease does not necessarily exist. Dyspepsia, debility, emaciation, nervous irritability, feeble and slow pulse, are symptoms belonging to these cases. The urine from which phosphate of lime is deposited is usually pale, and the sp. gravity varies. It is generally feebly acid, often decidedly so when first voided; it however speedily becomes alkaline. (Dr. A. H. Hassall, p. 131.)

PHYMOSIS, *New Operation for.*—Ricord's operation, which is the one generally practiced in this country, is thought by many to require a too extensive removal of the foreskin, and moreover entails a permanent exposure of the glans. M. Ridreau, a French military surgeon, recommends a new mode of operating, by which a covering for the glans is retained, and a much smaller portion of the textures is removed. The plan of proceeding will be found detailed at page 181.

SOLIDS IN THE URINE.—A very easy mode of determining the amount of solids in the urine is the following: Weigh a piece of filtering paper, then wet it in the urine, as far as it will bear without dripping, and weigh it again—the difference between the two will be the amount of urine employed; then carefully dry it and weigh it again—the difference between this and the weight of the paper before wetting will be the amount of solids present. This mode is susceptible of great accuracy, but without due precaution it may lead to very considerable errors, owing to the drying of the paper being either carried too far, or being insufficient. (Dr. W. Seller, p. 124.)

STRICTURES, *Dilatation of.*—The following plan of treating strictures will be found to involve very little loss of time, and to cause but little inconvenience. Obtain a catheter with about the last two inches prolonged into a conical point (see wood-cut, p. 180), pass this instrument at night as far as it will go, (perhaps only the point is admissible through the stricture), and tie it in its place, *making it exert a little pressure.* By morning it will be found *to have dilated the stricture* to a very great extent. This may be re-

peated once a week, each time using a rather large-sized catheter. The part where the cone commences will vary with the seat of stricture. When the patient is a man engaged deeply in business, it is often of great consequence to avoid confinement and loss of time, and this plan is especially suited for such cases. (Mr. B. Shillitoe, p. 180.)

## AFFECTIONS OF THE SKIN, ETC.

ERUPTION ABOUT THE ANUS IN CHILDREN.—In that obstinate form of cutaneous ulceration about the anus in children, attended often by tumefaction and deep fissures, apply the ordinary yellow wash, gr. i. hydrarg. bichlorid. c. ʒj. aqua calcis. The disorder will be speedily cured. (Mr. J. Bell, p. 193.)

FAVUS.—The inveteracy of favus evidently depends, not upon the difficulty of removing its visible traces, but on the complete infiltration of the scalp with the sporules of the fungus; and no cure can be complete which is not carefully pursued until a complete new growth of scarf-skin has been obtained, perfectly free from all traces of the noxious germs. The hair must be kept shaved, and an alternation of oily applications with soap and water be maintained for a very lengthened period. The simple treatment by oil and soap seems quite as effectual as the medicated ointments and lotions so frequently recommended. (Dr. W. T. Gairdner. p. 192.)

PITYRIASIS.—In P. capitis, in which the hair becomes dry and falls off, one of the best applications is undiluted glycerine. The following is a good application to P. rubra and P. simplex: Oil of almonds and glycerine, equal parts, and one-half of oxide of zinc. (p. 193.)

RODENT ULCERS.—This is not all affected by internal medicines. The great object is to remove the whole of the diseased parts, and to procure a soft and supple cicatrix. There must be no tension on the cicatrix, as this will certainly cause a relapse. You may use deep escharotics, such as the chloride of zinc. In some parts, however, as the eyelids, escharotics are inadmissible: here free excision should be used, followed by transplantation of healthy skin from the neighbouring part. (Mr. J. Hutchison, p. 190.)

SMALL-POX.—*Preventing of Pitting.*—In cases of confluent small-pox, of a typhoid character, simply painting the parts over with a mixture of gutta-percha and collodion, suffices to prevent pitting to any extent. The crusts comes off in large flakes, composed of the dry exudations and the covering material, leaving the skin uninjured. But in cases presenting a more sthenic type, the best plan is to cover the face with a linseed poultice, continuing this application to an advanced stage of the disease. This plan excludes the air, moderates local irritation, and prevents the drying and hardening of the scabs. (Dr. Stokes, p. 193.)

## AFFECTIONS OF THE EYE AND EAR.

ATROPINE.—*Strength of Solution used.*—When it is wished to dilate the pupil for ophthalmoscopic investigation, the strength of the solution used should not be more than half a grain of the sulphate to an ounce of water. This will suffice for the purpose, and the unpleasant effects of a stronger solution will be avoided. (Mr. H. Walton, p. 218.)

CATARACT, *Ophthalmoscopic Investigation of.*—Do not use too strong a light in cases where there is a suspicion of cataract. If the eye be over-illuminated, the striæ in the lens are not seen. (Mr. H. Walton, p. 221.)

CORELYSIS, *or Freeing the Pupil from Adhesions, by Operation.*—As to the prevention of adhesions in cases of iritis, there is no improvement on the old plan by belladonna and mercury. The latter should be administered in frequent small doses, continued some time after the disappearance of inflammation, and no case of iritis should be treated without the use of belladonna. of the iris is not perfectly free at the subsidence of the inflammation, some-

times atropine will cause its separation; but if this does not suffice, the band of adhesion ought certainly to be divided. When the drug is applied to the skin, belladonna itself should be used, rubbed down with glycerine. Atropia is adapted better for application to the eye itself: a quarter of a grain of the sulphate of atropia to an ounce of water, will produce full dilatation of a healthy pupil. A stronger solution acts more rapidly, and its effects last longer, so that if time be very important it may be employed. Pupillary adhesions may actually induce a second attack of iritis, and may moreover protract an established case indefinitely. There is of course no doubt of the optical superiority of a central pupil, and hence of any operative procedure by which the natural pupil is restored, rather than an artificial one formed. The only objection to an operation of this nature can be, that it is possible to tear the lens capsule, and produce cataract: but, in answer to this, it may be said, that out of fifty cases this has only followed once, in one of the earliest, in which the instrument used was not perfectly rounded and well polished. Corelysis (the meaning of which is *pupil-freeing*) should not be attempted until all ciliary redness and signs of inflammation are past; for though it may prevent a return of iritis, it cannot be proposed as a cure. The instruments necessary for freeing adhesions of the iris will be in the possession of every ophthalmic surgeon, except the spatula hook, which is an instrument devised by Mr. Streatfeild, especially for this operation. The pupil is reached by passing the hook through a puncture in the cornea, the puncture varying in situation according to the situation of the adhesion. The operative procedure will be found described at page 194. If the restoration of the natural pupil can be effected, it is a great improvement on the other alternatives—a motionless pupil, or an artificial one. (Mr. J. F. Streatfeild, p. 194.)

Glaucoma.—*Graefe's Operation.*—The leading primary effect of glaucomatous disease is intra-ocular pressure. From this spring a multitude of secondary effects: paralysis of the retina, congestion, inflammatory attacks and their various consequences, and excavation of the optic nerve, all tending to one end—total loss of sight and a spoiling of the tissues of the globe. Hence, in the treatment, all our efforts (failing to cure the cause which produces the intra-ocular pressure) must be directed to obviate the primary effect—the excessive tension. This tension once relieved, the retina recovers itself, more or less, and the circulation tends to its natural equilibrium. The only treatment answering these ends is Graefe's operation, or Iridectomy. It is of the most value when performed early in acute cases. Temporising measures in such cases, as simple puncture, bleeding, mercury, and colchicum, occasion dangerous delay. In all cases, if any sight be left, it should be performed at once; it will often spare this last remnant of vision, and in many cases permanently. (Mr. W. Bowman, p. 202.)

*Mr. Hancock's Operation.—Division of the Ciliary Muscle.*—The appearances of the bloodvessels in glaucoma are chiefly due to the constriction exercised by the ciliary muscle: therefore, to relieve the disease, divide the ciliary muscle as follows:—Introduce a Beer's cataract-knife at the outer and lower margin of the cornea, where it joins the sclerotica. The point of the knife is pushed obliquely backwards and downwards, until the fibres of the sclerotica are divided obliquely for rather more than one-eighth of an inch; by this incision the ciliary muscle is divided. (Mr. H. Hancock, p. 208.)

Myopia.—*Surgical Treatment.*—Mr. J. V. Solomon has found, that, by dividing in a transverse direction some fibres of the ciliary muscle, or muscle of the lens, the focal range of short-sighted persons, whose corneæ are not conoidal, may be doubled in length. The patient is seated in a chair, and the operator standing behind, fixes the globe with the finger of the left hand, whilst with the right, by means of a cataract-knife, he divides successively the corneo-sclerotic union, the pillars of the iris, and the ciliary muscle. (Mr. J. V. Solomon, p. 226.)

Opacity of the Vitreous Humour.—Opacities of the vitreous humour may be divided into those which accompany acute diseases and those which occur *without any other symptom*, or any discernible cause. The latter are

by far the most grave, yet many of these cases get quite well of themselves. The cloudiness which accompanies acute inflammations, whether ordinary or specific, generally passes away with the subsidence of the cause. (Mr. H. Walton, p. 222.)

Rheumatic Ophthalmia.—It may occur that the galvanic current will remove rheumatic pains from the limbs. In such cases it may succeed when the eye is attacked, therefore apply an excited thirty-six link Pulvermacher chain, the positive pole upon the spine, the negative upon the closed eyelid, on going to bed. Let it remain on till morning. (Mr. H. Lobb, p. 227.)

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## MIDWIFERY, AND THE DISEASES OF WOMEN.

Cystic and Adenoid Tumours of the Breast.—*Diagnosis from Cancer.*—The principal point on which to rely in the diagnosis of a cystic mammary tumour from cancer, is the presence or absence of elasticity. A cancerous tumour is entirely devoid of elasticity. Those forms of cystic tumours which consist of a number of small cysts enclosed in a fibrous stroma, do not present this character so distinctly. In all cases where, from this sign, it is suspected that the tumour may be cystic, a fine exploring trocar should be pushed into the tumour:—if the contents be fluid this will be evident; if solid, no harm will be done. It must be remembered that cystic tumours are often associated with cancer. Adenoid, or chronic mammary tumours are of common occurrence, and generally occur before the cancerous age, between the age of 18 and 25. They are lobulated, and non-adherent to the skin or pectorals. There is no cachexy or glandular enlargement, and in fact they appear to be quite local and benign. (Mr. J. Erichsen, p. 256.)

Deodorization *in Obstetric Medicine.*—The oil of tar and lime have been recommended for this object. These will only *retard* the decomposition of organic matter, while "Condy's Patent health powder" will *decompose* it. This powder can be procured at small cost. (Mr. White, Medical Times and Gazette, Sept. 29, 1860, p. 269.)

Flooding after Delivery.—Inject about half a pint of very cold water (iced if it can be obtained) into the rectum; at the same time give cold brandy and water, with one drachm of Curtis's liquor secalis cornuti in it. (Mr. P. Hubbert, p. 244.)

Forceps.—*Dr. Knight's.*—These forceps are a decided improvement of the old instrument. The pelvic curve is less, they have no fenestra, and the blades are smaller and taper more towards the handles. In fact, they *fit the head better* and press less on the external parts when the head is advancing, taking up little more room than the head itself. See engraving and description at page 230. (Dr. S. Knight, of Baltimore, p. 231.)

Metorrhagia.—In cases of uterine hemorrhage from fibrous tumours, carcinomatous growths, or other causes, the tincture of larch-bark is often a remedy of signal service. It is of similar use in cases of hæmaturia. The dose is from half a drachm to a drachm every three hours. (Dr. J. M. O'Ferrall, p. 77.)

Profuse Menstruation.—*Quinine.*—The periodicity of the menstrual process is to be referred for its cause to the ganglionic system of nerves. There is considerable analogy between each individual menstrual flow and an attack of ague. Now, when the process occurs too frequently, every fortnight or three weeks, it may generally be controlled by quinine (unless owing to severe inflammation of the neck of the womb.) The quinine may be given in doses of two or three grains every night or every other night, and if nervous symptoms predominate, it may be combined with sedatives, or if anæmia is present, with iron. (Dr. E. J. Tilt, p. 286.)

*Puerperal* Convulsions.—*Subcutaneous Injection of Morphia.*—Professor *Scanzoni*, of Wurtzburg, considers that a kind of intoxication, produced by *opium, leads* with more certainty to a favourable termination than any other

means recommended in puerperal eclampsia. But, unfortunately, opium cannot always be administered by the mouth, from the comatose condition of the patient, or the rapid succession of the paroxysms. By injecting morphia subcutaneously, however, this difficulty may be overcome, and the effects will moreover be obtained much more rapidly. In one case treated in this manner, after three injections of meconate of morphia, there occurred only two attacks in nine hours, while previously there had been three attacks in an hour and three-quarters. This mode of treatment requires and demands further trial. (Prof. Scanzoni, p. 242.)

Turning in Abnormal Labor.—*New Method.*—If by any means in our power we place the fœtus in a transverse direction in the uterus, with the abdomen downwards, the knees of the fœtus are opposite the os uteri. Even should the back of the fœtus be downwards the case would be rare in which one side or other will not be the most dependent, and consequently one or other knee be within finger reach of the os. It is then obvious, that if in an abnormal presentation (as the shoulder) the fœtus can be made to assume the transverse direction, the labour is at our command. Again, it is obvious from the oval shape of the uterus, that any contraction of its walls, or any extraneous force, however applied, as by the hand of the accoucheur, will be sufficient to determine the presentation of either the head or lower extremities. It is tolerably easy (supposing the shoulder presents) to place the fœtus transversely in the uterus, by employing pressure on the fundus externally, and on the shoulder internally, in opposite directions. The direction in which the pressure is made, and the fœtus made to revolve, will vary according to its original position. When placed transversely in the uterus, as before shown, we have its ultimate presentation at command. A leg may be easily seized if desired, or by a little manipulation the breech may be depressed, and made the presenting part. By this means of operating it is unnecessary to introduce the hand into the uterus, and from this several manifest advantages spring, and several evils are avoided. (Dr. B. Hicks, p. 235.)

Vaginitis.—*Tannin Ointment.*—In cases of simple vaginitis, or superficial inflammation of the neck of the uterus, an excellent local application is an ointment composed of tannin and lard. This may be smeared over a piece of wadding and introduced through a speculum, a thread being attached to it, so that the patient can remove it herself. This does not supersede the use of constitutional remedies as well. (Dr. Foucher, p. 264.

Vesico-Vaginal Fistulæ.—Mr. Price's new form of needle, by which metalic sutures may be inserted with the greatest facility through the lips of the deep-seated wounds made in the operation for vesico-vaginal fistula will be found described and figured at p. 165. It may be obtained of Matthews, Portugal-street.

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## MISCELLANEA.

Chloride of Lime Sticks.—Finely pulverized chloride of lime may be incorporated with gutta-percha softened in boiling alcohol, a warm porcelain mortar being used. The mixture may be rolled rapidly to the diameter of a quill, and kept in a wide-mouthed bottle in powdered lime. These sticks thus remain perfectly hard, and when used the chloride slowly exudes, becoming liquid by the action of the air and the skin. (p. 280.)

Opium, *Administration of.*—No drug varies so much in its action on different patients as does opium. In one case, perhaps ten drops of laudanum will produce alarming symptoms, whilst in another, even six drachms will have very little effect. This depends principally upon the previous habits of the patient; if he or she has been in the habit of taking a glass or two of wine daily, a moderate or even large dose, may be safely given; whilst if no wine or other form of alcohol have been habitually taken, a very small dose will be required. When the system is, so to speak, saturated with alcohol,

the largest doses are required, as in delirium tremens. (Dr. G. Bodington, p. 308.)

Poisoning.—*Modification of Stomach Pump.*—Instead of using the ordinary stomach pump, whereby fluid is forced into the stomach and then withdrawn, it is a much simpler plan to pass a long tube into the stomach, and pour water down into that viscus, then to depress the upper end of the tube, so as to make a syphon, and the fluid will run out of itself. Of course the tube must be sufficiently long to reach below the epigastrium when the upper end is depressed. This plan was, we believe, first suggested by Dr. Pyemont Smith, of Leeds. Dr. Crook, of Northfleet, in a paper published at p. 313, recommends a similar plan, but not one so simple.

*Poisoning by Arsenic.*—A mere trace of fatty matter in contact with arsenious acid, is sufficient to reduce its solubility to one-fifteenth or one-twentieth of what it would be, *cæteris paribus*, without the intervention of the fatty principle. Jugglers are able to swallow with impunity, pinches of arsenic, or solid lumps of that substance, because they have previously taken the precaution of swallowing milk and fatty substances; they afterwards bring the whole up again by vomiting when the company have retired. In cases of poisoning by solid arsenious acid, instantly give the patient a drink of new milk, or administer any other fatty substance at hand. (M. Blondlot, p. 300.)

*Poisoning by Datura.*—Not only is opium antagonistic to belladonna, cases of poisoning by the one having been successfully treated by the administration of the other; but, as appears from an interesting case related by Dr. Anderson, of the Bengal army, opium and stramonium are similarly antagonistic. In his case of poisoning by stramonium, fifteen grains of the muriate of morphia were administered in eighteen hours, with marked success. Probably all narcotic poisons with distinctly opposite actions will counteract each other: for example, the distinctly coma-producing narcotic, opium, will be antagonized by any of the poisonous Solanaceæ; whilst in poisoning by strychnine, conium and hemlock will probably be found of use. (Dr. J. Anderson, p. 309.)

*Poisoning by Phosphorus.—Detection of.*—The smallest trace of phosphorus may be detected by its action on nitrate of silver. A piece of paper moistened in a solution of the latter salt, and suspended over the liquid contained in a closed vessel, will be speedily blackened; if the liquid contain much organic matter, the paper may be moistened with nitro-prussiate of sodium, which will produce a violet colour if phosphoretted hydrogen be present. (M. Scherer, p. 312.)

*Poisoning by Strychnia.*—Tannin is an excellent antidote to strychnia, its action is purely chemical, an insoluble tannate of strychnia being formed. About twenty-five times the quantity of tannin relatively to that of the strychnine, is required to neutralize the latter. But, in cases of poisoning, as much of the tannin will be absorbed by the other contents of the stomach, it will be well to administer a much larger quantity. If tannin be not at hand, gall nuts may generally be procured and pulverized. (Prof. Kurzak, p. 311.)

Give sulphate of zinc emetics freely, apply the cold water douche, and assist by chloroform and camphor. (Dr. Part, p. 313.)

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THE

# RETROSPECT

OF

# PRACTICAL MEDICINE AND SURGERY,

BEING

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CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.


EDITED BY

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LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

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SYNOPSIS—A COMMENTARY ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN—INDEX.

# PRACTICAL MEDICINE.

&c., &c.

## DISEASES AFFECTING THE SYSTEM GENERALLY.

### ART. 1.—ON THE IDENTITY OF THE TYPHOID AND TYPHUS FORMS OF FEVER.

By Dr. JOSEPH BELL, Physician to the Glasgow Royal Infirmary.

[It is at once conceded by the author, that the symptoms of the two affections are not precisely similar; but he contends that the points of dissimilarity are neither so constant, nor so well defined, nor so important as to authorize the conclusion that the two forms of disease arise from different specific causes. Again, the symptoms which are identical are much more numerous and important than those which are non-identical. In another part he asserts that the dissimilarities which do exist are merely differences in degree, arising in consequence of differences of constitutional vigour, or the amount or virulence of the poisonous agency from which the diseases arise.]

Dr. Bartlett has, indeed, supplied the true explanation, when he states "that the blood when drawn from the body has its proportion of fibrine diminished in a degree closely corresponding to the gravity of the affection;" and that in typhus "it is of a dark colour, and non-fibrinous." This fact was pointed out long ago by Andral, and affords a most satisfactory explanation of all the dissimilarities which exist in the symptoms. For example, when the blood is not much impaired, the eruption will chiefly consist of congested papillæ, or at most of the effusion of a little liquor sanguinis, which may have its fibrine consolidated, forming a little conical spot. But when the blood has been nearly defibrinated the eruption will be darker, softer, and more difficult to efface, as fluid blood has been effused. I need not pursue the subject further, because on the slightest reflection it must become obvious that constitutional causes, combined with differences in the amount or virulence of the exciting poison, must influence the symptoms. These facts afford a most satisfactory explanation of the differences which are found to exist between the two forms of fever. So long as the advocates of the non-identity theory accept of this principle as explanatory of the dissimilarities seen in epidemic diseases, they are bound to show how it will not apply to typhus and typhoid fever. This they have never attempted. Again, they ignore one of the best-established principles in medicine, namely, that poisonous and medicinal substances even in the same doses produce dissimilarities in their effects on different patients: and that differences in the amount of the dose is followed by different effects in the same person. These are medical axioms—indeed, they may be denominated self-evident propositions, and are in daily use to explain the differences which we perceive to exist in the phenomena of disease and the results of remedies. Strange to say, the advocates of the non-identity theory keep these facts entirely out of sight in the question at issue, and rest their conclusions on an entirely opposite foundation, namely, that dissimilarity in symptoms proves a difference in the nature of the existing cause.

Again, let us refer to syphilis—how many different phenomena have we resulting from this poison? In one patient ulcerated tonsils; in another lepra; in a third impetigo; in a fourth iritis; in a fifth nodes: in a sixth rupia; in a seventh psoriasis; in an eighth lupus, and so on. Surely these dissimilarities are much *more striking* than those seen in fever, and yet no

one doubts of the identity of their origin. Examine the effects of opium or alcohol on different persons in the same or in different doses, and we have the most startling dissimilarities in the phenomena produced. It is, therefore, a most obvious fallacy to argue that a mere dissimilarity in the symptoms of diseases proves the non-identity of the cause. In the question at issue, the differences are so slight (indeed, often so much so that none can be defined by the most acute and careful observer), that it is almost incomprehensible how any person possessed of the most ordinary intelligence, could for a moment entertain the doctrine of the non-identity of their origin. This becomes still more extraordinary when we reflect on the fact that some of the symptoms which are said to be so dissimilar often become convertible. Thus, for example, the eruption of scattered rose-spots occasionally becomes mulberry in colour and extensively diffused. This I have often witnessed. I have seen a patient labouring under a well-marked attack of typhoid, after a severe epistaxis have the rose-coloured elevated spots converted into dark-coloured non-elevated maculæ, at the same time a large additional number making their appearance, so that in the course of a few hours a case of undoubted typhoid eruption became converted into an unmistakable one of typhus. I have seen the same circumstances occur when the patient's strength became reduced from other causes, such as a severe purge, much undue exertion, &c. Dr. Kennedy of Dublin, has lately drawn attention to this circumstance, and it affords me much pleasure to corroborate his observations. The theory of the co-existence of the two poisons, so ingeniously adduced by Dr. Murchison of London, will not explain away this important fact.

We therefore find, in the first place, that the differences which exist in the symptoms of the two forms of fever are merely those of degree. 2nd, That these dissimilarities are not uniform. 3rd, That the symptoms most dissimilar are convertible into each other. The inference is consequently inevitable, that such differences do not authorize the conclusion that the two forms of disease are non-identical in their nature. The differences which exist denote nothing beyond mere varieties in the manifestation of the effects of the exciting cause—varieties arising from peculiarities of constitution and other concomitant circumstances.

I have examined the bodies of *fifty-one patients* who have died from fever —ten of the typhoid, and forty-one of the typhus form—with the following results:

1. In five who died before the 8th day, more or less congestion or inflammation of the mucous membrane of the small intestines, and deposit in the aggregated follicles were detected.

2. In eighteen who died after the 8th, but before the 15th day, inflammation of the mucous membrane, softening and ulceration of the aggregated follicles, and enlargement of mesenteric glands were observed.

3. In twelve who died after the 15th, but before the 28th day, there was seen extensive chronic ulceration of the mucous coat—in one of these, perforation and fatal peritonitis.

4. In six who died after the 15th, but before the 28th day, death arising from cerebral and pulmonary disease, complete cicatrization had taken place in four instances, and nearly so in the other two; a few small ulcerations still remained.

5. In six who died after the 28th day, extensive thick-edged ulcers existed in the ileum of all the cases, also in the cœcum of two, and in one case the colon was extensively ulcerated.

6. In four who died after the 28th day, complete cicatrization was detected; two of these patients died from cerebral disease, one from extensive bad sores and cellulitis, and another sunk from erysipelas.

Twelve of these inspections were made whilst I resided in the country; six among private patients during the epidemic of 1846-7; and twenty-one were made in the Clyde Street Fever Hospital, under the charge of the late Mr. James Thomson; six were made during the years 1852-3, at the Royal *Infirmary*; one last year, in private (at Patrick); the remaining five are *those narrated* in the number of this Journal for last January.

I beg to remark, that in all the cases placed under the 2nd, 3rd, 4th and 5th heads, cicatrization of numerous follicles was detected. In many it was evident that only some of the glands had degenerated into chronic ulcers. I have never examined the intestines of a fever patient who died from the effects of intestinal ulceration, without observing numerous cicatrices of primary ulcers. This fact has been altogether overlooked by the advocates of the non-identity theory; I beg most respectfully to direct their attention to the matter in their future inspections.

Taking into consideration the results of my own observation of fever, I am forced to the conclusion that disease of the mucous membrane of the small intestines constitutes the anatomical lesion in typhus as well as in typhoid fever, consequently the two diseases are identical in this essential pathological lesion.

I have read very carefully the works of Louis, Chomel, and of numerous other French writers on fever, and I have been unable to discover a single important point in which the disease which they describe differs from the fever that I have witnessed. Be it observed, that neither Louis nor Chomel contend that the fever of France is different from the fevers of this country. On the contrary, the latter undoubtedly regards the fevers of London, Dublin, Edinburgh and Glasgow, as narrated by Tweedie, Elliotson, Christison, Marsh, Stokes and Miller, as identical with the disease which he saw in Paris. In discussing the causes of fever in reference to contagion, he compares the fevers described by the above-named writers with those which prevailed in France, and dwells upon the similarity of their etiology, symptomatology, and pathology; and besides this he always designates the British fever by the term "typhoide," "l'affection typhoide."

It is of great moment to keep in mind that Dr. Williams and many other writers have divided fever into two forms—typhus mitior and typhus gravior; the former corresponding to the first three varieties of Chomel, and the latter with his ataxique and adynamique. It is also worthy of remark that Dr. Williams employs the terms typhoid and typhus synonymously, and it is evident from his work that he was quite conversant with the writings of Louis and Chomel, and it is equally obvious that he regarded the disease which they described as identical in cause, symptoms, and pathology with the disease he witnessed in London—the result of the post-mortem examination in his cases corresponding in every particular with the anatomical lesions found by the French writers in their experience.

We have seen that the result of Dr. Bright's dissections of fever cases exactly coincided also with that of the French Physicians. The same remarks will apply to the experience of Dr. Armstrong. I hold that these unquestionable facts furnish the very strongest argument in favour of the identity of the fevers of London and Paris.

It may be objected, in the first place, that Williams may have been influenced in his views by the works of Louis and Chomel, but this objection will not apply either to Bright or Armstrong, as they both had recorded their observations prior to the publication of the opinion of Louis. With regard to Dr. Williams, it is merely necessary to point to the circumstance, that so far as the question at issue is concerned, it is not his opinions that are of importance, but the *facts* which he records regarding the symptoms and anatomical lesions which he witnessed. These are perfectly identical in every particular with those narrated by Louis. But it may be objected, in the second place, that Drs. Williams, Bright, and Armstrong confounded typhus and typhoid cases together—that they were not aware of the distinctions which have been more recently expiscated by Stewart, Jenner, and others. To this objection it is a sufficient reply, to remark that the description which Bright, Armstrong, and Williams give of the symptoms of the disease, perfectly accords with the disease described by Louis as typhoid. A reference to the cases narrated by these authors will satisfactorily establish the correctness of this statement.

Secondly, all the inspections made by these three distinguished physicians revealed the same anatomical lesions that have been described by Louis.

It must, therefore, be admitted that, so far as the post-mortem experience of Bright, Williams, and Armstrong is concerned, all the cases which they inspected were typhoid. Now, it is the very climax of absurdity to suppose that they could possibly have inspected only the typhoid cases, and overlooked those of the typhus form. Hence we perceive the utter worthlessness of the objection urged by some of the most ardent advocates of the non-identity theory, with the view of invalidating the legitimate conclusions deducible from the observations and experience of Bright, Williams, and other writers on the character of the fevers of this country.

Again, it must be conceded on the one hand that Armstrong and Bright describe a disease which would be defined by the advocates of non-identity as typhus, and on the other it cannot be denied but that disease of the mucous membrane was detected on the inspection of the bodies of those who died from the disease. Farther, in the epidemic of typhus described by M. Reveille Parisse the glands of Peyer were found inflamed. Forget and Cruveilhier found the same diseased condition constantly present in the inspections which they made during the epidemic of typhus in 1814. Pinel reports the same circumstance as occurring in his experience. Hildenbrand also asserts that he found the glands of Peyer always diseased in typhus. Herzog tells us that in the exanthematous typhus which prevailed in the duchy of Posen in 1829–30, he found inflammatory conditions of the mucous membrane of the intestines present, Peyer's patches being tumefied, livid, and with surrounding congestion. Fleury and Pellicot, in their account of the typhus which prevailed at Toulon in 1832, inform us that the inflammatory condition of the intestinal canal was slight and variable, and that no ulceration of Peyer's glands was detected.

Let us next turn to the experience of physicians to whom the doctrine of non-identity was familiar. I beg to direct attention, in the first place, to the experience of the late Dr. Perry of this city. In a paper published in the *Edinburgh Medical and Surgical Journal* for January, 1836, he sums up the results of his careful observation (extending to upwards of four thousand cases and three hundred microscopic inspections) in the form of sixteen propositions; in the last of these he states "that dothinenteritis, or enlargement of the mucous follicles of the smaller intestines and enlargement and ulceration of the aggregated glands of the lower third of the ileum, *occur in combination* with contagious typhus, and are to be met with in about *one in six* of those who die *from typhus*."

I would like very much to know how the advocates of the non-identity theory would attempt to set aside this evidence.

Dr. Stokes of Dublin is the next author to whom I beg to refer. The accuracy of his powers of observation, the extent of his experience, are too well known to permit of any allegations against his ability to discriminate between the symptoms which characterize the two forms of fever. He observes that—

"Many circumstances observable in the epidemic fevers of Ireland, must make us cautious in drawing too strong a distinction between, not only the maculated and the non-maculated cases of fever, but also between typhus—properly so called—and the typhoid disease already specified. It is not to be denied that the characteristic cases of either of these groups of diseases are different; and we have seen that with respect to the condition of the heart, which is the subject more immediately in hand, some remarkable contrasts have been found; yet we cannot help believing that these diseases are but varieties; and that they are results of the same poison or exciting cause acting on individuals in different states; but we need not here enter into lengthened discussions on this point. Let it suffice to indicate generally the grounds of this opinion; they are as follows:—

"1. That epidemics of fever may occur in which, with well marked petechiæ, the follicular ulcerations of the intestine exist in a large proportion of cases.

"2. That in cases of the best marked maculated typhus we have found

occasionally the above anatomical condition, and this at a time when no epidemic disposition to the disease could be supposed to exist.

"3. That we have frequently observed, in cases where a large number of a single family have been successively attacked by contagious fever, that every form of the disease, from the most malignant typhus to the mildest typhoid fever, may be presented by different members of the family.

"Lastly, that a patient who has gone through the typhoid fever in its best marked character may, after an apyrexial period of a few days, be attacked with the most severe form of maculated fever; and conversely, that in certain cases where the first fever was maculated typhus, the symptoms in the second attack or relapse were those of typhoid fever.

"These circumstances, when fairly considered, makes a strong case against the existence in these countries of two essentially different forms of fever; and, if anything was wanting to strengthen this conclusion, it is the great fact that although in the so-called typhoid fevers we are not so often nor so urgently requested to employ stimulating treatment, the general principles of management in both forms of disease are truly the same."

Let me next turn the attention of the reader to the work of Dr. Magnus Huss, of Stockholm. This physician, after twelve years' experience of fever in the Seraphim Hospital of Stockholm, visited the principal hospitals on the Continent, America, &c. After giving a very lucid exposition of the points of similarity and dissimilarity which he has observed to exist between the two forms of fever, he states his opinion to be "that typhus and typhoid fever (typhus petechialis et abdominalis), such as they appear in the north, neither can nor ought to be *considered as two distinct* maladies, but only as two varieties of the same morbid process;" and he adds, "I am persuaded such are the opinions of the majority of Swedish physicians." This may consequently be considered as the opinion of the north on the subject.

Be it observed, that the grounds on which this opinion has been founded are fully given by the author in his introductory chapter. In this chapter the reader will find numerous most important observations bearing on the subject, both as regards the disease in its epidemic and sporadic attacks. But I must refer to the work itself for further information on the subject.

Skoda is the only other author from whom I will at present quote. He thus states the opinion which prevails among the physicians of Germany regarding the identity of typhus and typhoid fevers: "We consider that the disease here is *one*, and ought to be called abdominal typhus; that the typhoid fever of the French is not to be distinguished from what they call typhus; that exanthematic typhus does not constitute a specific affection; and that it is nothing else than the union of their typhus with the accidental appearance of different cutaneous exanthemata."

The preceding facts, collected from writers of all countries, and from men who wrote both prior and subsequent to the promulgation of the doctrine of non-identity, completely coincide with the result of my experience on the subject, namely, that in typhus fever the *same condition of Peyer's glands exist as were found in typhoid.*

I beg briefly to repeat the facts of the case. Williams, Armstrong, and Bright, of London, have found ulceration and disease of Peyer's glands in typhus. Parisse, Forget, Cruveilhier, Pinel, Hildenbrand, Herzog, &c., have found the same ulceration in their inspection of patients dying from typhus. Dr. Perry, of Glasgow (an advocate of non-identity), found *ulceration* of Peyer's gland in the proportion of about 20 per cent. out of upwards of 300 inspections of *genuine typhus*. Dr. Stokes tells us that he has found disease of Peyer's glands in the best marked cases of genuine maculated typhus. Huss states that both he and his brethren of the North of Europe have found the same morbid condition present in genuine typhus. Skoda tells us that he and other German physicians have found the same occurrence in undoubted typhus. I have placed on record in this journal *five* well observed cases of *typhus*, in which the same morbid lesion existed. I repeat that these *five* cases are fair specimens of many others in which the same conditions were found.

I hold, therefore, that these undoubted facts surely justify the conclusion that typhus and typhoid fever are identical as to the pathological conditions of the small intestines; or, in other words, they are identical as to anatomical lesion.

[The writer considers that if it can be proved that the same phenomena accompany the two forms of fever during life, and that the two affections can be traced to a common origin, that then it must be granted that a "complete" proof of their identity has been furnished. Dr. Bell has seen numerous instances of both forms of fever occurring in the same family, and at the same time.]

A young farmer, aged 25 years, was seized with the typhoid form; he had a few elevated, rose-coloured spots on abdomen and chest; his tongue was chopped and red. About the end of the second week, diarrhœa and other abdominal symptoms supervened; these conditions existed during the space of two weeks, after which he slowly recovered. At the end of the fourth week of the illness, his father, aged 58 years, was attacked with febrile symptoms. On the sixth day of the disease an extensive typhus eruption made its appearance on chest, abdomen, and extremities, along with other symptoms of typhus, such as stupor and depression; he required the free use of stimulants, but had neither diarrhœa nor any other abdominal symptom. On the fourteenth day improvement commenced, and his recovery was rapid. During convalescence his daughter, aged 22 years, was seized with fever; on the seventh day her whole body was covered with the dark mulberry rash peculiar to the typhus form; she died on the eleventh day. On the day of her funeral, another brother, aged 30 years, was attacked; the disease assumed the typhoid form, under which he lingered upwards of a month, having suffered severely from diarrhœa. This family lived in an elevated situation about a mile from the village of Barrhead. It was considered that the son who was first attacked, caught the infection from a labourer who had recently recovered from fever.

On another occasion I attended a boy, aged 14 years, during an attack of well-marked typhoid; he suffered much from diarrhœa; became greatly emaciated; his illness lasted upwards of one month. He had three sisters, aged respectively 17, 22, and 24 years; two brothers, the one aged 19, and the other 26. His father was aged 59, and his mother 53. The two youngest sisters and the youngest brother were attacked, though not simultaneously, yet very nearly so (in the course of the third week of the lad's illness), with well-marked typhoid symptoms, viz., scattered rose-coloured papular eruption, diarrhœa, &c. Next week the eldest sister and brother were seized with the disease in the genuine typhus form, and had extensive mulberry eruptions, stupor, &c. The father was next attacked with typhus; the mother was ultimately seized, and died on the ninth day, having presented true typhus symptoms. The daughter, aged 22, also died, but from extensive ulceration of the ileum. The others recovered. Ten weeks elapsed from the first seizure till the death of the mother. The family lived in an elevated situation in one of the cottages attached to Ferneze spinning-mill.

I might quote from my note-books many similar instances, showing, that in the same family, at the same time, and in the same bed, I have had some members labouring under well-marked typhoid, and others presenting equally distinctive typhus symptoms.

In glancing over some of these old dusty pages, my eye has fallen upon a memorandum of a very striking instance of this community of origin. In the year 1841, a young woman came from Glasgow to Barrhead; she obtained lodgings in one of the worst ranges of buildings in the village, the houses being [illegible] greatly overcrowded and badly ventilated. I was called to see her on the [illegible]rd day after her arrival; she had all the symptoms of fever. I immediate[illegible]t th[illegible]mmunicated with the Inspector of Poor, and recommended her remov[illegible]at c[illegible]the Glasgow Royal Infirmary, being convinced, that if she were allow[illegible]he fol[illegible]emain, the disease would spread extensively among the *inhabitants.*

*In these d*[illegible]*in cas*[illegible]ial relief was administered on exceedingly economical

principles. The inspector very kindly visited the patient, but he did not participate in my apprehensions; however, after a little importunity on my part, he granted a line of admission to the Infirmary, but would allow half-a-crown only to pay a cart to convey her there (a distance of seven miles). Of course no person would accept of such an insufficient remuneration, consequently she was allowed to remain. The case turned out typhoid; on the ninth day a few rose-coloured spots (about a dozen) appeared over chest and abdomen; about the twelfth day diarrhœa accompanied with pain and gurgling on pressure in the right iliac fossa. The illness lasted nearly six weeks. Mark the consequences—a girl, aged 20 years, a factory-worker (who lodged in the same room), was seized about the beginning of the third week; a few days later the mistress of the house was attacked, one with typhoid, and the other with typhus. Next, a young man who lived in the opposite room was seized, then his sister and next his father; the children had the typhoid, but the father the typhus form. The disease went on spreading until no fewer than thirty cases occurred in this range of buildings, some presenting typhoid and others typhus symptoms. Let it be observed that there was not a single case of fever in the locality when the girl arrived from Glasgow. After the disease had existed some weeks in the buildings to which she came, it next spread, not only to other parts of the village, but extended to Neilston. So much on the one hand, for the effects of parochial economy, and on the other, for that which is of more importance, our immediate object, viz., the community of the origin of the two forms of fever.

In Glasgow I have not had the same opportunities of tracing cases of fever as I had in the country; but in the wards of the Royal Infirmary, I have seen on several occasions some members of a family labouring under typhoid, and others under typhus. For example, five members of a family were admitted in August, 1853; three had the disease in the typhoid, and two in the typhus form. Again, on the 23rd July, 1859, a girl, aged 23, was admitted on the fifth day of fever; on the seventh day, the mulberry eruption of typhus covered almost the entire surface of her body. Convalescence commenced on the 2nd of August, and she was dismissed on the 16th. A cousin, who lived in the same house with her, was seized with febrile symptoms on the 23rd, that is, seven days after the girl returned home; he was admitted on the 30th; he had a well-marked attack of typhoid, a few rose-coloured spots, which appeared in successive crops, and abdominal tenderness and diarrhœa. He was a month under treatment.

Another girl, aged 19 years, was admitted on the 4th of August, labouring under fever of nine days' duration; her body was covered with a very distinct mulberry rash; bowels constipated. The other symptoms, which occurred in the course of her illness were distinctly typhus. Her two brothers, aged respectively 7 and 14 years, were admitted on the 10th and 14th of August, both having the disease in the typhoid form.

The few cases to which I have now alluded by no means exhaust my experience on the subject—an experience which is far from being unique, or unprecedented in the practice of other physicians. In the last number of this journal, I quoted the result of Dr. Stokes' observations on this point. He tells us "that we have frequently observed in cases where a large number of a single family have been successively attacked by contagious fever, that every form of the disease, from the most malignant typhus to the mildest typhoid fever may be presented by different members of the family."

[The writer then proceeds to consider the objection of those who consider the two diseases non-identical—that these cases are so rare as to be of no moment. Huss speaks of them as of frequent occurrence, and Stokes says that he has frequently observed them. One of Dr. Jenner's arguments is drawn from the exanthemata.]

The proper argument furnished from a consideration of the history of exanthematous diseases, is the very opposite of that deduced by Dr. Jenner. We see that a poison essentially the same produces in different individuals non-identical symptoms, and different pathological lesions, and therefore the differences which we *see between* cases of fever should be considered as

mere dissimilarities in the manifestations of the same poison or specific cause. It is as absurd as it is illogical to argue from the fact that the specific poison of one exanthematous disease will not produce the peculiar manifestation of another, that, therefore, typhus and typhoid fever must each have its own specific cause. The diseases as viewed in the argument by Dr. Jenner, are not parallel; and, besides this, there does not exist the slightest logical connection between the premises and the conclusion.

His second argument is equally fallacious: it is founded on the circumstance that during three years, viz., 1847, 1848, 1849, he ascertained among the patients admitted into the London fever hospital, that only two instances occurred in which typhus and typhoid cases came from the same house and family. He affirms that in every other case the typhoid and typhus patients came from different localities. He therefore infers that the two diseases must arise from different specific poisons.

The fallacy of this argument is very obvious. In the first place it is a well-known fact that during the prevalence of any epidemic, the cases which occur among the destitute and overcrowded localities are much more malignant than those which appear in other places of the same district. 2nd. The period of three years is by far too limited to authorize the general, indeed, I may say, the universal conclusion deduced by Dr. Jenner. 3rd. The two exceptions upset his conclusion. These two cases are *positive* facts, and are of immensely greater value in reference to the question at issue than many negative instances.—*Glasgow Medical Journal. July and October.* 1850, *pp.* 173, 305.

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## 2.—ON ENTERIC FEVER.

By Dr. John Richard Wardell, Tunbridge Wells.

Of course our readers are aware that, by those who have had most opportunity of judging, fevers are divided into typhus, enteric or typhoid, relapsing, and febricula. The two first are the principal and of most importance. Dr. Wardell published some papers on this subject fourteen years ago, founded on the observation of 120 cases of fever in the hospitals of Edinburgh. At that time he believed enteric and typhus fever to be the same.

Enteric fever begins more insidiously than typhus, and much more so than the relapsing. In the latter, the attack is sometimes quite sudden; a person may go to bed comparatively well, and in the morning be seized with shivering, headache, pains in the limbs, and other symptoms. There is less illness and stupidness in the enteric than in typhus, in which the great nervous centres seem [illegible] to be more readily impressed, as if the poison at once affected [illegible]. It is quite true, however, that all forms of continued fever in this [illegible] many of access in present much in common, and [illegible] it is often difficult during the first few days, [illegible] to arrive at a decided diagnosis. In typhus, the [illegible] great prostration not only at the first, but [illegible] the disease the patient lies helplessly on his back.

[illegible] patients have a greater [illegible] the delirium is of a more [illegible] In many cases the delirium [illegible] when it does in the first [illegible] it supervenes before [illegible] cases ends in coma. [illegible] and head complica- [illegible] table giving [illegible] the average, to [illegible] warning symptom in the [illegible] the brain and its mem- [illegible] any such marked ap- [illegible] entering into the [illegible] experience in mor- [illegible] cases.

bid anatomy, it is well known that the encephalic mass gives little explanation of the essential nature of continued fevers. A small amount of subarachnoid and ventricular effusion, a pinkish flush of the cortical substance, or it may be a few puncta in the centrum ovale, constitute often the entirety of morbid phenomena, and I have repeatedly examined this organ when it evinced no lesion. Even coma may not be traced after death. There is no doubt that the views enunciated by the late Dr. Clutterbuck and those professing similar opinions—regarding fever to consist of inflammation of the brain—led to much mischief in practice, and that a more correct pathology (which is, it cannot be denied, an approach to the once widely-received and then discarded humoral doctrine) has been succeeded by a more successful mode of treatment—viz., that treatment which as far as possible dispenses with depletive measures, which is more expectant and conservative in its aims, and which, by having a just relation to a set of morbid actions not primarily located in any single organ or separate tissue, but pervading the entire organism, opposes asthenia. The late Dr. Alison, whose clinical assistant I was, and whose practice I carefully observed, used to dwell with much emphasis upon the Cullenian maxim of "averting the tendency to death;" and although he did not carry the wine and brandy remedy to the extreme which by some it has latterly been advocated, yet he had long seen, with much sagacity, that acute diseases, and more especially fevers, had, from some subtle and inexplicable cause become less sthenic in their nature, demanding remedies which in previous years were accounted inapplicable or decidedly prejudicial.

Those organs and surfaces which are of an eliminative, a depurative, or defecating character—those emunctories and outlets whereby the products of organic waste and effete matters are carried off, are precisely those which are most liable to become affected in the progress of fever. That poison which by a most rapid multiple has so vastly increased as to contaminate the whole of the circulating fluid, in obedience to certain vital laws inherent in the organism, by such channels becomes expelled, hence, according to the power of such agent, the excess or defect of function of such organs and surfaces; and thus it is that the skin, the mucous membranes, and the parenchyma of secernant viscera more especially manifest the effects of the specific poison. Ancient theory and every-day facts convince us of those conservative qualities which the system possesses, whereby it essays to eject what it cannot assimilate, and to get rid of such noxious matter as it may have contracted. In the exanthemata, the skin and the mucous membranes of the air-passages are the chief seats of its determinations; in enteric fever, the pustulation is in the digestive tube, mainly in the ileum; in typhus, the mulberry rash evinces an effort made at the superficies to throw off the poison; and in relapsing fever, the powerful diaphoresis which so frequently at once resolves the fever, inculcates the same doctrine. It now being conceded that the three eruptions peculiar to the respective types of continued fever, are as pathognomonic of those varieties as are the eruptions in the exanthemata, properly so called, it would really seem that it is merely an arbitrary distinction on the part of systematizers and compilers of nosology, whereby they are differently arranged. They all have many features in common; they all originate from peculiar morbid poisons, requiring a greater or less period of incubation; they all pass through a certain train of febrile phenomena; and in all the great centres of animal and organic life are in varied degree affected. Again, there are many points of resemblance in the complications which arise, in the demand of treatment, in the sequelæ, and in the phenomena of the fatal issue. If we were to make a hasty comparison between small-pox and spotted typhus, how many characteristics they manifest of a like nature, still unquestionably caused by poisons, with which like only produces like. Both originate from peculiar poisons, are contagious, require a certain period for development, are diseases in which the prime agent specially operates upon and multiplies in the blood; in one the eruption may be looked for on the third, in the other on the sixth day; in both are all the pyrexial symptoms, as a quick circulation, high temperature

furred tongue, anorexia, diminished secretions, and a great impress is made on the nervous system; in each organic complications of the inflammatory kind may arise; there is delirium and often coma. In the one form, death mostly takes place on or about the eleventh day; in the other, rarely till after the twentieth; and both may have sequelæ attacking the same organs and tissues. And similar comparisons might be drawn between any two types taken from continued fevers and the exanthems. Although human reasoning cannot say why it is that in one variety the pustulation is in the intestines, in another in the skin, yet accumulated observations have long shown, that between the skin and the mucous membrane of the digestive tract there is a peculiar sympathy, and that between the enteric and variolous phenomena many of the fundamental symptoms exemplify no slight or casual features of resemblance.

Dr. Kennedy, of Dublin, in a paper read a few months ago before the Medical and Chirurgical Society, propounds the doctrine that typhus and typhoid (enteric) are mere varieties originating from the same poison. Dr. Jenner's facts are opposed to this opinion. Of relapsing and typhus I can speak with much certainty. Sixteen years ago I maintained from very elaborate data their distinct essence, and such doctrine still holds good. In more than 1200 cases I never saw typhus and relapsing blended. The infection caught from one fever never produced the other. Like always produced like in a multitude of instances. I have given in my papers thirty-two cases, in which the two forms succeeded each other within a short space of time. Seventeen out of the thirty-two who had passed through the relapsing, contracted typhus during convalescence, or within the brief period of three months. The proofs of the non-identity of their essential cause were as clear as the common-sense proofs we have, and as practice ever tells us, of the non-identity of small-pox and scarlet fever. If typhus differs from the relapsing, why may it not differ from the enteric?—*Lancet, Nov.* 17, 1860, *p.* 482.

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3.—*General and Clinical Remarks on Scurvy.* By Dr. STEPHEN H. WARD, Physician to the Seamen's Hospital, Dreadnought.—The treatment of scurvy may be considered under the heads of *curative* and *preventive*. The following is an outline of the plan of curative treatment adopted at the *Dreadnought*:—From two to three ounces of lime-juice, qualified by a proper amount of sugar, are given daily; oranges occasionally when in season; milk and beef-tea, where the state of gums is such as to render solid food inadmissible; otherwise the ordinary diet, consisting of potatoes, vegetable soup, beef and bread. In all severe cases, and in slighter cases where there is marked anæmia, the recumbent position is strictly maintained. This is, indeed, the point of treatment of most immediate moment; the main risk being from fatal syncope, induced by suddenly putting upon the heart more work than it can accomplish. Proper diet and rest are alone perfectly adequate to the cure of the worst forms of the disease. Something must also be set to the account of the moral influence of the sense of security imparted to the minds of the poor sailors on getting into hospital under proper *régime*, and feeling that the exciting and aggravating causes of the malady no longer exist. Special medicine is not, however, quite powerless in the treatment of scurvy. Mr. Corner, when resident medical officer, reflecting upon the service rendered by chlorate of potash in other forms of mouth affection, was led to infer that it might be equally beneficial in the scorbutic disease of the gums. He accordingly tested cases in which it was administered, against others, of equal severity, in which it was not, with the result of decided testimony to the efficacy of the drug in facilitating the cure of the mouth affection. A positive advantage, moreover, is gained by the rapid cure of the gums, in enabling us to put the patients earlier upon the more nutritive solid diet. Another drug, opium, is also useful at times for the relief of sleeplessness and irritability. I should have remarked that the chlorate of *potash did* not seem to have any influence upon the other symptoms of *scurvy*. Doubting the soundness of the premises upon which their admin-

istration was based, and satisfied with their failure in the hands of others, I have not cared to make trial of other salts of potash in this disease, to the exclusion of lime-juice or its analogue. The certainty with which the physician can predict the recovery of the very worst cases of scurvy, even those in which life is all but extinct, is very satisfactory, inspiring confidence in himself, and enabling him to impart it to the patient.—*Lancet, Dec.* 8, 1860, *p.* 557.

## 4.—ON THE ARSENIATE OF SODA IN SCROFULA.

By Dr. Bourguet.

The manifestations of the scrofulous diathesis are all exceedingly inveterate, and the treatment directed for their cure is only too often unsuccessful. All forms of scrofula, however, are not equally obstinate. The tertiary forms, which appear as tuberculosis, are generally incurable; of the other forms, whether primary or secondary, those which affect the bones are especially tenacious and of long duration; and, in truth, it is only over the mucous, glandular, and cutaneous manifestation of scrofula that therapeutics can exert any decided power. The author has used in turn all the medicines usually reputed anti-scrofulous: iodine, mercury, iron, baryta, bromine, extract of walnut leaves, cod oil, arsenic, &c.; but, of all these agents, the arseniate of soda has appeared to him to be, under the circumstances which he describes, the most energetic and the most efficacious. In suitable doses, arsenic is one of the best *tonics* and *corroborants* we possess; it is only in too large doses, or when its use is too long continued, that it deserves to figure as an *alterative*. As a tonic, it is an admirable succedaneum of iron, quinine, and cod-liver oil; and is of great service in the majority of organic and nervous cachexiæ. In the scrofulous cachexia it is an excellent remedy; and under its influence children ordinarily regain their appetite, strength, and colour. In such cases there is amelioration only; but the cases where it cures are those in which the diathesis has not as yet produced a cachectic condition, and where the local manifestations are superficial and confined to the skin, the mucous membranes, and suppurated lymphatic glands. Beyond these, in tuberculosis and the diseases of bones, it is only a good palliative. But, although thus limited, the therapeutic effects have considerable importance. It is no slight matter to be able to abridge the duration of a coryza, an ophthalmia, or the suppurations of glands, cutaneous ulcers, otorrhœas, leucorrhœas, &c., which depend upon the scrofulous constitution. The author possesses a number of facts relating to such cases, as well as to scrofulous perforation of the palate, reputed syphilitic, suppurating cervical glands (formerly called *écrouelles*), &c. The result in all of them was the same; a rapid cicatrization of the sores was always observed. The following are some examples:—

*Obs.* I.—A female child, *aged* 10, entered the Hospital Ste. Eugénie on 3d June, 1858; born of scrofulous parents; pale, thin, and weak, with an open scrofulous sore under the chin, discharging an abundant suppuration; below the angle of the jaw, other enlarged glands, not suppurated; appetite and digestion natural. Arseniate of soda was prescribed in increasing doses, from 5, 10, 15, to 20 milligrammes (1-14th to 1-4th grain) in a julep. Taken from July to December, the medicine produced no disturbance in the digestive organs. On October 15 the child was completely cured, and was dismissed on December 27. The tumours of the neck had disappeared, and there remained a solid cicatrix of good appearance under the chin.

*Obs.* II.—Girl of 13; two large scrofulous ulcers in the neck, with abundant suppuration, and the skin undermined. Arseniate of soda in increasing doses. Cure in three months.

*Obs.* III.—A girl of 10, suffering from osteitis of the phalanges of the big toe, onyxis, and suppuration of the matrix of the nail; treated with arseniate of soda internally, and foot-baths of chlorate of potash. In three months the wound had *completely healed*, although there still remained some swelling

of the toe, and the skin was red and tender. The child had gained colour, had a keen appetite, and had become stout and plump.

The fourth case, a girl of 11, affected with lichen, and presenting numerous glandular enlargements and fistulous ulcers under the jaws, had a chronic abscess of the chest, which had been punctured and injected with iodine. She had been treated with cod-liver oil for three months, but had lost flesh, and the abscesses continued to discharge an abundant suppuration of an unhealthy character. On May 15, the arseniate of soda was commenced. On July 15, the abscesses were almost cured; and the child had a fresh healthy colour, and had gained flesh. On September 1, the abscesses on the neck and chest were completely closed. The cure continuing, the child was dismissed, Nov. 8. Since that date all the scrofulous children presenting superficial cutaneous affections in M. Bouchut's wards have been treated in the same way, and the result has always been favourable.

In the case of a girl, aged 11, affected with swelled submaxillary glands, and large scrofulous ulcers extending from the ear downwards under the chin on both sides, the improvement began after a fortnight's treatment with the arseniate of soda, and she left the hospital cured in four months.

Another girl, the subject of tinea tonsurans and osteitis of the big toe had been ill for a year, and entered the Hospital, July 8, 1858. The general health was pretty good; with the exception of some paleness, there was no functional disorder. Epilation, lotions of corrosive sublimate, applied to the head; arseniate of soda internally; sulphurous baths. In November, 1859, the sore of the foot had completely cicatrized; but the tinea tonsurans had spread. The epilation was continued, and she was dismissed cured on the 15th October, 1860.

In all these cases the arseniate of soda was begun in doses of 5 milligrammes (1-14th grain), and increased at the end of a few days to 10-15, and finally 20 milligrams (1-4 grain). Beyond that dose, symptoms of gastralgia, vomiting, and diarrhœa are apt to come on, and should be guarded against. It may be given in gum julep, in Bordeaux wine, in syrup of cinchona, or syrup of gum. The following formula may be employed, and the medicine left in the charge of families, for use during several weeks;—Syrup of cinchona, 300 grammes (3 x); arseniate of soda, 5 centigrammes (gr. j.); one or two teaspoonfuls each day; each teaspoonful containing about 1 milligramme (1-70th grain) of the arseniate of soda. In this dose, and with the precaution of increasing it gradually, the arseniate of soda presents no danger. Its effects are, to increase the appetite and produce a richer sanguification, manifested by a ruddy colour of the skin, muscular energy, and an unmistakeable appearance of health. Such results, says the author, are not to be despised in the case of scrofulous subjects, pale, emaciated, and exhausted by long suppurations and mucous discharges; and it is on these grounds that he recommends the arseniate. He does not propose it, however, as a specific, but only as a tonic or corroborant, which stimulates the appetite and imparts increased activity to the molecular nutrition of the tissues. In scrofulous constitutions, it is the slowness of the movements of nutrition, and of the exchange of the circulating materials, which gives the diseases their peculiarly chronic character. In this respect the arsenical medication is useful, as perhaps the cod-liver oil is also, by stimulating nutrition; and the results obtained should induce practitioners to have recourse to it. It must be noted, however, that the arseniate of soda is suitable only in scrofulous diseases of the cutaneous, mucous, and glandular textures. Its efficacy is doubtful in diseases of the bones; and it is only a palliative in the case of tertiary scrofula, that is in tuberculization.—*Bull. Gen. de Therapeutique.—Edinburgh Medical Journal, March*, 1861, *p.* 830.

## 5.—ON THE CONNEXION BETWEEN GOUT AND DEGENERATION OF ARTERIES.

By EDWARD CANTON, Esq., Surgeon to the Charing Cross Hospital.

[A great many authors regard gout as a fertile source of atrophy and degeneration of arteries. An opinion of some weight is that of Dr. Hope, who states gout to be "an affection remarkably often attended with arterial ossifications, and their frequent concomitant, angina pectoris." Mr. Canton, however, considers that we may more rationally look to the subversion of normal nutrition of texture, which springs from malassimilation in the digestive process.]

Amongst the diseases which give rise to atrophy and degeneration of the arteries, gout is specified by Otto; and this malady is believed by Andral to be a very prolific source of these changes in the blood-vessels. "Gout," observes Dr. Copland, "generally originates in an excessive use of animal food, conjoined with deficient assimilative and secreting powers of the frame. The highly azotized blood of a person thus circumstanced becomes surcharged with urea and phosphate of lime, as evinced by the state of the urinary secretion, which always, in such cases, abounds in uric acid and the earthy salts. The experiments of Magendie have proved that by changing the diet of a person who has been living chiefly on animal food, and by substituting substances which contain no azote, the uric acid and phosphates disappear from the urine. May we not, therefore, infer that, in consequence of the excessive use of animal food, conjoined with imperfect assimilating and secreting powers, these substances will accumulate in the blood to a hurtful extent, the urinary organs being unable to eliminate them entirely from the circulating fluid? The necessary result of this state of the blood will be, that these substances will occasionally be deposited in other parts, giving origin to the uric acid concretions found in the small joints, and to the phosphate of lime found in the arterial system and some other parts."

While considering the subject of atrophy and degeneration of blood-vessels in connexion with gout, I may devote a short space to a subject which, at the present time, is prominently under the notice of the profession—viz, *glaucoma*, a disease, which, as far as my experience has extended, is closely allied to that particular form of inflammatory action known as *arthritic*. Nor is this opinion, by any means, a peculiar one; for the most eminent opthalmologists, concurrently, agree in their expression of the same belief—e. g., Lawrence says: "Glaucoma certainly occurs more frequently in such persons as are liable to gout than others." The cases related by Sichel are chiefly those of glaucoma, with arthritic inflammation. Mackenzie remarks, that "the subjects of glaucoma often labour under the symptoms of irregular gout, and not unfrequently, have long suffered from those pains in the teeth and head which are generally accounted rheumatic. The Germans appear to consider glaucoma as almost always connected with arthritis, or rather as the result of slow arthritic inflammation of the eye, especially the choroid." Tyrrell believes the disease to take place during suppressed gout, and speaks of the alternation of the two diseases. He relates cases also, showing clearly the association of gout with glaucoma. I have frequently heard the late Mr. Guthrie express an opinion that glaucoma is to be regarded as a gouty affection. "Glaucoma," observes Beer, "and green cataract, considered as consequences of inflammation, belong to arthritic inflammation of the globe; and the same form of disease, when occurring without inflammation, are only seen in the gouty."

Mr. Hulke states, on the other hand, that he has been unable to trace any connexion between glaucoma and gout and rheumatism.

Of the glaucomatous eyes which were dissected by Elbe, one was from a woman who had suffered much from gout, and the other from a man who had frequently experienced arthritic inflammation of the eyes. Of the specimens examined by Rosas, Lawrence remarks—"they must have been cases of

arthritic inflammation of the internal tunics, and chiefly in their chronic stage."

Although I believe the true arthritic nature of an attack of acute glaucoma is not, at present, determined with such precision as we might desire it to be, in order to enable us to speak of the disease as an undoubted example of gout invading the eye, the symptoms nevertheless are very closely allied to those which commonly characterize this latter complaint when elsewhere occurring; the sudden accession of pain of a racking character; the remissions, succeeded by fresh paroxysms; the swelling of superposed parts (chemosis of conjunctiva or tumefaction of the softer tissues of the toe); greatly increased vascularity (of synovial membrane or retina); augmented pulsation of vessels passing to the inflamed part (tibials, or centralis retinæ); effusion of fluid into the cavity of the eyeball or articulation; blood extravasated from over-engorgement of vessels in either case (this may be sometimes seen to have occurred, also, into the sub-conjunctival areolar tissue where arthritic inflammation attacks this membrane).

*In the dissections made of eyes affected by glaucoma, has inquiry been instituted respecting the presence of urate of soda in any of the humours or tunics?*

The impairment of the nutrition of the bloodvessels of the retina is well illustrated by the fact—first noticed here, by Mr. Bowman—of the pouched or aneurismal condition they assume in glaucoma; yielding, and even at last bursting, from diminished power to sustain their contents. "The first observations of this state," remarks Mr. Paget, "were published, I believe, by Kölliker and Hasse, in an account of a case of inflammatory red softening of the brain in which many of what, at first sight, appeared to be points of extravasated blood, proved to be dilatations of capillary vessels filled with blood. After this, they found the same changes, but in a much less degree, in some cases of inflammation artificially excited in the brain of rabbits and pigeons."

In acute glaucoma neither atheroma nor calcification of the retinal vessels exists; but Mr. Hulke observes—"The subsequent changes in the choroid and retina, in the advanced stages of the disease, have an atrophied character. In some cases I minutely examined in the summer of 1856, I found the dilated capillaries of the retina had become dark and granular; they were charged with minute oil globules, and often contained a few grains and little masses of red pigment. In short, these vessels were in a state of fatty degeneration, and the neighbouring parts supplied by them participated in the atrophy, and formed small spots of dull stone-grey colour."—*Lancet, Nov.* 17, 1860, *p.* 480.

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## DISEASES OF THE NERVOUS SYSTEM.

### 6.—ON SOME OF THE MORE UNUSUAL FORMS OF PARALYSIS.

By Dr. William Roberts, Physician to the Manchester Royal Infirmary.

[The most frequent and serious cases of paralysis are those arising from some interruption of the line of communication between the volitional centres and the muscles, from inflammation and its consequences, from pressure by tumours, depressed fractures, effusions, &c. There are other cases about which, until of late years, very little has been known, in which no interruption can be found, (except as a result and not a determining cause). The following groups will be found to embrace nearly all these cases.]

Group 1. Paralysis from mineral poisons, lead, arsenic, copper, &c.
" 2. Wasting palsy (*Atrophie musculaire progressive* of Aran).
" 3. Reflex paralysis—including
Infantile paralysis (*Paralysie Essentielle* of Rilliet).
Paralysis following the endemic colic of hot countries.
Paralysis following zymotic diseases, cholera, dysentery, continued and eruptive fevers, diphtheria, &c.

Facial paralysis, and certain other local palsies.
Hysterical and emotional paralysis.

Group 4. Paralysis affecting the co-ordinating power, (*Ataxie Musculaire locomotrice* of Duchenne).

It is not my purpose, in the ensuing pages, to enter on a full consideration of each of these groups, but merely to offer some contributions to the study of the less known among them in the form of comments on cases that have fallen under my own observation.

The first group of cases to which the reader's attention is desired are examples of Wasting Palsy. The necessity of erecting this class of cases into a distinct nosological species, was first pointed out by Cruveilhier, in 1848, and forcibly advocated in the able Essay of Aran, in 1853.

The characteristic of wasting palsy is a gradual loss of motive power from atrophic degeneration of the muscles, independent of any disease of the brain or cord. The volitional impulses proceed to the muscles without impediment, but the decaying fibres are no longer able to contract in response. The wasting may extend to nearly all the voluntary muscles, both of the trunk and extremities, or be confined to one or more groups in the upper or lower limbs.

This led Aran to divide the cases into two divisions, according as the atrophy was partial or general. Practically it is of importance to keep the two groups distinct. The gravity of wasting palsy, *so far as the part is concerned*, is commensurate with its extent, *but so far as life is concerned*, it depends on the location. So long as the disease is limited to the extremities, life is not imperilled, but so soon as the muscles of respiration are attacked, the prognosis becomes exceedingly grave, for death is the usual result.

General wasting palsy differs also from the partial variety, not merely in the extent and locality of its ravages, but in its course and condition of origin; so that there is good reason, apart from the contrasted prognosis, to consider the two groups as distinct varieties.

Since the publication of my essay, a considerable number of cases of wasting palsy have fallen under my notice; but all except two presented the disease in a partial form. Some of the most interesting of these I shall briefly relate: accompanying the recital with a running comment on the most important points.

Afterwards I propose to bring forward some illustrations of reflex paralysis, especially as it occurs in infancy and childhood, and finally some cases exhibiting loss of the co-ordinating power over the voluntary muscles.

[*Case* 1, is a little boy aged 8, who five years ago, had two fits, at intervals of six months. He continued well, however, till a year since, when his arm was noticed hanging almost powerless at his side. After this the muscle gradually wasted.]

About six months from the debut of the complaint the patient entered the infirmary for the first time. At that period his condition answered to the following description:—He was a well grown lad, standing upright, and walking without the least impediment. The chest and right arm were perfectly developed. The left arm, however, was the seat of a very serious loss of power and *great wasting*. The scapulo-humeral muscles, especially the deltoid, were affected to that degree that there remained scarcely any power to lift the arm from the side. The muscles moving the elbow joint were also deeply injured, but the unweighted forearm could still be flexed and extended completely although slowly and feebly. The forearm and hand were comparatively free, though considerably inferior in bulk and power to the corresponding parts of the opposite limb. The left pectorals were somewhat touched. The general health was not quite satisfactory. There was a languor of mind and body, not usual to the boy; the tongue presented a dirty yellow fur, and the appetite failed somewhat from time to time.

He was diligently galvanized through the affected muscles every other day. A stimulating liniment was also rubbed in on the alternate days, and the citrate of iron and quinine was administered internally.

He resided in the infirmary, undergoing this treatment, for a period of

three months. No advantage was gained; on the contrary the disease made slow but alarming progress. Before his discharge it was distinctly ascertained that the right shoulder was becoming affected; and a slight inclination of the head to the right indicated but too surely that the muscles on the left side of the neck were becoming too weak to withstand the tension of their antagonists. In this state he was sent home at the request of his parents. After an absence of seven months his father brought the patient again; and he was admitted a second time into the infirmary, on February 13, 1860. During this interval the disease had fulfilled the gloomy prognostications entertained at his discharge. The gait had now become decidedly halting owing to a want of power in the left lower limb; the head had fallen over to the right side and rested on the right shoulder, and scarcely any power remained to lift it into the erect position. The spine was twice sharply curved in a reversed direction in the cervical and lumbar regions, in such fashion as to preserve the general equilibrium. The unsightly deformity thus produced disappeared entirely when the patient was held up by the head or stretched out on the bed. The condition of the muscular system is thus described in the notes of the case:—

"The muscles of both upper extremities are wasted extremely from the shoulders to the hand. On the left side the scapular muscles can hardly be said to exist. The deltoid gives no response to the strongest galvanic shocks; the pectorals answer still, though feebly. The little power that remains in the larger pectoral overpowers the scapulo-humeral muscles and serves to draw the upper part of the humerus upwards and forwards; so that owing to the distinctness with which the prominences of bone about the shoulder can be seen, from the atrophy of the deltoid, the head of the bone looks as if displaced upwards and forwards between the acromion and coracoid processes. The anterior and posterior boundaries of the axilla are represented by thin webs of integument. The biceps contracts with scarcely sufficient force to flex the arm, and its antagonist is but just equal to the task of extending it under the influence of galvanism. The brach: Ant: and the flexors and extensors of the wrist and fingers are much in the same state. The thenar and hypothenar eminences are gone and the grip is altogether lost.

"The right upper limb is in a very similar condition, but the disease has not advanced so far; especially in the forearm and hand, which still possess some share of power.

"The head has dropped from atrophy of the left splenius and trapezius and rests on the right shoulder; it has also fallen a little forward.

"The rhomboidei and serratus magnus on both sides are implicated; and the muscular mass in the vertebral groove shows signs of weakened power, particularly on the right side.

"It is easy to understand that with this failure of muscular force the lad has great difficulty, when lying on his back, in raising himself to a sitting posture. He can only do so in fact by first turning himself to the right and then calling forth all the power still left in the right arm; by this manœuvre he just manages to accomplish his object. When going about the ward he pulls the left leg after him with a bad limping motion. The muscular power of the whole left lower limb seems weakened, but there is scarcely an appreciable atrophy of it; the difference between the circumference of the two thighs being not more than 3-8ths of an inch in favour of the right. He frequently tumbles in walking, and when this happens he appears bewildered, or as one of the patients in the ward expressed it 'knocked senseless,' and he lies on his face until some one picks him up.

"The destruction of the muscles of the hands and arms has reduced him to complete helplessness. He can neither dress nor undress alone. Not a morsel or drop can he convey to his mouth—the attendants having to feed him at every meal. The organic functions are altogether undisturbed. The muscles of the face, with those of the eye, the tongue, and the pharynx have entirely escaped. Pain is never complained of. No muscular vibrations have ever *been* witnessed, although the malady has been watched while in active pro*gress for* many consecutive months."

From the total failure of treatment in the first instance, but little expectation was held out that the disease could be cured; but it was hoped that its rapid progress might be stayed, and a state of permanent arrest be brought about, which would at least leave life intact. Cod-liver oil, with the syrup of the iodide of iron, was administered internally; and he was subjected four times a week, either by myself or one of the pupils, properly instructed, to a thorough stimulation with the secondary galvanic current passed through the affected muscles. By patient perseverance, a very notable amendment was brought about. The disease made no progress at any point after his second admission; on the contrary, it has distinctly retrograded in nearly every region. In the course of the last six months, during which the patient has continued, with the exception of a month, an inmate of the infirmary, he has recovered the power and volume of the muscles of the right arm and hand to a very great extent; so that he can feed himself with ease, and there is a very considerable force of grasp. The left is still very feeble, but he can readily lift the hand to the mouth—a movement which he could not attempt when at the worst. The head can be lifted into a nearly upright position by voluntary effort, and I think it could be wholly so, but for the stiffening of the vertebral joints, and the contraction of the muscles on the right side of the neck, from the long continued malposition of the head.

He raises himself from the recumbent position with facility; he walks with confidence, and has lost that constant tendency to tumble, which at one time almost put a stop to his progression altogether. He still limps badly. His general health is now perfect, and the rosy colour is restored to his cheeks.

This case is exceptional in the suddenness of its invasion. Wasting palsy is remarkable for its slow, insidious onset; but here we are told that 'one day' the arm was found powerless at the boy's side; and it is positively asserted that there was no wasting of the limb at that time. When I saw the patient six months later there was a very great wasting, and yet the father was not aware of it. It might, therefore, be questioned whether the muscles were not already affected before the discovery of the palsy. I am not, however, disposed to accept this explanation, but rather to attribute the original loss of power to an attack of infantile paralysis, and to regard the muscular atrophy, afterwards so extensively diffused, as a subsequent affection, related to, but not identical with the original disease.

It will be remembered that the boy had already had, on two occasions, when 3 years of age, a fit accompanied with unconsciousness. The occurrence of these would seem to show a susceptibility of the nervous centres, such as we should expect to find associated with the production of infantile paralysis. When the wasting palsy, however, had fairly started on its own account, it followed the *progressive* course usual with that complaint. The deformity occasioned by atrophy of the muscles on the left side of the neck is very singular, and when the boy was admitted the second time, it was attributed to some organic disease of the cervical vertebræ, producing curvature. Error from this source was at once detected when the patient lay in bed: for then the spinal curvature could be readily undone, and the column restored to its proper shape.

A most gratifying, and unfortunately, far from usual, circumstance in the history of this case is the success of the treatment; and that under a most threatening aspect of the disease. On his second admission the wasting was rapidly extending; it had already deeply affected the neck and the right upper and the left lower limb, and there was every fear that the muscles of deglutition and respiration would not long escape. The daily application of galvanism, however, speedily arrested the onward course, and in about six weeks a perceptible amendment could be observed. That the amelioration was not due to any spontaneous curative process, but to the means employed, was satisfactorily proved by a temporary intermission of the galvanism. The boy went out for a month, during the white-washing of the wards. On his return he appeared to be exactly as he was at his departure. On the resumption of the galvanism amendment again went forward as before.

It may not be without use to point out the precise method in which the galvanic current may be employed with the most advantage in cases of wasting palsy. The important thing is to pass the current through the substance of the affected muscles, and not along the nerves leading to them, nor into the spinal cord. This disease is regarded as a lesion of the nutritive operations in the muscle, and to the muscle itself, therefore, our remedy must be applied. To effect this the two poles of the battery should consist of small brass balls, closely covered with sponges. The sponges, thoroughly wetted, should then be placed over the muscle to be galvanized, in close proximity to each other; that is, at a distance of an inch, or an inch-and-a-half apart, so that the current in passing from pole to pole may pass through the muscular substance. If the brass balls are applied dry to the surface of the skin, there is little penetration into the muscle—the current passing almost entirely along the integument; but when the wet sponges are pressed on the surface, the current passes deeply through the parts beneath. Again, every muscle ought to be galvanized more or less strongly as it has suffered in its nutrition. The more a muscle is atrophied, and its contractility diminished, the more strongly it should be galvanized; but when the contractility begins to return, it is better to moderate the strength of the current. The sittings should not be too prolonged, lest the muscles be exhausted, and evil produced instead of good. No sitting should be protracted beyond ten or fifteen minutes, and the affected muscles should be successively stimulated one after the other, the operator passing rapidly from muscle to muscle, and then returning to each until it has received its appropriate quota of stimulation.

The case we have been now considering belongs to that very serious class, happily very rare, in which the disease threatens to invade all the voluntary muscles, thereby endangering life, as well as destroying the usefulness of the limbs. But very far more common are the cases where the wasting is confined to a single group, or a limited number of groups of muscles in the upper extremities, or more rarely in the lower extremities, and where there is no tendency to a generalisation of the atrophy. Some of these present very odd deformities from the disappearance of the muscles in the afflicted region, while the remainder of the limb has preserved its volume. Sometimes the shoulders and upper arms of a patient are emaciated to the last degree, while the forearms and hands are plump and muscular. There is, at the present time, an example of this variety in the Royal Infirmary, under the care of my colleague, Dr. Browne.—*London Med. Review, Nov.* 1860, *p.* 212.

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## 7.—ON THE DIAGNOSIS AND TREATMENT OF THE PRINCIPAL FORMS OF PARALYSIS OF THE LOWER EXTREMITIES.

By Dr. C. E. Brown-Sequard, F.R.S.

[There are two distinct groups of cases of paraplegia, one distinguished by symptoms of irritation, the other characterised by the absence of these. The symptoms of irritation observed in the former class are convulsions, cramp, twitchings, erection of the penis, formication, and itching. Diminution of temperature, wasting of muscles, œdema, bed-sores, and alkaline urine. In the second class all these symptoms are wanting, and the paraplegia is caused by the white or non-inflammatory softening, or is of the reflex kind. These two groups are, then,]

1st. Cases in which there are symptoms of irritation, with an increased amount of blood in the spinal cord or its membranes; 2nd, cases in which there is an insufficiency of nutrition, and most frequently an insufficiency in the amount of blood in the spinal cord, and also no symptoms of irritation. This division leads to the establishment of the following rules of treatment:

1st. In those of paraplegia in which there are symptoms of irritation, employ those means of treatment that diminish the amount of blood in the *vertebral* canal.

*2nd.* In those cases of paraplegia in which there are no symptoms of irri-

tation, employ those means of treatment which increase nutrition and the amount of blood in the spinal cord.

We are now led to examine by what means these two effects may be obtained—i. e., to increase or diminish the amount of blood or the nutrition of the spinal cord. With these rules in view, we will say a few words on each of the various means of treatment that are known to have been employed successfully in cases of paraplegia.

1. *Belladonna.*—This most powerful remedy has been employed quite blindly in the various forms of paraplegia by French and Italian physicians. The *rationale* of its mode of action is generally so little known, that it is often prescribed in those cases in which, instead of being useful, it increases the paralysis. An eminent author of a very learned work on Therapeutics and Pharmacology declares that "it is quite obvious that it (belladonna) should never be employed in cases dependent on congestion, inflammation, or organic lesion of the nervous centres, until this condition shall have ceased entirely, and nothing left but inertness." The truth is, that it is precisely in cases of congestion or inflammation of the spinal cord or of its membranes that belladonna should be used against paralysis. The mistake made by this most able writer depends in a measure upon the general but erroneous opinion that belladonna is a stimulant of the nervous centres. We will not speak here of its action on the brain; but as regards the spinal cord and the spinal nerves, belladonna, far from being a stimulant, acts in diminishing the vital properties of these organs. As we have already said, belladonna is a powerful excitant of bloodvessels, and especially of those of the spinal cord and its membranes. In consequence of this influence, it diminishes the amount of blood in the vertebral canal, and in so doing produces a relative diminution of the vital properties of the spinal cord and its nerves. It is, therefore, in those cases in which these vital properties are increased that belladonna should be employed. We will sum up the indications to use or to avoid belladonna in saying:

1st. Belladonna is one of the most powerful and reliable remedies that we may employ, in cases of paraplegia with symptoms of irritation of the motor, sensitive, and vaso-motor or nutritive nerve-fibres of the spinal cord, or of the roots of its nerves; in other words, in cases of congestion, meningitis, or myelitis.

2nd. Belladonna is a most dangerous agent, able only to increase the paralysis, if employed in cases of paraplegia, without symptoms of irritation, such as cases of white softening, or of the reflex paraplegia.

2. *Ergot of Rye.*—This powerful remedy has also been employed quite blindly, although its mode of action being better known than that of belladonna, it would have been easier to discern in what cases of paraplegia it is able to be of benefit. Ergot, like belladonna, produces a contraction in the bloodvessels of the spinal cord and its membranes, and, therefore, diminishes the amount of blood circulating in these organs. The following is a summary of the indications and contra-indications to its use:—

1st. Ergot must be employed in cases of paraplegia with irritation of motor, sensitive, or vaso-motor nerves—i. e., in congestion or inflammation of the spinal cord or its meninges.

2nd. Ergot must be avoided as an agent only able to increase the paralysis in cases of paraplegia without symptoms of irritation, such as cases of the reflex paraplegia, or of non-inflammatory softening of the spinal cord.

3. *Strychnine.*—This most powerful remedy is also employed indiscriminately. I know of a large number of patients whose paralysis has been increased by the influence of this medicine. Its mode of action consists in increasing the amount of blood in the spinal cord and its membranes, and, as a consequence, in increasing the vital properties of that nervous centre. Therefore—

1st. Strychnine ought to be employed in those cases of paraplegia in which there is no sign of irritation, or of increase of the vital properties of the spinal cord, such as the cases of reflex paraplegia and of white softening of the spinal cord.

2nd. Strychnine ought to be avoided as a most dangerous poison in those cases of paraplegia in which there are signs of congestion or inflammation of the spinal cord or its meninges. In those cases strychnine can only increase the cause of the paralysis.

4. *Sulphur.*—The *modus agendi* of this medicine in paraplegia is not well known, but it is at any rate certain that sulphur may be employed with advantage in cases of reflex paraplegia or white softening of the spinal cord, in which there is no irritation of this nervous centre. My own experience confirms the assertions of Graves in that respect.

5. *Phosphorus.*—The mode of action of phosphorus is still less known than that of sulphur. German physicians have successfully made use of this agent in paraplegia, probably in cases of insufficiency in the amount of blood in the spinal cord. We must say, however, that we have tried it without any benefit in two cases of white softening of the spinal cord.

6. *Mercury.*—No agent has been more extensively and more blindly used than mercury in cases of paraplegia. It is only in cases with an increased amount of blood in the spinal cord or its meninges that mercury may be used with advantage. Its depressing influence, however, is such that I never employ it except in cases of syphilitic paraplegia. It is decidedly injurious in cases of white softening of the spinal cord, and also in cases of reflex paraplegia.

7. *Iodide of Potassium.*—This remedy is not sufficiently employed in paralysis. It is one of the most powerful agents of absorption of fluids effused in the cranio-vertebral cavity, either out of or in the substance of the nervous centres. It is the only known remedy that may be employed without danger in the various forms of paraplegia. It is especially useful in cases of white softening of the spinal cord, due to the fatty degeneration of the blood-vessels of that organ. It has, more than mercury, the power of producing the absorption of effused fluids in the vertebral canal, and it is decidedly less depressing than mercury. In cases of syphilitic paraplegia its curative influence is sometimes very rapid.

8. *Cantharides.*—This is another remedy, the mode of action of which is ignored, and which is employed blindly in any form of paraplegia. I have tried it without any apparent benefit in cases of paraplegia belonging to the two most distinct forms of this affection—i. e., a case of white softening of the spinal cord, and a case of chronic meningitis. It seems, however, to have been useful in some cases of chronic myelitis.

9. *Stramonium, Hyoscyamus, and Indian Hemp.*—These agents act more or less like belladonna and ergot of rye on the bloodvessels of the spinal cord, and they are, therefore, to be used in those cases of paraplegia in which the amount of blood is increased in that organ. Of course they must be avoided in cases of white softening or of reflex paraplegia. Against insomnia in cases of paraplegia with symptoms of irritation of the spinal cord, hyoscyamus or Indian hemp ought to be employed, instead of opium, the use of which is then dangerous, as it produces a congestion of the spinal cord.

10. *Ammonia, Sulphate of Quinine, and Iron.*—These medicinal agents are sometimes of great service in paraplegia. It is chiefly in cases of insufficiency in the amount of blood and in the nutrition of the spinal cord that they are to be employed; but they may be also used with benefit in cases of chronic myelitis or meningitis, or of spinal congestion, when the symptoms of irritation are not violent, if the pulse is weak and slow.

11. *Cod-liver Oil.*—Some cases of paraplegia have been reported in which cod-liver oil seems to have produced a very great amelioration. We have never employed it alone; but a notable amelioration has taken place in some cases of chronic myelitis, in which we have employed cod-liver oil with belladonna and ergot of rye. In two of these cases, the patients had been treated for five or six weeks without amelioration by belladonna and ergot, and they began to get better only when cod-liver oil was added to the treatment.

12. *Bleeding and Cupping.*—The chronic inflammation of the spinal cord or its meninges does not require bleeding. Except in one case, we have

never seen any decided good effect produced by bleeding in cases of paraplegia owing to chronic inflammation or to congestion. We hardly need say that any loss of blood might prove most injurious in cases of paraplegia in which there is a diminution in the amount of blood in the spinal cord or of the nutrition of that organ, as in cases of white softening or of reflex paraplegia. Dry cupping frequently applied to the spine is an excellent means of treatment in those cases in which there is either an inflammation or a congestion in the spinal cord or its membranes. It would rather be injurious than useful in cases of white softening or of reflex paraplegia.

13. *Blister, Moxa, Issue, Actual Cautery, &c.*—Revulsion, by these various means, is, to say the least, useless in cases of white softening of the spinal cord and of the reflex paraplegia. But these various means are often of great service in cases of myelitis or meningitis, especially when the inflammation is limited to a small extent in the length of the cord or its membranes.

14. *Cold and Hot Douche.*—The hot douche (at about 100°), applied *loco dolente* on the spine, is very useful in cases of inflammation or congestion, and useless, if not hurtful, in cases of reflex paraplegia and of white softening. As regards the cold douche, the cold shower bath, sponging with cold water, &c., they are certainly most beneficial in cases of white softening, or in the reflex paraplegia.

From the above review of the various means of treatment of paraplegia, it results:—

1st. That in cases of paralysis of the lower limbs, with symptoms of irritation of the motor, sensitive, and vaso-motor nervo-fibres of the spinal cord or of the roots of its nerves, the proper treatment consists in the use of some of the following agents or means:—Belladonna, ergot of rye, hyoscyamus, stramonium, Indian hemp, dry cupping, blisters, moxæ, issues, the hot douche, and also sometimes the iodide of potassium, ammonia, sulphate of quinine, iron, or cod-liver oil.

2nd. That in case of paraplegia without symptoms of irritation of the spinal cord or the roots of its nerves, the rational treatment consists in the use of strychnine, sulphur, the cold douche or shower bath, and also of the iodide of potassium, and frequently ammonia, quinine, and iron.

As regards the principal, I might say the essential, agents to be employed against paraplegia, I can now repeat what I have so often said in these lectures, that it is of the utmost importance in the treatment of this affection not to make use of strychnine, belladonna, mercury, &c., before having ascertained positively whether there are or not symptoms of irritation of the spinal cord. Giving strychnine indiscriminately in all cases of paraplegia, as is done by so many practitioners, is a very dangerous mode of proceeding, as it may increase notably the disease to which the paralysis is due. The same thing may be said of the indiscriminate use of mercury, belladonna, cantharides, ergot, &c.

We will not say more here as regards the treatment of the various forms of paraplegia, as we have already discussed at length, in the beginning of this lecture and in the two preceding, the means of improving the nutrition of the spinal cord, of preventing the alterations of nutrition in the paralysed limbs, in the bladder, &c., and also of diminishing the external irritation in cases of reflex paraplegia.

*General Conclusions of the Course.*—Our principal object in these lectures has been to show—

1st. That there is a form of paralysis of the lower limbs entirely distinct from all others, as proved by its mode of production, by morbid anatomy, by its symptoms, and by the influence of a certain mode of treatment, and that this form of paralysis fully deserves the name of reflex paraplegia.

2nd. That the reflex paraplegia may be caused by the most various irritations of the skin, the mucous and serous membranes, the abdominal or thoracic viscera, as well as of the genital organs or the trunks of the spinal nerves.

3rd. That most cases of *paraplegia* can be placed in two groups, entirely

ferent one from the other, according to the existence or the absence of mptoms of irritation of the motor, sensitive, and vaso-motor nerve-fibres. 4th. That most of the therapeutical means to be employed in paraplegia re also to be grouped in two categories, one of which is fitted to those ases in which there are symptoms of irritation, and the other to those cases n which these symptoms do not exist.—*Lancet, Dec.* 22, 1860, *p.* 605.

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## 8.—ON THE DIAGNOSTIC VALUE OF THE SYMPTOMS OF DISEASES OF THE BRAIN.

By Dr. Brown-Sequard.

[The following is an abstract of one of the Gulstonian lectures for this year. Speaking of the diseases of the base of the encephalon, the author observes:]

A very important observation has been made—that all the nerves entering the brain through the base of the skull decussate near their entrance into the encephalon; and after the decussation, most of them send fibres upwards into the brain. Knowing this, we may first determine theoretically what symptoms should be produced by injury of a given part; and then examine how far this theory of symptoms agrees with what is actually observed.

With the exception of the auditory nerve, the cranial nerves springing from the *medulla oblongata* have their origin at the back rather than at the front of this part of the encephalon. All these nerves pass into the grey matter, and there undergo partial decussation. To illustrate the effects of disease on these, according to its seat, the lecturer selected the sixth nerve.

Diseases affecting the medulla just at the origin of the sixth nerve on one side will produce paralysis of the external rectus on the same side as the injury; while there will be paralysis of the opposite side of the body. This agrees with what is observed in practice. But if it be inflammation that affects the origin of the sixth nerve, there will be convulsive action of the muscle supplied by it, and the eye will be drawn outwards. Again, if this nerve be injured after its decussation in the pons Varolii, the external rectus of the other side is paralysed; and the paralysis of the muscle of the eye here occurs on the same side as the general paralysis of the body and limbs. In another class of cases, there may be an injury on one side, small in extent, yet near enough to the point of decussation to affect the nerves on both sides. Here there will be paralysis of both external recti, although one side of the body is paralysed.

Analogous phenomena are observed when other of the cranial nerves are affected. The phenomena of "alternate paralysis" (paralysis affecting partly one side of the body and partly the other) have lately been made the subject of a paper by M. Gubler. M. Brown-Sequard, however, had given an explanation of the fact ten years ago; and even then he did not think he was advancing any new doctrine. The phenomena had also been explained by Romberg and others prior to the appearance of M. Gubler's essay. M. Gubler has collected cases of injury at the lower part of the *pons Varolii*, affecting the facial and trigeminal nerves, and producing paralysis of the parts supplied by these nerves on the same side; while the opposite side of the body has been paralysed. It would appear, however, that M. Gubler has drawn too general a conclusion from these facts; for, if the injury be at the level of the decussation of the nerves, then the paralysis of the parts supplied by them occurs on both sides. Two cases of this kind have been recorded. Again, injury of the pons above the point of decussation of these nerves will produce paralysis of the face on the opposite side—on the same side as that on which the general paralysis of the body occurs.

The effect of injury of the pons Varolii in producing paralysis of the cranial nerves then varies according to its situation. When the injury is *below the* point of decussation, the paralysis is on the same side; when it is [illegible] paralysis is on the opposite side; and when it is between these

so as to affect the nerves just where they decussate, the paralysis of the parts supplied by the nerves involved occurs on both sides.

Are, then, these signs sufficient for the positive diagnosis of paralysis originating in injury of the pons Varolii? No; but there are other symptoms which aid in the diagnosis.

If the injury of the pons be produced rapidly, as by effusion of blood, the breathing will soon be affected. Again, in the paralysed parts there is a remarkable diminution of temperature; and, as a general rule, the presence of an irritation in the pons and perhaps in some part of the medulla oblongata, is denoted by a considerable loss of heat on one side of the body. If, however, there be no irritation, the temperature in the paralysed limbs will be increased. Again, if there be paralysis of the muscles of the eye, attended with constriction of the pupil, there is an irritation somewhere; and, if there be no other obvious source for this, it must be located in the pons. Further, in most cases of simple injury of the pons, the intelligence remains normal; and there is difficulty in deglutition, and in the motion of the tongue. It is remarkable, however, that it is only when the lower part of the pons Varolii is injured, that the function of the hypoglossal nerve is much impaired.

The small size of the *anterior pyramids* in the medulla oblongata at first sight renders it difficult to believe that they are the main channel of the motor fibres in their passage from the brain to the spinal cord. Yet injuries of the other parts of the medulla do not paralyse; while an injury of the anterior pyramid of one side produces complete paralysis of motion of the opposite side of the body. Pathological facts also show that these anterior pyramids are the continuation of the lateral columns of the cord, and that a decussation takes place in them; for injury of the anterior columns of the cord just below the pyramids does n t produce paralysis, while an injury of one lateral column produces paralysis of the body on the same side. The fact of a decussation of motor fibres in the anterior pyramids is also shown in the result of injury of the pons Varolii. If decussation of the nervous fibres did not take place before their entrance into this part, then injury of one side of the pons ought to produce paralysis of the same side of the body; whereas the contrary occurs, as has been shown in sixty or seventy cases.

The symptoms of irritation in the pons and medulla often manifest themselves in the form of convulsions. But it does not follow, because the convulsions may be epileptiform, that the actual seat of epilepsy is here. The medulla is only the starting-point of an irritation; the seat of the epilepsy may vary. The first convulsions are observed to occur sometimes in one and sometimes in another muscle; and occasionally the first result of the irritation is contraction of the blood-vessels of the brain, producing loss of consciousness.—*British Med. Journal, March* 2, 1861, *p.* 230.

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## 9.—ON THE TREATMENT OF NEURALGIA.

By Dr. E. H. Sieveking, Physician to St. Mary's Hospital.

[Speaking of the hypodermic treatment of neuralgia, the writer observes that the dose thus given must be very carefully graduated, and its effects watched, the absorption from the areolar tissue being so much more ready than from the stomach.]

Within a few minutes after the solution has been thrown under the skin, the patient begins to feel the general effects of the narcotic; there is giddiness, followed by more or less drowsiness, and there may be the retching, sickness and prostration after a time, which are occasionally the troublesome effects of the administration of opiates by the mouth. According to the idiosyncrasy of the individual, no less than according to the dose administered, these symptoms vary in intensity and duration; but they have appeared to me to occur more frequently than when similar doses are given by

the mouth. The general symptoms are sometimes so severe as to prevent the patient from submitting to the injection again, even though there may have been great alleviation of the neuralgia. More frequently, however, this is not the case, and the relief afforded by the injection induces the sufferer even to apply for its repetition. With regard to its local effect, I can speak of it in terms of praise. Applied, as advised by Dr. Wood, at the point of greatest tenderness, which, for instance in sciatica, is generally at the sciatic notch, the anodyne does not fail to arrest the pain completely for a time, but it generally requires repetition before long. The irritation arising from frequent injections in the same locality may be avoided by varying the site of injection, and by using very concentrated solutions of morphia. If a solution of four grains to the drachm be used, seven minims and a half (equivalent to half a grain) would be the quantity suitable for an adult. The syringe should have a fine perforated nozzle, which supersedes the necessity of a canula and trocar, and permits of the injection being made with the smallest possible opening. I have not myself employed other solutions than those of morphia for hypodermic treatment, and do not understand how the method can be of very general use for the introduction of remedial agents into the system, as the majority would induce so great an amount of local irritation as to prevent their beneficial operation. This objection would apply strongly to such substances as quinine, which it was proposed in America to introduce in large doses subcutaneously. While I regard the hypodermic employment of morphia as a valuable addition to our therapeutic armamentarium, I do not think it ought to be indiscriminately used either for the purpose of allaying pain or of procuring sleep. As a local anodyne, I consider the endermic application of morphia in the majority of instances preferable; but where this fails, or where the symptoms are so urgent as to require instant relief, there we may have recourse to the syringe.

A very few words may comprise what I have to say regarding chloroform in these cases. For the purposes of anæsthesia it may be administered in severe and enduring paroxysms in the presence of a medical man; the necessity for a frequent repetition, the risk attending its administration in large doses by the patient or his friends, and the brief respite from pain which it affords, prevent the inhalation of chloroform from being largely used in the treatment of neuralgia; as a temporary palliative of course it is excellent, and in the hands of an experienced practitioner it cannot fail to be of much use. Anodyne liniments containing chloroform are in many cases valuable adjuncts in the treatment of neuralgic affections.

Amongst the many topical applications that are made to the unbroken surface there are two upon which I would briefly dwell; the one is belladonna, the other veratria. Of the former I have a high opinion; I hold it to be of more use as a topical application to the unbroken skin than preparations of opium. Whether it is more readily absorbed, or whether it possesses specific powers, I know not; but the frequent, almost daily, service that liniments and ointments containing belladonna have done me justifies the encomium. Veratria too, though not generally employed, is a remedy which exercises a controlling influence over neuralgia; if an ointment containing from one to two grains to the ounce of lard be well rubbed into the seat of pain, the neuralgia is deadened, and generally a rash appears on the part. The following two cases serve as illustrations of the use of this agent:—

*Neuralgia intercostalis.*—W. C.——, a waiter, aged twenty-five, applied for relief in September 4th, 1856, on account of pain in the lower left thorax, to which he had been subject off and on for two years. For the last three months it had been so bad as to force him to leave off work; for a fortnight he has had a cough, owing to a cold. During the last three months there have been frequent night sweats. There is a total loss of appetite, but no nausea or sickness. At times he suffers pain after eating, but not always. *There is* frequent palpitation, and pain on slight exertion. Four months ago *he had a swelling* of the right leg for three days, which has not returned.

His face is large and bloated in the morning. The urine is dark, but there is no pain in the back. There is no trace of heart disease. The tongue is covered with a white fur; the pulse 92, feeble. The patient had a urethral discharge nine years ago, but never had primary chancre or secondary syphilis; though troubled with frequent nocturnal emissions, is not guilty of masturbation. The bowels have lately been relaxed. The pain is confined to the cartilages of the false ribs, which seem slightly enlarged on the left side. An ointment, composed of four grains of veratria to two ounces of lard, to be rubbed into the painful parts morning and night; an ounce and a half of rhubarb and magnesia mixture to be taken every morning.

Sept. 16th. The ointment has been rubbed in three times a day with great relief to pain; it produced no rash or other visible effect on the surface. Tongue white; appetite poor. Ordered fifteen grains of citrate of iron to an ounce of the compound infusion of gentian three times a day; and to repeat the ointment.

19th. There was a slight return of pain last night. Twenty grains of the sesquioxide of iron with treacle were given thrice a day.

26th. Has had the pain three times for one or two minutes since his last visit. His general health is much improved. The ointment has been rubbed in regularly; has caused the skin to be numbed, but has produced no rash. Repeat electuary and ointment.

Oct. 3rd. Has no pain. Repeat electuary.

17th. There is still some palpitation. Ordered infusion of digitalis and quinine mixture, of each half an ounce, three times a day.

Nov. 7th. Is much better; has had no pain for three weeks, but at times there is still palpitation. Apply belladonna plaster to the left thorax, and repeat electuary.

He continued under treatment until November 28th, at which time there had been no return of the pain. He was therefore discharged cured.

This is a genuine case of neuralgia, in which it was necessary in the first instance to eliminate the possibility of organic disease before determining upon the treatment to be adopted. Had a lowering system been pursued, the neuralgia would have been perpetuated, if not actually converted into an organic disorder. Had any doubt existed with regard to the diagnosis, the effect of the treatment must have dispelled it; while the benefit of the local application of veratria also stands out in relief, as it was employed in the first instance without any internal anti-neuralgic remedy. I subjoin one more case illustrative of the use of veratria.

*Neuralgia faciei.*—H. H., aged 39, the wife of a mason. She had had rheumatic fever four years before applying for aid, and has been in feeble health ever since. For six weeks past she has suffered from pain in the left side of the face. There are no carious teeth. The pain commences in the left lower incisors, and shoots to the neck and ear. It is worse by day, and is brought on by the least exertion. The patient is nursing a baby of three months old, and has a cough with expectoration, but there is no thoracic mischief. Pulse 80.

June 24th. Ordered, tincture of iron, fifteen minims; chloric ether, twenty minims; quinine mixture, one ounce: thrice a day; and five grains of compound colocynth pill every night.

30th. The face is much better. She has had no pain since the second day of taking the medicine, and there is a better supply of milk.

The patient continued the same treatment to July 18th, when she is reported as having a return of the pain in the left side of the face, which, although less than formerly, is still very severe, and causes the eyes to water. The veratria ointment to be rubbed into the seat of pain every night. Repeat the mixture.

July 29th. She states that the ointment "seemed to draw the pain out of the face:" that it caused a smarting pain for about half an hour, and a slight rash on the surface.

She continued, with slight variations, under treatment till Sept. 5th, when the report is that the veratria has brought out a rash on the face, and that

she had no severe attacks since employing it. She still has a slight pain flying about the face; and if she takes anything warm into her mouth she experiences a pain in the right side. Quinine mixture was now ordered, and the patient ceased from her attendance.

In this case we have to deal with a variety of debilitating causes, each sufficient to serve as a substratum for neuralgia. These had to be met by general tonics. That the local disorder was successfully combated by the local application as well, scarcely admits of a doubt, unless the patient's words are discredited.—*Lancet, Feb.* 9, 1861, *p.* 131.

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## 10.—ON CHOREA.

By Dr. C. H. Jones, F.R.S., Physician to St. Mary's Hospital.

With regard to the essential nature of chorea, it seems to me that we shall not be far from the truth if we keep in mind the well-established law, that usually mobility and debility of nervous tissue go hand in hand, that the weak structure is unduly sensitive and excitable. To determine the exact seat of the affection is not possible; and very probably it is not confined to any one locality. It is much more important to know the state of the nervous structures; and this, it seems pretty certain, is one of debility and exhaustion. This is the radical and necessary condition; but the causes which produce it may be very various. In the majority of cases, the nervous tissue falls of itself into a state of enfeeblement, or is reduced to it by ordinary influences, as bad air, poor food, depressing emotions, &c. In other cases, the impairment of nervous energy depends on worms or accumulation of fæces in the bowels, which, on the principle of inhibitory action or morbid stimulus, depress and enchain the nerve-force. In other cases, possibly some poison, by circulating in the blood, may conditionate the state of nerve-debility. The treatment must, of course, vary according to the presumed cause.

The connexion between chorea and rheumatism seems to be in some degree elucidated by viewing the latter as a disease in which the nervous system is materially involved. Without denying that acute rheumatism may depend on the presence of lactic acid in the blood, it yet seems clear that the ordinary inflammations of rheumatism manifest peculiarities which can scarcely be accounted for except by the intimate concernment of the nervous system. Rheumatic affections shade, in fact, from the most decided severe, local, pus-generating inflammations of pericardium or lungs, down to mere neuralgia with little or no vascular disorder. The more fugitive and shifting they are, the more of the nervous element one traces in them. This is not the place to discuss at length the arguments for this view; but if it be admitted that a state of paresis of the nervous system is common both to chorea and rheumatism—in one the cerebro-spinal department being affected, and in the other the vaso-motor—we have at once an important relation established between them. The inflammations of rheumatism might result from the mere paralysis of the articular vaso-motor nerves, without the presence of lactic acid in the blood; or assuming that this is essential, its injurious effects might be greatly intensified by this nerve-disorder, or even developed in cases where otherwise they would have remained wanting.—*Brit. Med. Journal, Jan.* 5, 1861, *p.* 5.

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11.—*On Chorea.*—[Chorea is a disease of extreme frequency at Birmingham. In thirty-five cases which occurred during four months at the Birmingham General Hospital, a rheumatic history was discoverable in all. A fact of great interest frequently observed, is the localisation of chorea and rheumatism in the same limb.]

It may be interesting to state the treatment of the General Hospital, and which still remains without much change. Decoction of aloes (compound) *was first given*, thrice daily, in a quantity sufficient to move the bowels two

so as to affect the nerves just where they decussate, the paralysis of the parts supplied by the nerves involved occurs on both sides.

Are, then, these signs sufficient for the positive diagnosis of paralysis originating in injury of the pons Varolii? No; but there are other symptoms which aid in the diagnosis.

If the injury of the pons be produced rapidly, as by effusion of blood, the breathing will soon be affected. Again, in the paralysed parts there is a remarkable diminution of temperature; and, as a general rule, the presence of an irritation in the pons and perhaps in some part of the medulla oblongata, is denoted by a considerable loss of heat on one side of the body. If, however, there be no irritation, the temperature in the paralysed limbs will be increased. Again, if there be paralysis of the muscles of the eye, attended with constriction of the pupil, there is an irritation somewhere; and, if there be no other obvious source for this, it must be located in the pons. Further, in most cases of simple injury of the pons, the intelligence remains normal; and there is difficulty in deglutition, and in the motion of the tongue. It is remarkable, however, that it is only when the lower part of the pons Varolii is injured, that the function of the hypoglossal nerve is much impaired.

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The symptoms of irritation in the pons and medulla often manifest themselves in the form of convulsions. But it does not follow, because the convulsions may be epileptiform, that the actual seat of epilepsy is here. The medulla is only the starting-point of an irritation; the seat of the epilepsy may vary. The first convulsions are observed to occur sometimes in one and sometimes in another muscle; and occasionally the first result of the irritation is contraction of the blood-vessels of the brain, producing loss of consciousness.—*British Med. Journal, March* 2, 1861, *p.* 230.

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hypodermic injection of morphia was used, one-third to half a grain of the acetate being given once or twice daily, according to circumstances. Subsequently, aconitine was administered in the same manner, a forty-fifth to a thirtieth of a grain being injected under the skin of the arm. The diet was liberal; beef-tea, brandy, and wine, as much as could be taken.

The disease never became very acute. She had trismus, was not able to separate the teeth more than half an inch, well-marked risus sardonicus, twitchings of the face, and spasmodic catching of the breath and of the arms; but no general spasms, opisthotonos, &c. She continued for about a fortnight in nearly the same condition, then all the symptoms gradually and slowly abated, and she was discharged from the hospital, quite well, on the 12th of June.—*Lancet, Dec.* 8, 1860, *p.* 562.

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14.—*Treatment of Epilepsy.*—Dr. Brown-Sequard generally commences the treatment of epilepsy by belladonna. The usual dose of this remedy for an adult is one quarter of a grain twice a day in pill or mixture. It is very rarely indeed seen to produce any of its specific effects, as dilatation of the pupil, in cases of epilepsy. One patient complained of dimness of vision, whose pupils were evidently dilated by the drug; but this was the exception, proving the rule, as the case was not one of epilepsy or any convulsive disorder.

In the cases in which there appear to be a tendency in the fits to appear at regular intervals, for instance once a fortnight, Dr. Brown-Sequard prescribes quinine in large doses, *e. g.*, five, ten, and even fifteen grains, to be given at intervals, shortly before the fit is expected. By this means the fit is frequently prevented, and the patient goes on to the next, or even to a longer period. In reference to these large doses of quinine, it is well known that some temporary deafness will often follow, and curiously enough, Dr. Brown-Sequard states that there is a kind of deafness which the administration of this remedy in large doses will cure.

Another therapeutical means in epilepsy is the ligature, in cases in which the aura epileptica, arising from one of the limbs, is present. Dr. Brown-Sequard has two patients, both girls, about the age of nine years, in the hospital, in whom the fits are frequently stopped in this way. The ligature is kept constantly on the arm; when the child feels the warning, the nurse of the ward tightens the bandage, and the fit is prevented. The success in these cases has been very great. It is of great consequence to have the ligature in readiness, so that it may be tightened at once. Grasping the limb tightly will do in the absence of proper means, but it is much better to keep a bandage or folded handkerchief tied on the arm ready to be tightened. Dr. Brown-Sequard has invented an apparatus to encircle the arm, and to tighten by a screw, in order that the pressure may be quickly applied.

Dr. Brown-Sequard frequently uses the actual cautery locally in a variety of nervous affections. In epilepsy, patients frequently complain of either a pain or a sensation proceeding from some part of the body. A woman, aged 20, had had fits for thirteen years; they invariably commenced with pain in the left side, just below the mamma. Dr. Brown-Sequard applied the cautery to this part in two or three places. The relief was most marked. It had not prevented the fits altogether, but it had reduced their number very considerably. Instead of having them every other day, she had them only once a week. The cauterising iron is heated to a white heat, and is then applied suddenly to the part once or twice. It appears to cause but trifling pain, and the patients do not seem at all to dread its repetition.—*Med. Times and Gazette, Oct.* 27, 1860, *p.* 407.

## DISEASES OF THE ORGANS OF CIRCULATION.

### 15.—ON PERICARDITIS.

By Dr. W. T. Gairdner, Physician to the Royal Infirmary of Edinburgh.

[Friction-murmur *per se* can only be taken as evidence that the pericardium is roughened, not that it is inflamed or actively diseased. There can be no doubt that the white-lymph patches and their allied fibrinous deposits in the pericardium, at some period of their progress, produce murmur.]

During upwards of six years of almost constant attendance in the Edinburgh Royal Infirmary as physician, I have of course had under my care a great many instances of pericardial affections arising in the course of rheumatism, and a certain number also concurring with other constitutional or local diseases. Purely idiopathic pericarditis I have rarely, if ever, witnessed; and I am much inclined to believe that (as morbid anatomy teaches) it very rarely occurs as a severe or clinically important form of disease. Pericarditis from direct injury, or from purulent affection of the blood after injuries; pericarditis following erysipelas, or pleuro-pneumonia; chronic or subacute pericarditis in connexion with unhealthy suppurations in various parts of the body; with epidemic fevers, especially small-pox and scarlatina; and, finally, pericarditis associated with Bright's disease, have all fallen more or less frequently within my observation, and in many of these last-mentioned forms the issue has been fatal. But of *rheumatic pericarditis* and of *pericarditis without previous disease*, acute or chronic, *I have not had under my own personal care a single fatal case.*

Let me endeavour to indicate, shortly, what has been my usual course of proceeding in dealing with such cases. In every such case of rheumatic disease, whether acute or chronic, I have made, as a rule, at the commencement of treatment, at least one or two very complete investigations of the cardiac sounds and impulse, to be used as testing observations for the future. I have also, at the same time, carefully investigated the habitual and the existing state of the cardiac function, and the history of any uneasy sensations or functional phenomena indicating disease, which may have been present at a former period. This done, and the heart being found free from suspicion, I have in future observations dwelt as lightly as possible upon the local examination of the heart, merely assuring myself, from time to time, that there was no very material change requiring more systematic attention.

If a murmur has been present on admission, it has not been too hastily assumed to be a new morbid process, unless accompanied by pain, or by the signs of effusion, or by marked tenderness on pressure, either over the costal cartilages or in the epigastrium. I have not thought it too much to wait for twenty-four or forty-eight hours, before commencing treatment, for corroborative evidence of the existence of true pericarditis in a doubtful case. Of course, careful watching, and examination at least twice a-day, has been the rule in all suspicious cases.

But in many cases of murmur, even when decidedly originating under observation, it has been my practice to look for something more than murmur as an indication for commencing the treatment addressed to an acute pericarditis; and this for two reasons. The alarm created by the announcement that the heart is wrong, is, in susceptible subjects, a serious objection to making this announcement on slight grounds. Having, accordingly, the clear conviction that many friction-murmurs, unaccompanied by signs of effusion, or by cardiac symptoms, may be safely neglected; and being further of opinion that, as a general rule, the treatment of the rheumatic condition of the system is the best treatment also for the pericarditis, I have been slow to give effect to anticipations of evil founded on the presence of a mere murmur, particularly if slight and narrowly circumscribed in locality. The immense majority of such murmurs are found to present themselves over a part of the right ventricle, between the third and the fifth costal cartilage, and I have rarely found them, except when accompanied by tenderness on

pretty *firm* pressure, or by signs of effusion, or of marked excitement of the heart's action, or by a short dry cough, or by cardiac oppression and angina, give any cause for permanent uneasiness.

It is consistent with my observation, though opposed to the statements of several authorities, that one or other of the above-mentioned symptoms is rarely wanting when signs of effusion, even to a limited extent, are superadded to those of murmur. In fact, I more and more tend to disbelieve in *really acute pericarditis, apart from vital phenomena or symptoms;* although many cases called pericarditis may have been insidiously developed, and have escaped attention till a late period, either from inattention, or from their not being really acute in the sense of demanding treatment. Among the symptoms mentioned, the dry short cough is perhaps the one most likely to attract attention, and should always lead to the suspicion of pericarditis, when not explained by the state of the lung. Moreover, the symptoms mentioned are occasionally developed as the earliest phenomena of the disease, being succeeded by the murmur within two or three days, as has been shown by Dr. Mayne, of Dublin, and Dr. Stokes.

With regard to the treatment of pericarditis actually pursued by me in hospital practice, I shall keep in view chiefly the rheumatic form, inasmuch as most of the others present little room for remarks tending to any satisfactory result.

In saying that rheumatic pericarditis will usually end favourably under various methods of treatment, I am very far from wishing to depreciate the value of medical practice in this disease. No doubt, the field within which our operations are to be restricted is more limited than it has been supposed to be by those who think that a particular method is a *specific* against inflammations in general, or pericardial inflammation in particular. But I trust that few educated members of the medical profession will think the worse of any treatment, because it does not claim the character of a specific method. What I argue is, not that treatment is of no use, but that the general and constitutional treatment applicable to rheumatism should over-rule the means addressed to the local disease, except within the very narrow limits which I shall presently point out.

I find it impossible to arrange the facts of my experience in this matter in the statistical form. For, in the first place, I do not know how many cases of really acute pericarditis I have treated; and in the present state of our knowledge, as indicated in a preceding part of this paper, it may safely be said, that every attempt to number such cases must involve elements of most serious fallacy. But, in the second place, I do not know how many of the cases I have treated have got well in the end, and looking to the remote consequences; some of them, for instance, may have recovered for the time with more or less of adhesion of the pericardium. Of this only I am certain, that as a physician of the Royal Infirmary, I have not had under my care a case of rheumatic pericarditis fatal during the acute period of the attack.

Let me try to turn this fact to its proper use, as regards what may be called the heroic methods, bleeding and mercurial salivation.

I believe that the profuse employment of blood-letting in inflammations has long been diminishing, and that in Edinburgh, at least, it is pretty nearly extinct. I shall not, therefore, occupy space in demonstrating that that is a wrong thing which, to the best of my knowledge, very few indeed are in the habit of doing. For many years past, I have not heard that any physician in Edinburgh has used a lancet in rheumatic pericarditis. At all events, I am very sure that the lancet is used most sparingly by the profession in general, and has been so for many years past.

But I am not so sure about mercury. Undoubtedly the use of this treacherous mineral is now marked by very great caution; and we almost never hear of those bad consequences which are the direct result of excessive mercurial action. But is its use, as some even now use it, expedient or necessary? And in rheumatic pericarditis in particular, would patients recover *better,* or worse, were no mercury exhibited? Without altogether holding *the question* as decided, I am strongly inclined to answer both of these ques-

tions in a sense unfavourable to the use of mercury. My own use of this much vaunted and much abused remedy has been rather experimental than founded on conviction.

The reserve with which I have used a remedy which has so much testimony in its favour, may appear to require some explanation. The truth is, that, as a student, it was my fortune to serve in the hospital under a very bold mercuralist—a man of the most humane character, and of the most entire conscientiousness, who proved his sincerity in regard to this subject by undergoing, in his own person, three distinct salivations in the course of his fatal illness. Under this gentleman's directions, I learned much that was valuable; and, among the rest, something in regard to the bad effects of mercury in rheumatic pericarditis. But I have never succeeded in learning anything as to its good effects, though on many occasions afterwards I have administered it with such caution as my knowledge of it inspired. It may be said that I have not done justice to the remedy. In one sense, this is true; for I have very rarely given it, except after other remedies. But surely, with a remedy of the power ascribed to this one, and especially regarded as promoting the absorption of exudation, it is no real injustice to call it into operation only in cases of a certain degree of severity, and to watch with care its influence upon cases that have in some degree resisted other treatment.

But whether I have done justice to mercury or not in my personal trials of it, I believe I have used it so as to do justice to nature, which is, after all, perhaps a better thing. For, assuredly, if I had adopted the plan of giving mercury instantly, in every case where a slight roughness existed in connection with the first sound of the heart, I should have failed to observe that the immense majority of these cases never went beyond a slight roughness, or, at most, a slight but decided friction-sound; that of those which went beyond this, and were accompanied by a degree of effusion, a large proportion had only moderate effusion; and that, even when considerable effusion was present, a good cure was still possible without mercury.

I will conclude by mentioning, as nearly as possible, what has been the usual course of treatment adopted in the cases here referred to, in so far as it has differed from that of acute rheumatism, or of the other primary disease. I trust I have not acted under any narrow or bigoted feeling of opposition to established doctrines, any more than of empirical devotion to single remedies.

In the very beginning of some cases of pericarditis, where the pain was very marked, and especially where it had strongly the characters of angina, leeches have not unfrequently been applied in moderate numbers. From four to six leeches so applied, and followed by fomentations, have very commonly relieved the pain, and been followed rapidly by improvement. Where relief occurred, but was not complete, the application has sometimes been repeated. More commonly, one application has been all that I have thought requisite; and this only when strength and condition permitted, and when the symptoms had a certain degree of urgency. General blood-letting has not once been practised.

Fomentations, sometimes plain and sometimes medicated with opium, friction with camphorated and iodurettted liniments, and in obstinate cases the use of blisters, have been the chief local remedies besides leeches.

To conclude, I believe the principles of the safe treatment of pericarditis to be as follows:—1. To make large allowance for the insignificant and spontaneously healing class of cases revealed more by physical signs than by symptoms, and to regard them as demanding little active treatment; 2d. To consider rheumatic pericarditis in general as a disease susceptible, to a great extent, of cure under mild palliative local remedies and fitting constitutional treatment; 3d. To hold the constitutional treatment as subordinate to that of the disease with which the pericarditis is associated.—*Edin. Med. Journal, Jan.* 1861, *p.* 626.

## 16.—ON THE INFLUENCE OF OZONIZED COD-LIVER OIL ON THE PULSE.

By Dr. E. Symes Thompson.

[The attention of the Profession was first drawn to this subject by Dr. Theophilus Thompson, in a paper read before the Medico-Chirurgical Society some time since].

He recorded the cases of about twenty patients at King's College Hospital, to whom the ozonized oil was administered. The usual dose was two drachms twice-a-day. Scarcely any effect was observed from doses of one drachm. The influence of the oil on the pulse increased in proportion to the dose in which it was given, the effect of half-ounce doses, two or three times a day, being more marked than that of smaller quantities. The following table exhibits, as simply and concisely as possible, the results:—

*Table showing the Changes of Pulse under Ozonized Cod-Liver Oil.*

| Name. | Sex. | Disease. | Time of Taking Oil. | Pulse. | |
|---|---|---|---|---|---|
| | | | | Before. | After. |
| | | | Days. | | |
| J. P. | M. | Phthisis, first stage ..... | 7 | 116 | 98 |
| J. S. | M. | Ditto ..... | 7 | 120 | 108 |
| W. B. | M. | Tuberculous larynx . .... | 21 | 112 | 92 |
| M. H. | F. | Phthisis, second stage .... | 7 | 80 | 80 |
| E. A. W. | F. | Ditto ..... | 14 | 140 | 112 |
| C. H. | F. | Phthisis, first stage ..... | 14 | 100 | 104 |
| R. N. | M. | Phthisis, third stage..... | 7 | 150 | 116 |
| E. D. | M. | Phthisis, second stage .... | 7 | 138 | 112 |
| J. O'D. | M. | Emphysema ..... | 4 | 104 | 86 |
| M. M. | F. | Phthisis, second stage.... | 5 | 140 | 108 |
| E. R. | F. | Anæmia..... | 4 | 120 | 116 |
| M. S. | F. | Phthisis, third stage ..... | 2 | 94 | 92 |
| E. H. | F. | Phthisis, second stage.... | 10 | 104 | 88 |
| W. S. | M. | Phthisis, first stage ..... | 7 | 104 | 104 |
| P. R. | M. | Phthisis, third stage ..... | 4 | 104 | 96 |
| S. E. | F. | Phthisis, second stage.... | 20 | 120 | 92 |
| G. M. | M. | Phthisis, third stage .... | 4 | 140 | 120 |
| H. C. | F. | Phthisis, first stage ..... | 2 | 108 | 95 |
| W. H. | M. | Phthisis, first stage ..... | 5 | 120 | 96 |
| J. R. | M. | Phthisis, first stage ..... | 9 | 120 | 98 |

The author also recorded the effects of ozone in another form. He had made use of the ozonized water (as prepared by Condy), and found its influence in retarding the pulse considerable. In reference to a belief still prevalent amongst some members of the profession that ozone was a high oxide of hydrogen, the author remarked that he had used the peroxide of hydrogen (prepared by Messrs. Bullock), and found that it exerted no special influence on the pulse, which was more often accelerated than retarded under its use, which seemed to corroborate the conclusions of Dr. Andrews, who showed (in the "Philosophical Transactions of the Royal Society," vol. cxlvi. p. 1, *et seq.*) that ozone was not an oxide of hydrogen, but simply an allotropic modification of oxygen. Of the 20 cases in which oxonized oil was given, in eleven the pulse was reduced more than 20 beats a minute; in 4, upwards of 10 beats; in 1, no effect was produced; and in 1 only was any permanent acceleration observed, and this could only be fully accounted for by disturbing circumstances. Of the 7 cases in which ozonized water was given, in 3 the pulse was lowered more than 20 beats, in 3 about 10 beats, and in 1 it was at one time retarded and at another accelerated. Dr. Thompson drew special attention to the importance of the inquiry as connected with

the treatment of phthisis, since, in this disease, anything that could retard the excessive rapidity of the vital changes would likewise check the development and progress of the disease. He showed that this had long been realised by the profession, and mentioned several remedies that had been used for this end, especially digitalis, which, though useful in some cases, was greatly inferior to ozone, being both less certain in its action, and often dangerous from its cumulative tendency; while ozone exerted on the heart, not a depressing, but a strengthening and invigorating influence. It was suggested that the explanation of the remarkable effect of ozone in phthisis might be found in the greater affinity which phthisical blood had with oxygen—an affinity which it also preserved when in the allotropic form of ozone. Before concluding, the author alluded to a paper recently published by a French physician, "On the Use of Oxonized Oil of Turpentine in Hæmoptysis," in which the suggestions made by Dr. Theophilus Thompson, in 1859, had been followed, and observed that he had himself prescribed turpentine with ozonized cod-liver oil in hæmoptysis with much benefit.—*Med. Times and Gazette, March* 9, 1861, *p.* 266.

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## 17.—ON FIBRINOUS DEPOSITS IN THE HEART.

By Nathaniel Crisp, Esq., Swallowfield.

[The following is extracted from the 'Annual Retrospective Address,' delivered before the Reading Pathological Society. After detailing one or two other cases, in which fibrinous clots have been found in the heart, and had been the cause of death, the author proceeds:]

*Uterine Phlebitis: Formation of Fibrinous Clots.* — Mr. Walford read notes of the case and exhibited the uterus and uterine appendages of a woman, who had died of phlebitis. She was the mother of a large family, had been confined a month, and about this time was looking pale and ill. She complained of pain, tenderness and swelling, below Poupart's ligament; at the same time general constitutional disturbance existed. She continued in this state for six days, when increased præcordial dulness, and bellows-sound, existed over the cardiac region. No rheumatic taint or previous heart disease had existed. Rapid emaciation ensued; cough came on a day or two afterwards, and florid blood was expectorated. Nine days before death, the right arm became swollen and very painful; and on the day prior to her decease, no pulse was to be detected at the wrist, and the jugular veins were swollen like a cord. Death took place on the eighteenth day of her illness, seven weeks after her confinement. The lochia continued in a slight degree the whole time. On removing the uterus and appendages, a granular kind of spot, of about the size of a shilling, was seen on the os. The lining membrane of the uterus was vascular. Some of the uterine veins were blocked up with fibrinous clots, and this state could be traced into the body of the uterus; a coagulum existed in the right uterine vein, close to its entrance into the vena cava. Serous effusion was found in the abdominal and right pleural cavities, as well as within the pericardium. The liver was large and nutmeggy. The lungs were generally gorged with blood. The right side of the heart was filled with clotted blood, and a fibrinous clot extended from the auricle into the ventricle; and in the right ventricle was a layer of fibrine of a rose colour. The tricuspid valve was a little thickened; and one of the larger divisions of the pulmonary artery was completely blocked, as well as the jugular and subclavian veins on the right side. Mr. Walford, in his remarks on this case, drew attention to the length of time which had elapsed between labour and death, and was inclined to think that the granular appearance on the os uteri, (already referred to), was an ulcer, whence all the mischief sprang, and concluded by making some remarks on the utility of keeping the vagina well syringed in such cases. The general opinion of the meeting appeared to be against the idea that a breach of surface existed, that, if any ulceration had existed, it was not of recent date, and was healed.

*Remarks on Fibrinous Deposits.*—The subject of fibrinous depositions in the heart, of *ante mortem* origin especially, has of late been brought more directly under our notice by Dr. Richardson's most able and interesting publications; and as a body, we are much indebted to him for so clearly explaining cases of death, which previously, to a certain extent, appeared obscure. Although as yet, such discoveries may have done but little to lessen mortality, still, to use his own words, they "may make prognosis more accurate, may guard us against false practice, and in time, may lead to suggestions bearing directly on curative treatment;" and, doubtless, to some extent, his words have already proved true. Is it not much more satisfactory to ourselves to be able to make up our minds as to the true nature of our patients' symptoms, than to be merely certain that a fatal issue is about to ensue; when such an event has taken place, and a *post mortem* examination reveals the true cause, to be satisfied that no remedies we have used could have accelerated it, and perhaps to feel now and then justified in cherishing the hope that our efforts had at least the tendency to prolong existence, if not to lead to a still more happy result? We may not have been guilty of fancying that we had discovered a serpent in the blood, as some of our forefathers did; but I suspect that many of us have allowed the fact of the existence of cardiac clots to pass by unnoticed, and have ascribed death to more remote causes. At the same time we must not be too ready to attribute all cases to such a cause; even if the symptoms are much allied to those described as indicative of such formations. In proof of this, I may mention a case occurring in the practice of a friend of mine. A gentleman, during a short illness of only twenty-four hours, exhibited symptoms which might have led to such a supposition. On examining the body after death, a considerable quantity of blood was found in the pericardium; no rupture of any portion of the heart or large vessels could be detected; the blood appeared to have simply oozed through the muscular structure of the ventricle. This case is, I think, as rare as interesting, and deserving of mention in connection with the present subject.—*Brit. Med. Journal, Nov.* 24, 1860, *p.* 918.

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## DISEASES OF THE ORGANS OF RESPIRATION.

### 18.—ON THE USE OF LOOPED WIRE IN THE REMOVAL OF FOREIGN BODIES FROM THE AIR PASSAGES.

By Dr. J. J. Tomson, Davenport, Iowa.

[The patient was a lad about eight years of age, who had accidentally got into his trachea a piece of clay pipe-stem, about one and a half inches long, and of a large size. Tracheotomy was performed, but the piece of pipe could not be seized by any expedient tried.]

After trying the forceps, hooks, &c., I suggested the use of a *looped wire*. A piece of small wire, about two feet long, was obtained, and looped in the middle, of sufficient size to embrace the end of the pipe-stem (on the same principle as removing corks from a bottle with a string). The patient's head being well thrown back, I proceeded to introduce the *looped wire*. On passing it down to the right bronchus, it came in contact with the foreign body. At this point, I raised the end which I held in my hand, and pressed the end next the foreign body towards the spine, so as to pass my wire behind the pipe-stem. The pipe-stem was firmly impacted in the bronchus, so that it required some force to push the wire between it and the wall of the bronchial tube. The wire was passed some two inches or more below the point of obstruction, and then, on gently withdrawing it, the loop came in contact with the lower end of the pipe-stem, which was thus easily removed. The orifice of the trachea was closed, and the boy made a rapid recovery.

*On the* twenty-fifth of last month, my partner, Dr. Maxwell, and myself, *were sent for by* Dr. Carpenter of Blue-Grass, to assist him in removing a

grain of corn from the trachea of a child about one year old. The operation was performed by Dr. Maxwell; after which I passed the loop of wire as in the other case. It was passed down the right bronchus, and passed quite easily the point of obstruction; and on its removal, it brought the kernel of corn into the trachea, which soon after made its appearance at the orifice, and was easily removed.

I wish to call the attention of the Profession to this *simple*, *cheap*, and *harmless* instrument, from the fact that I believe it will succeed in some cases where nothing else will. It can be used with perfect freedom by any one who is acquainted with the anatomy of the lungs, in searching far into the air-passages for small bodies, with little or no risk of producing serious irritation. There are other cogent reasons for its trial which will suggest themselves to the mind of every medical man. I submit its trial, with the cases above reported, to the Profession, hoping that it may be found of *some* service in such painful and unfortunate cases.—*American Med. Journal*, *Jan.* 12, 1861, *p.* 25.

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19.—*On the Palliative Treatment of Asthma.*—By T. L. PRIDHAM, Esq., Bideford, North Devon.—The first on the list is stramonium, the fumes of which may be collected in an inverted glass bowl with a narrow mouth; the bowl being charged to its full is placed under the mouth of the patient, who is directed to inhale to the fullest extent in his power the smoke which has been collected in the bowl, taking care to hold his head away from the bowl when an expiration takes place. Chloroform, both taken internally or inhaled, is a powerful remedy, but it must be employed with caution, and never administered except by a medical attendant. The fumes of nitre paper in a state of ignition, well inhaled, is often a valuable remedy. Care should be taken to procure the best prepared from a good chemist. Chloric ether and the tincture of the lobelia inflata will occasionally relieve. Bicarbonate of soda, as well as chlorate of potass, given in full doses, I have frequently seen produce a good effect. Again, I have seen repeated doses of sulphate of alum procure relief, the powder being allowed to dissolve on the tongue before it is swallowed, in ten grain doses. I have also seen the fumes of tobacco, inhaled as I have recommended in the use of stramonium, relieve, when other remedies have failed; but I do not like this remedy, it produces such deadly faintness and nausea. Small drinks of the best Mocha coffee, made strong, will often procure relief. On two occasions, when every other remedy failed, I succeeded in procuring almost instant relief, by injecting two grains of morphine and a drachm of tincture of assafœtida. These were cases where mental distress appeared to be the exciting cause.

I have often sat at the bedside of one, suffering from the severest form of the disease, watching with great anxiety the result of prescribed remedies, and it has not unfrequently happened that many have been tried without relief, the patient all this time gasping for life with sufferings the most intense, when relief has at length come from a remedy apparently the most unlikely to procure it—so capricious is the disease, and so uncertain the remedy in asthma cases of this particular character.—*Brit. Med. Journal*, *Dec.* 29, 1860, *p.* 1009.

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## 20.—DRILL FOR AUSCULTATION.

By Dr. THOMAS K. CHAMBERS, Physician to St. Mary's Hospital.

[Students should shelve long books on auscultation and study nature, by using the following 'order of drill.' The student will, in three or four days, acquire all that is practically useful in the clinical examination of the lungs.]

FIRST STEP of Drill.—Look over the whole outside of the chest, and see if there be anything wrong about the SKIN or deformity of SHAPE. Note particularly *flatness* or *bulging* of ribs. In males this is quickest done by stripping the upper part of the body all at once. A female's shift is looser,

and can be drawn *first backwards* so as to look down the back and sides, *then forwards* to look at the front and sides.

SECOND STEP.—Get behind the patient, put your two thumbs on the top of the two scapulæ, and lay your fingers firm and flat on the collar-bones and upper ribs. Make the patient sigh deeply, and then looking along your fingers you can compare the motion of the two UPPER LOBES of the lungs with one another. If a bright light shines on your finger-nails you can detect a difference in expansion of one-twentieth of an inch. Then put your thumb-tips on the lowest dorsal vertebræ and span the waist as far as you can, make the patient sigh, and you feel the extent of motion of the LOWER LOBES.

THIRD STEP.—Percussion—what do you want to find out by it? Only if one part of the pulmonary tissue is more solid than another. Of course it cannot be *all* solid; so if any part is so, it will contain less air, and sound therefore less drummy, less "resonant" than a more healthy part with which you compare it. Do not think about finding anything else, or you will not pay due attention to this.

Place the patient so that you can lay the fingers of your left hand flat on the thorax. Swing the right hand free and easy, and hammer with the finger-*tips*, on your knuckles in the following order:—

*Auscultation Posts.*

| | |
|---|---|
| 1. On right clavicle. | 2. On left clavicle. |
| 3. Under right clavicle. | 4. Under left clavicle. |
| 5. Above right scapula. | 6. Above left scapula. |
| 7. Close under right scapula. | 8. Close under left scapula. |
| 9. Under right mamma. (an inch below). | 10. Under left mamma. |
| 11. Right lateral region. | 12. Left lateral region. |

Do not go on hammering long, but compare by two or three quick strokes each place with the same place on the other side. In all these parts you ought to find the resonance of the two sides equal except in the left mammary region, where the heart ought to make dull the spot it is felt to beat in.

If you can detect no absence of normal resonance, take a note of it, and go on to next step. But if there be dulness where it ought not to be, percuss round and round the spot where you first find it, and take a note of the EXTENT OF DULNESS.

FOURTH STEP.—Apply your stethoscope *flat, to the naked skin.* Apply it to the same posts in the same order that you have percussed, and make notes in the same order of what you hear. Do not trust to what you may consider deviations from an ideal standard, but compare the two sides and note if they differ.

Now, as to what you may expect to hear:—

*Natural sounds.*—Some healthy lungs breathe very softly, some very harshly; so the degree of sound, if equal throughout, tells nothing. Note therefore only when they are *defective* in any places, while breathing fully elsewhere.

*Morbid sounds.*—Think first what your ear *can* tell you, and do not trouble your brains by thinking about what it *cannot.* It can tell you—

(*Point* 1) Whether air enters the subjacent lung or not;

(*Point* 2) Whether it passes through natural, soft, yielding tubes, or through stiffened tubes;

(*Point* 3) Whether it bubbles through fluid or not;

(*Point* 4) Whether the bubbles are large or small, and therefore whether the open space they break in are large or small.

*As to Point* 1—if no air enters, there is an end of your observation at that spot; take a note and go on.

*As to Point* 2—if the tubes and tissues are natural, the sound of expiration is less than the sound of inspiration. If it is equal to it or greater than it at any spot compared with the opposite side, call it "tubular breathing"—that *is to say, the air goes through tubes only, and does not enter into the terminal vesicles.* The typical example of it is what you hear on each side of

the sternum over the large bronchi, and therefore it is often called "bronchial breathing." Tubular breathing tells you that the tubes are stiffened either by their coats being swelled or hardened, or by the surrounding tissue being condensed.

*As to Point* 3—air passing through a thickish fluid, like mucus or pus is sure to make a crackling noise. If it does not do so, if there are continuous whistling, snoring, or piping sounds, they may be safely called "*dry*" sounds, and noted down as "whistling," "snoring," or "piping," without waiting to find a French name for them.

*As to Point* 4—the size of the crackles is important. The very finest are evidence of their being situated in the terminal vesicles of the lung. Imitate this fine crepitation by rubbing your hair between your fingers, and then make as large a bubble as you can with saliva between your lips, and you hear the types of the two ends of the scale of size. The larger they are, the larger the bronchus or cavity in which the bubbles break.

I said that air passing through a viscid fluid is sure to make a crackling noise, so that the *absence* of crackling proves the absence of fluid. But the *presence* of crackling does not necessarily prove the presence of fluid. Unfortunately animal membranes will make a creaking just as leather does. And the creaking of inflamed pleura exactly resembles the crackling of small bubbles. *The sounds themselves* are undistinguishable. Do not be taken in by persons who profess to distinguish them; they do it by collateral circumstances, as you must do. When you hear crackling, put it down in your note-book, and leave the distinction to the future.

FIFTH STEP.—Go round again to the same posts with your stethoscope, at each post making the patient speak a few words of several syllables—such as counting on from twenty. What you have to notice is—(1), whether the voice is heard through the stethoscope at all; (2), if so, whether it is (3) *natural*, that is, a confused sort of buzzing; or (4), *with increased vibration*. In cases where the vibration is increased notice if it is like a man talking Punch—i. e., *ægophonic*; or if, when the patient whispers, you can hear the whispering up the stethoscope—i. e., *whispering pectoriloquy*.

All besides this is fancy work, showy, but waste of time. Even on this real work do not pause too long; the moment you hear a sound, go on to the next post, or you confuse your ear and learn to dawdle.

It is a great saving of time in practice to be able to take notes with rapidity, as well as to auscultate with rapidity. For this purpose I recommend you to acquire the habit of drawing a rough but correct outline of the chest and shoulders, and to have certain marks to record what you have observed. To do it occupies not a quarter of the time of a written description. I can make the two outlines of the front and back of the chest, on the slate in fifty seconds, and in thirty seconds more I can mark upon each of the posts of auscultation what I hear by these or any other arbitrary signs.

In this sketch the most commonly observed phenomena are recorded by conventional signs, the less common by the first few letters of their names, the still less common by the name written at full length.

A curved line along the spine, ), may denote that the spine is curved to one side or another; and another that there is bulging of the ribs.

Oblique lines, ////, that there is flatness of the chest observed in the first step of the drill.

Parallel lines, ====, that there is dulness on percussion.

*T.* or *t.* that there is tubular breathing; the loudness and depth of tone, or smallness and sharpness of tone, being indicated by the size of the letter, showing the size of area it is made in.

*C*, or *c*, or *cc*, that there is crackling large or small, according to the size of the letter, and extensive according to the number of letters.

*B* may denote bronchial or vibratory voice.

*P*, pectoriloquy.

*Def. m.* may be written on a line to denote *deficient motion* of the ribs as far as that line extends.

*Æg.*, may denote *ægophony; res.*, *unnatural resonance*, with a line drawn

round it to show how far it extends; while "*whispering voice*," "*metallic tinkling*," "*cracked pottery*," or any fancy sound which you think it worth while to record, had better be written at full length, with a dotted line leading from the post where it is heard.

*H*, of course, means *healthy sounds*, which you should take a note of, though they do not necessarily prove that the lung is healthy. Remember, now and always, that your note records what you hear, not your diagnosis. Thus, on the diagram, you may see I have noted in the left infra-clavicular region the ribs are flattened, though the clavicle has not fallen in; that there is a lateral curvature of the spine, with the greatest projection towards the left side; that the ribs on the left side bulge considerably; that there is deficient motion of both upper and lower lobes of the lungs on the left side; that there is dulness on percussion in the left infra-clavicular, left supra-scapular, left infra-scapular, and left lateral regions; that the cardiac region is resonant; that there are crackling or crepitant râles in the right supra-clavicular and left supra-clavicular regions; that in the latter these are the larger and coarser; that there are the same râles in greater amount and with small tubular breathing in the right infra-clavicular; that in the left infra-clavicular there is also very loud tubular breathing, large and small crepitant râles, very loud, bronchial voice, whispering pectoriloquy, and metallic tinkling; that in the left infra-scapular region there is ægophony; that in the left lateral there is bronchial voice and mixed crepitant râles of various sizes: that the sounds were those of a healthy condition of lung in the right infra-scapular, infra-mammary, and lateral regions.

I need not ask you which mode of record you think will take longest or be most exact.

If you are not quick in drawing with a pen, a further saving of time may be effected by having the outline cut in wood for a few shillings, and stamping in your case-book when and where required. Or you may have it stamped on thin paper, gummed at the back like a postage stamp, and stick it in when wanted.

Under any circumstances, whether you adopt my method or one of your own, I think it of paramount importance that you should take notes, not of your diagnosis, but of the *grounds on which you base it.* It is of no use to yourself, your patient, or to science to remember that on such a day you thought that there was pneumonia or tubercle, but it is of great use to all to remember why you thought so.—*Lancet, March* 30, *and April* 6, 1861, *pp.* 307. 334.

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## DISEASES OF THE ORGANS OF DIGESTION.

### 21.—ON THE COMPARATIVE DIGESTIBILITY OF VARIOUS ARTICLES OF DIET.

By Dr. George E. Day, Professor of Medicine in the University of St. Andrews.

[The following, like several other short articles in this volume, is extracted from Dr. Day's recent admirable work on 'Chemistry in its Relations to Physiology and Medicine.']

A chapter on "The Digestion," would obviously be imperfect if it included no notice of the digestibility of the different ordinary articles of food. Unfortunately, our knowledge on this subject is very imperfect, the experiments which have been made being in part unsatisfactory in the mode in which they were devised, and in part, giving absolutely contradictory results. The experiments of Gosse, who possessed the power of vomiting at will, and who thus brought up food which had remained for different lengths of time in the stomach, and even the much more valuable observations of Beaumont on Alexis St. Martin, are unsatisfactory in the following respects. (1.) These observers regarded the digestive process as ended when the food in the stomach had been reduced to a uniform pulp, and altogether neglected the *changes impressed* upon the food beyond the stomach. (2.) They used

mixed, variously prepared, often half vegetable and half animal food, a method which is totally unfit for determining the digestibility of individual articles of diet. (3.) No mention is made, at least in Beaumont's experiments, of the quantity of food that was taken, or of how minutely it was divided. Without entering into details of the discrepant observations made on this subject by Schultz (who at a certain time, after feeding dogs and cats with different kinds of food, killed them and examined the contents of their stomachs), and by several other physiologists, it is sufficient to quote the opinion of Blondlot, who first introduced into physiology the operation of artificial gastric fistula, and who obtained such very indefinite and inconclusive results, that he was led to express the view that the digestibility of different articles of diet depended solely on the state of the stomach at the time of the experiment, and that it is pure waste of time to labour at the determination of the digestibility of individual articles of food. Busch found in his case of intestinal fistula that flesh, eggs, and vegetables began to appear at the fistulous opening in from fifteen to thirty minutes after they were swallowed, but that if a copious meal had been taken, traces of these substances continued to appear for three or four hours. He gives the following results:—

| | | | |
|---|---|---|---|
| Boiled eggs | appeared in | 3 experiments | after 26, 20, and 35 minutes. |
| Cabbage | " in | 2 " | after 19 and 15 minutes. |
| Flesh | " in | 2 " | after 30 and 22 minutes. |
| Parsneps | " after 12 minutes. | | |
| Potatoes | " after 15 minutes. | | |

The following table, extracted from Busch's memoir, is deserving of attentive study, as showing how much of each of the articles of food mentioned in the first column was absorbed before reaching the fistula in the jejunum.

| | Ratio of weight of food to weight of matters subsequently discharged through the fistula. | Ratio of solid constituents of food to solid constituents of the matters discharged through the fistula. | Ratio of solid constituents of undissolved portion of discharged matters to the solid constituents of the dissolved matters. |
|---|---|---|---|
| Gelatin | 1 : 3·675 | 1 : 0·94 | |
| Boiled eggs | 1 : 2·73 | 1 : 0·76 | 1 : 2·3 |
| Flesh | 1 : 1·73 | 1 : 0·35 | 1 : 2·27 |
| Milk | 1 : 1·25 | 1 : 0·62 | 1 : 4·3 |
| Parsneps | 1 : 1·2 | 1 : 0·49 | 1 : 0·94 |
| Cabbage | 1 : 0·91 | 1 : 0·58 | 1 : 0·66 |
| Potato soup | 1 : 0·7 | 1 : 0·53 | 1 : 1·5 |

From the experiments on which these numerical results are based, Busch concludes that flesh is more digestible than eggs, that parsneps are more digestible than potatoes or cabbage, and potatoes more digestible than cabbage.—*Glasgow Med. Journal, Jan.* 1861, *p.* 468.

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## 22.—ON THE INTESTINAL CONTENTS.

By Dr. George E. Day, Professor of Medicine in the University of St. Andrews.

The bright yellow semifluid excrements of infants at the breast contain, as was shown by Simon, a large quantity of fat, a considerable amount of coagulated bile, undigested casein, and a sufficient quantity both of the biliary acids and bile-pigment to give certain evidence of the presence of these bodies by Pettenkofer's test and by nitric acid respectively. Epithelial structures are moreover present.

The excrements sometimes present a green colour, which, until comparatively recently, was regarded as a sign of the presence of an excess of bile. This is, however, seldom the cause of a green colouration of the faeces, and only occurs when there is at the same time an excess of bile and a preponderance of free acid in the intestine; as, for instance, in *icterus neonatorum*. In these cases the ordinary brown bile-pigment seems to be converted into the modification termed bilifulvin. As we shall presently show, the presence of blood may sometimes give rise to a green colour.

Lehmann has confirmed the observations of Höfle and others, regarding the occurrence of sulphide (sulphuret) of mercury in the green or greenish-black stools that are voided after the use of calomel. The colour in this case seems due partly to the presence of almost unchanged bile, and partly to that of the mercurial compound.

The excrements often assume a black or dark-green colour after the prolonged use of ferruginous preparations or chalybeate waters (especially such as contain sulphate of soda with carbonate of protoxide of iron). Lehmann has definitely shown from analyses of the green and black excrements of persons taking the Marienbad waters, that the colour is here due to the presence of protosulphide of iron.

The excrements are usually green after the medicinal use of indigo, and are often black after charcoal has been taken.

As only a definite quantity of fat can be absorbed by the intestines in a given time, food very rich in fat or the ingestion of cod-liver or other oils may give rise to the presence of a large quantity of fat in the excrements. In some diseases, especially such as specially interfere with the general nutritive processes, such as pulmonary phthisis, Bright's disease, diabetes mellitus, and more particularly diseased conditions of the pancreas, an augmentation of the fat in the fæces is often observed.

Sugar has been occasionally found, but it is not always present, in the excrements of diabetic patients.

The occurrence of blood in the fæces is by no means rare. Omitting all notice of those cases in which its presence is too obvious to be overlooked, we may remark that when the hemorrhage is very slight, and proceeds from the stomach or small intestine, it may impress upon the fæces a peculiar colour and appearance, whose cause may easily escape recognition. In such cases, we often have black or chocolate-coloured tar-like stools, in which imperfect blood corpuscles can be discovered by the microscope, and in which hæmatin can be chemically detected. In some of the intestinal diseases of young children, and occasionally in some forms of continued fever, semi-fluid green excrements are discharged which owe their colour to a slight admixture of blood which may be readily detected by the microscope.

Albumen in a coagulable state sometimes occurs in normal excrements; it is, however, found in large quantity in the evacuations in dysentery and in typhus (and not unfrequently in the fluid or semi-fluid stools which sometimes occur in Bright's disease), and in lesser quantity in cholera.

Epithelial structures occur in the stools in all cases of diarrhœa. They are extremely abundant in the evacuations in cholera.

Cytoïd corpuscles are abundant in the evacuations in intestinal catarrh and in dysentery; and are occasionally observed in typhus and cholera. The glassy mucus which sometimes occurs in roundish masses in catarrhal affections of the large intestines is apparently secreted by the follicles of the colon.

The intestinal evacuations have been especially studied in typhus and cholera.

In typhus the stools are usually fluid, of a yellowish-brown colour, an abominable smell, and an alkaline reaction. On standing for some time they separate into a yellowish sediment, consisting of flakes of undigested food, white granules of about the size of a pin's head, and probably resulting from intestinal ulcers, epithelium, and often mucus, and numerous crystals of phosphate of ammonia and magnesia; and an opaque supernatant fluid of a yellowish or pale brown tint, generally containing albumen and a large quantity of chloride of sodium.

In cholera the main peculiarities of the stools are, in addition to the abundance of epithelium already referred to, an extraordinary quantity of water, a little albumen, very little biliary matter, and a large amount of salts, among which the chloride of sodium preponderates to such an extent as often to exceed in amount all the organic matters. These evacuations contain only from 1·2 to 2·4% of solid constituents. The addition of nitric acid gives rise to a rose-red tint in these stools, which is often also observed in the evacuations in typhus.—*Glasgow Med. Journal, Jan.*, 1861, *p.* 465.

## 23.—ON THE ACTION OF TOBACCO, TEA, AND COFFEE.

By Dr. GEORGE E. DAY, Professor of Physiology in the University of St. Andrews.

With regard to tobacco, Dr. Hammond made two sets of experiments—(1.) when a sufficiency of food was taken to keep up the weight and vigour of the body, and (2.) when an insufficiency of food was taken. It should be mentioned that he is not in the habit of using tobacco in any form, and that during these experiments he smoked 450 grains daily. He finds:—

1. That tobacco does not materially affect the excretion of carbonic acid through the lungs, but that it lessens the amount of aqueous vapour.
2. That it diminishes the amount of fæces.
3. That it lessens the quantity of the urine and the amount of its urea and chlorine, but that it increases the amount of free acid, and of the uric, phosphoric, and sulphuric acids.

These results differ in several essential points from those yielded by alcohol. The fact that the amount of carbonic acid was not diminished, would indicate that the consumption of the fat of the body is not lessened by the use of tobacco. The general metamorphosis of the tissues would seem to be retarded, seeing that both the urea and the chlorine of the urine are diminished (in the first set of experiments the mean daily diminution of urea and of chlorine was represented by 42·4 and 23·0 grains; while in the second set the corresponding numbers were 62·5 and 15·0 grains); but as the phosphoric and sulphuric acids are increased (in the first set the mean increase being 24·2 and 4·4 grains, and in the second set 30·2 and 8·3 grains), we can only explain the apparent inconsistency in these results by assuming that there is an increased oxidation of the phosphorus and sulphur of the brain and nervous tissue generally, although the metamorphosis of the other nitrogenous tissues is lessened.

Tobacco resembles alcohol in these respects, that when the food is sufficient to preserve the weight of the body it increases that weight, and when the food is not sufficient, and the body in consequence loses weight, it restrains that loss; but it differs from alcohol in being unattended with any unpleasant effects upon the circulatory system, though its action on the brain is apparent in increased nervous excitement, followed by a pleasant feeling of ease and contentment.

Tea and coffee are usually believed to have a somewhat similar effect to that which, as we have shown, is produced by alcohol and tobacco.

Bocker made a careful series of observations upon himself, for the purpose of testing the effects of tea on the organism.

Three series of experiments were made under varying conditions, but they all coincided in the following particulars:—

1. Tea in ordinary doses has not any effect on the amount of carbonic acid expired, the frequency of the respirations, or of the pulse.
2. When the diet is insufficient, tea very considerably limits the loss of weight thereby entailed.
3. When the diet is sufficient, the body is more likely to gain weight when tea is taken than when not.
4. Tea very much diminishes the loss of substance in the shape of urea.
5. Tea very much lessens the quantity of fæces.
6. The loss by perspiration is also limited by tea.

Dr. Edward Smith has recently arrived at certain conclusions which are the very reverse of those of Bocker. He maintains that tea increases the amount of carbonic acid (and here he is likewise opposed to other observers), and that it actually accelerates the waste of the tissues. It is to be trusted that this discrepancy of opinion between two good observers may lead to this important question being further examined and definitely settled.

The action of coffee on the organism has been carefully studied by Dr. Julius Lehmann. He has investigated the separate actions of the different constituents of the coffee bean, viz., caffeine (identical with theine) and empyreumatic oil, as well as of their mixture. The point that mainly con-

cerns us in reference to the present subject, is (1.) that coffee protracts the decomposition of the tissues, and (2.) that the protraction is chiefly caused by the empyreumatic oil, and that the caffeine only causes it when it is taken in larger quantities than usual.—*Glasgow Med. Journal, Jan.*, 1861, *p.* 470.

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24.—*On Oxalate of Cerium in Gastric Affections.*—Dr. CHARLES LEE has tested the efficacy of oxalate of cerium, recommended by Professor Simpson of Edinburgh, as a remedy in gastric affections.

Dr. Lee first used the medicine in cases of obstinate vomiting in advanced pregnancy, which had resisted all the ordinary remedies, such as creosote, hydrocyanic acid, ice, bismuth, &c. In each of four cases of the kind, which he relates, he gave one or two grains of the oxalate of cerium every two or three hours with marked success; the vomiting being completely arrested. He has also found the effects of the remedy no less encouraging in the vomiting that often accompanies phthisis, in pyrosis, in hysterical emesis, and in various dyspeptic conditions, especially in atonic dyspepsia. In fourteen cases of the latter disorder, the effect was perceptible in a short time, both in relieving nausea, and in restoring the appetite. Dr. Lee confirms Dr. Simpson's statements as to the rapidity of action of the oxalate of cerium; but is disposed to question its action as a *sedative tonic*, as he has tried it without success in several cases of acute and subacute gastritis, both idiopathic and supervening on debauch, or delirium tremens.—*Amer. Journal of Med. Science.—Brit. Med. Journal, Jan.* 5, 1861, *p.* 24.

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## 25.—IS MERCURY AN HEPATIC SPECIFIC?

By Dr. THUDICHUM.

[This question has been broached by various authors lately. Dr. Thudichum and Dr. Inman have both written before on the subject. The following propositions express Dr. Thudichum's ideas on the subject; they agree essentially with Dr. Inman's:]

1st. That mercury does not make its appearance in the bile when given in the form of calomel has been proved by Mosler ('Virchow's Archiv.' vol. xiii., p. 29.) Doses of from twenty-two to fifty-two grains produced no appearance of mercury in the bile, neither was the quantity of bile secreted thereby augmented. If I recollect right, an author in India, writing in the 'Lancet,' attempted to controvert the facts of Mosler, and stated that he had found mercury in the bile. Even if that should be so in some cases, there remains the fact, nevertheless, that the quantity of bile is not increased by that agent.

2nd. H. Nasse, Kölliker, and H. Müller, found that the addition of calomel to food, which under ordinary circumstances produced a certain and normal quantity of bile in dogs, diminished the quantity of bile.

3rd. Taking into consideration three experiments of Kölliker and four of Scott's, related in Beale's 'Archives,' Dr. Inman comes to the conclusion that the chances are six to one that calomel will diminish the hepatic secretion. The facts I have stated make the proportions worse. In nine cases out of ten, calomel, whether it purges or not, will diminish the quantity of bile secreted. Where it acts as a purgative, the diminution is most conspicuous.

4th. In my reply to the observations of Drs. Leared and Routh, I allowed that mercurials, particularly calomel, by their purging action, might relieve some disorders of the liver or other organs, or of the general system. This effect could be purchased at less cost by other purgatives, because the specific action of mercury was always hurtful, excepting, of course, in cases of syphilis. The mere fact, therefore, of cases of liver disease having been *improved* by the action of mercurials, is no proof of their specific advantage.

*5th.* The stools which are passed after the use of purging mercurials, par-

ticularly calomel, are supposed to contain more bile than usual. This assumption rests upon the observation that these stools are mostly green—a fact which appears to me to be at the bottom of the entire tissue of errors.

The green colour of calomel stools is due to sub-sulphide of mercury, just as the black colour of stools following the use of preparations of iron is due to sub-sulphide of iron. The sub-sulphide of mercury can be easily obtained from those stools by levigation, or chemical proceedings. So much is proved. On the contrary, it is not proved that calomel stools contain any increased quantity of either bile or biliary colouring matter (cholochrome.) From the experiments quoted above, the reverse of this unproved hypothesis will probably be found to be true—namely, that the green calomel stools contain a smaller amount of biliary colouring matter than an equal amount of solid matter from healthy fæces.

*Note.*—When the fæces passed after a dose of calomel or blue pill become green, they also begin to scald the anus. This sensation has been ascribed to the bile, which was then believed to be passing. It is only reasonable to ascribe this scalding, not to bile, which is not present, but to the sub-sulphide of mercury, which has been proved to be present.

6th. Dr. Inman adopts the estimate of the quantity of bile discharged by a man of 160 lbs. weight, in twenty-four hours, as sixty-six ounces. The most accurate calculations from experiments upon animals permit me to assume that an adult person secretes between 195 and 675 grains of solid matter through the biliary channels. The amount of solid matter contained in human bile varies between eight and sixteen per cent. It therefore follows that the amount of average bile secreted by an adult in twenty-four hours fluctuates between 1200 and 9000 grains; or 2 oz. 240 grs., and 18 oz. 360 grs.—a vast difference from 66 oz. But substituting these figures for those given by Dr. Inman, and assuming the fæces to weigh half a pound instead of a pound (the dry residue of a healthy man's fæces does not usually exceed two ounces), Dr. Inman's subsequent conclusions become still more correct.

7th. Dr. Inman assumes that the clayey, white stools of persons suffering from jaundice, might contain the ingredients of bile minus only the colouring matter. This has not been proved to obtain. As, on the other hand, it has been proved that the healthy fæces contain no biliary matter except a derivate of cholochrome, Dr. Inman's assumption becomes very improbable, and the reverse opinion gains ground, that the clayey, white stools of the jaundiced contain neither bile, acids, nor cholochrome. I am far from admitting, as a reliable fact, that the liver may secrete colourless bile. I also cannot admit that in jaundice the colourless part of the bile could go into the intestines, while the coloured part passes into the blood. With regard to these points, I differ entirely from Dr. Inman, not because I think that such might not occur, but simply because no such occurrence has been proved.

8th. I take this opportunity to point out that the question of the discharge of any modified biliary matter besides cholochrome in the fæces requires further study. The ordinary biliary salts are certainly not present; the fæces contain little soda. But some modified cholic acid might still be contained, and escape observation, as it had hitherto done in gall-stones. According to Berzelius, five ounces of fresh excrements contained twenty-one grains of a matter similar to bile. Considering that 675 grains is the maximum, and 195 grains the minimum of solid bile, any modified cholic acid in the fæces could not be less than three, nor more than ten per cent. of the bile secreted in twenty-four hours.—*Lancet, Oct.* 27, 1860, *p.* 411.

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## 26.—THE ACTION OF MERCURY ON THE LIVER.

By Dr. Thomas Inman, Liverpool.

[The question which Dr. Inman endeavours to answer in the following paper, is, whether the brown colour of the stools really is owing to biliary matter

The subject first suggested itself from several papers by Dr. Ward and Sir H. Cooper, in which it is taken for granted that such is the case.]

With an honest desire to attain exactness, I attempted to supply the required links, and made the inquiry, Upon what does the brown tinge in the stools depend? Dr. Ward gives his own case to show that diarrhœa with clayey stools may exist for weeks without there being any evidence of real hepatic disorder: in that observation I concur. I have already noted the persistence of white stools when an individual is on a milk diet. I know one case of severe headache always accompanied with clayey stools, in which mercurials *invariably make the stools whiter;* and another, in which the motions become brown again in four days without any medicine at all: clayey stools, therefore, do not necessarily indicate hepatic disease requiring medical interference. But Dr. Ward says, that mercury, as a general rule, restores the healthy colour sooner than any other drug—granted, for the sake of argument; but how does that affect the question if we rejoin, *the brown colour of the stools is due to a secretion from the colon?*

In cogitating over this matter, I was greatly struck with the acuteness with which Dr. Thudichum had struck what I felt was *the* weak point in my first paper. His remarks, as reported in the Journal, Oct. 27th, 1860, *a propos* of my notion, that the clayey stools might contain colourless bile, were, "This has not been proved to obtain; as, on the other hand, *it has been proved that the healthy fæces contain no biliary matter, except a derivate of cholochrome*, Dr. Inman's assumption becomes very improbable; and the reverse opinion gains ground, that the *clayey white stools* of the jaundiced contain *neither bile acids nor cholochrome.*" Taking this in its entirety, we assume that there is no direct proof that clayey stools contain *any bile.* But memory recalls many cases of jaundice with total suppression of the bile, when the stools *have been brown.* Whence, then, the colour? This involves a very simple question, viz.: *Where does the brown colour of the stools begin?* That I might not deceive myself on this point, I propounded the question separately, without giving any reason for it, to four different young medical men, all close observers, and diligent in their attendance in the dead-house; the answer was the same from all, viz., *in the colon.* My own memory upon this point may possibly be treacherous, I therefore make the remarks interrogatively rather then affirmatively, and ask, "Did ever any one see the contents of the small intestines of a brown, deep yellow, or even a bilious hue? Did ever any one see, in cases of hernia, a brown fæcal matter flow from the bowel, if the small intestine only was implicated? Is it not a fact, that the intestinal secretion is always whitish prior to its reaching the ileo-cæcal valve, and that it attains its fæcal character and colour *in the colon? May not a clayey diarrhœa, then, simply demonstrate that the colon, and not the liver is sluggish?* I next examined with care the intestines of a man who had died in the Royal Infirmary rather suddenly. The canal was slit up from end to end, and it was clear that "fæcification" commenced in the colon: the direct evidence was against the bile being the cause of the colon. After this, I recollected Dr. Petrie's assertion, that the green colour of the stools in children who had taken calomel was due to a vitiated secretion from the bowels, and chiefly from the colon; and on turning to a private letter from my friend, Dr. Russell, of Birmingham, date, Nov. 3rd, 1860, I found the remark. "My strong suspicion—I dare hardly yet call it a belief—is, that what is called bile in the fæces is really a secretion from the intestinal follicles, and often from those of *the colon.* I was led to this suspicion by finding 'fresh bile' in fæcal matter which had evidently been lying for a considerable period in the colon, and I propounded the heresy at one of our societies, but received general discredit."

These observations demanded the further inquiry, Is there evidence that calomel and mercurial compounds act upon the intestines generally, and upon the colon in particular? The answer in the affirmative has been given by Dr. Taylor, who writes, irrespective of the present inquiry, "The coats of the stomach are sometimes corroded; . . . similar appearances have been met with in the intestines, especially in *the cæcum.* In a case by Dr.

Herapath the stomach was softened, and the cæcum had been the seat of the most violent inflammation, the whole surface being of a deep black red colour, and there were patches of sloughing in the coats" (Taylor *On Poisons*, p. 395). Again (p. 397), "the duodenum and jejunum were healthy; there was slight inflammation of the mucous membrane about the lower two-thirds of the ileum, and this was the more marked towards the termination of the intestine; near to the cæcum there were several patches of inflammation; the whole of the large intestines were highly inflamed; the liver was enlarged and congested; the gall-bladder contracted and containing scarcely any traces of bile." Dr. Scott showed that calomel purges irrespective of bile in the bowels. Coupling these remarks with those of my first communication, the deduction is logical, that there is evidence that mercury *does not increase the hepatic secretion*, but that *it does modify the condition of the intestinal mucous membrane, and especially that of the colon.—Brit. Med. Journal, Dec.* 15, 1860, *p.* 985.

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## 27.—CASE OF DISPLACEMENT OF THE LIVER, AND OF OBLITERATION OF THE VENA PORTA.

By A. M. M'Whinnie, Esq.

Whilst attending a young lady with broken rib, occasioned by a fall from her horse, her mother observed that my patient's health, she thought, had suffered from the habit of tight lacing. The menstrual periods were irregular, her aspect was cachectic, and she suffered from hemorrhoids.

On examining into the physical effects produced by the continued constriction of the chest, it was found, by percussion, &c., that the liver extended downwards far beyond its proper region; its edge could be felt below the umbilicus, where it seemed pressed down and retained by the constriction of the lower part of the chest.

This case reminded me of some notes and a sketch of the liver of a female, aged twenty-two, who had died of pleuro-pneumonia. Here also the lower ribs were much pressed inwards. On opening the abdomen, the liver appeared to occupy the greater part of the cavity, increased to several times its natural size; and it extended from the hypochondriac region, where the right lobe seemed to press up the diaphragm and encroach upon the chest, downwards to the pelvis; its thickened inferior border reaching to the pubes, dragging down and altering the situation and relations of most of the viscera, the great end of the stomach—as in another instance related to me—being lodged in the left iliac fossa.

The form of the liver from above downwards was altered to a greater extent than I have ever seen in the male subject, and was undoubtedly due to continued external pressure. The ill effects on the functions of many of the organs, &c., must be obvious, and it may be a question how far many of the symptoms in connection with hysteria so eloquently described by Sydenham may not be referable to the above cause.

The return of venous blood we may imagine might be here much impeded, particularly as regards the vena porta. In connection with this subject, I may cite the following instance of

*Obliteration of the Vena Porta.*—A woman, aged forty-nine, was admitted into St. Bartholomew's Hospital for dropsy. She stated that she had been in perfect health six weeks previously. The catamenia were at this time suppressed, and the case was treated slightly, with the expectation that on the return of the natural discharges she would recover. She was, however, seized with pain and stercoraceous vomiting, and died. On examination, the vena porta was found obliterated to within two inches of its entrance into the liver. The splenic and superior mesenteric veins were pervious. The gastric veins were turgid, in consequence, no doubt, of their entering the obstructed part of the vena porta. The liver was healthy, and the gall-bladder gorged with bile. Hepatic, cystic, and ductus communis choledochus were pervious. Hardly a vestige of the pancreas, nothing but a very few

scirrhus-like lumps, was found, suggesting the idea that the obstruction might have been originally owing to a diseased state of the pancreas. The hepatic artery was of natural size.—*Lancet, Jan.* 6, 1861, *p.* 5.

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## DISEASES OF THE URINARY ORGANS.

### 28.—ON THE TWO LEADING TYPICAL FORMS OF BRIGHT'S KIDNEY.

By Dr. S. J. Goodfellow, Physician to, and Lecturer on Medicine at Middlesex Hospital.

[All the various forms of diseased kidney described by writers under the denomination of Bright's disease, may be referred to two leading typical forms: viz., the large white kidney, and the small contracted kidney. These two are essentially different, and never pass one into the other. The fact, however, must be admitted, that the two processes leading to the two different forms of disease, may go on together, and so produce a kidney having a more or less close resemblance to both these forms, either in the same or different parts.]

1. *The Large White Kidney.*—It is, as its name implies, larger than the normal kidney, varying in size from six to twelve ounces or even more; its external cortical, or secreting part is increased at the expense of the internal, medullary, or purely excreting parts, measuring generally between the base of the pyramids and the investing capsule, from half an inch to an inch or more; it is of a whitish or yellowish-white colour, flaccid and anæmic, partly from the obliteration of the vessels, partly from the more or less abundant serosity, and partly from the diminution in the amount of blood-corpuscles. There may, however, be, and there often are, a few enlarged and turgid veins, which in the interior are tortuous, and on the surface have an arborescent form; the medullary portion may present various degrees of engorgement, or may be as exsanguine as the cortical portion, but this, so far as my experience goes, is rare. The course of this disease is generally rapid; it is always accompanied by considerable general dropsy; and the urine is generally scanty, smoky, and contains large quantities of albumen, some blood-casts of tubes, epithelial and exudative casts, and some red blood corpuscles, and, at the later stages of the disease, more or less fatty matter. The duration is seldom longer than from four, five, or six to eight or twelve months; it may, however, persist even longer in some rare cases.

2. *The Small, Contracted Kidney.*—It is invariably smaller than the normal kidney, sometimes only half the natural size; it is hard, contracted, red, and granular; the external or cortical part is wasted, and therefore much diminished, without any corresponding increase of the tubular portion.

This form is chronic in its nature, its duration occupying many years, and is very seldom found without evidence of a similar process in other organs, especially in the liver, and not unfrequently in the spleen and heart; and it is more than probable that these have been affected contemporaneously with the kidney, from the influence of some cause affecting the body generally.

The symptoms of this form are somewhat obscure; certainly not so evident as those characteristic of the other and more acute form. They are more of a secondary character, and more particularly referable to remote organs, rather than to the kidney itself from the effects of the disease, and the retention of the urinary constituents, upon the blood, and upon the nutrition of the body. The general tissues of the body suffer degradation, and the subjects of the disease are cut off from some other disease, as apoplexy, or other head affections, pneumonia, pleurisy, pericarditis, peritonitis, &c., to which they have became predisposed, owing to the general dyscrasia produced by the insidious and unsuspected progress of the kidney affection; they die from pyæma, erysipelas, or other causes, after accidents or surgical operations which are not in themselves mortal, and not usually followed

by a fatal result. The urine in this form is, as a rule, as abundant as in health, sometimes even more abundant; it is of low specific gravity; it may or may not contain albumen, generally the only effect of applying heat and adding nitric acid is to render the urine slightly opaline, but to produce no sediment or actual precipitate; it scarcely ever contains any casts of tubes, either epithelial or exudative. There is very seldom any anasarca.. If there be any, it is small in amount, and then generally only at the close of life. But in many cases, on looking attentively, you may, as I have stated in a preceding lecture, detect a slight œdema beneath the conjunctivæ, and in the feet at night, and a general puffy condition of the eyelids, and of the loose subcutaneous tissues. The more positive or evident symptoms and signs of this form are pallor, neuralgia, headaches resembling those characteristic of hemicrania, noises in the ears, motes before the eyes, and other symptoms referable to the nervous system, dyspeptic and other symptoms indicative of gastric and intestinal irritation, and even inflammation: for even extensive ulceration of the stomach is occasionally found with this condition of kidney, but whether it be an effect of it, experience has not enabled me to decide, although it is not improbable.

Now, as I have said before, and indicated in preceding lectures, every kind of modification of these two forms may be met with, but never any true gradation from the one into the others—that is from the large white, or most acute form, into the hard, contracted, and more chronic disease. There may be, from some peculiarities in the nature of the cause, the habits of life and constitution of the individual, a state partaking of both characters, or the one process may attack a kidney previously the seat of the other process, and so in a manner become engrafted upon it, in which case the organ will of course present the characters of both. It may or may not be larger than normal; generally it is. It is much less pale than the large white; its vessels are much more numerous and more or less gorged with blood; the Malpighian tufts are red and solid, and the organ presents a coarse granular appearance. The symptoms, as you might be prepared to expect, are much less acute than the large white form, and more decided than in the purely chronic form. The urine is very seldom free from albumen, it may contain a very considerable quantity; the specific gravity is considerably under the average, but never so constantly low as in the small contracted kidney; there is almost always more or less deposit of albumino-fibrinous casts of tubes both large and small, and also casts of imperfect broken-down granular epithelium, in which there generally are some minute fatty molecules, and a few isolated blood-corpuscles. There is commonly considerable anasarca, with great proneness to effusions in the serous cavities, and even inflammatory formations—as flakes of lymph, &c.

Besides these two leading typical forms, and the third or mixed form, there are two others mentioned by writers, namely the Waxy, Lardaceous, or Amyloid kidney, and the Fatty kidney. These may be modifications of the first form (the large white), or they may be produced by an independent morbid process. They much more rarely accompany the hard, contracted type of kidney.

You will see that the terms lardaceous, waxy, and amyloid are synonymous, and are used to indicate the same disease. The first was used originally by Rokitansky, from the supposed resemblance of the kidney when so affected to bacon rind. The second was a name given to it by some English pathologists from a fancied resemblance to wax, and the third is the term given to it by Virchow, because of its offering reactions on the application of iodine and sulphuric acid, resembling, if not identical with, those of the same agents upon starch.

This form of kidney must be rare, for I have not met with it in my practice at this Hospital. But now that we have the means of detecting this degeneration even when partial, and before it has become so general as to affect the whole cortical substance, so as to be apparent to the naked eye, we shall doubtless find it more frequently; for I have for some time had the impression that this change or metamorphosis must be frequent in scrofulous

and phthisical patients, and have often been surprised at not discovering palpable evidence of it in the dead-house. Virchow says, "that a large proportion of the cases of Bright's disease especially the chronic ones, are assignable to this change" (into the lardaceous, waxy, or amyloid condition), "and must, therefore, be separated from many other similar forms as constituting a special, altogether a peculiar affection." From the interesting account of this condition given by Dr. Harris, it appears that the kidneys are generally enlarged, and that the cut surface of the cortex is of a pale yellowish-white colour, here and there irregularly depressed, the depressions giving the surface an uneven lobular appearance. The capsules strip off readily, leaving the surface smooth and not torn. The symptoms of this form of kidney are obscure. There is generally anasarca more or less considerable, a great proportion of albumen in the urine, and also some small pale waxy casts, and a few epithelial cells, and red blood-corpuscles. The specific gravity of the urine is generally under 1012. It being a constitutional disease, and the urine containing but a very small proportion of urea, the general symptoms are severe, especially the nervous symptoms.

The next form is the fatty kidney. In most, indeed in all, of the other forms of these affections, the kidney may undergo the fatty metamorphosis, but especially is it prone to take place in the large white kidney, and in the mixed white and granular kidney. Virchow has found that the kidney, whose epithelium has passed into a fatty degeneration, nearly always shrivels up, and the result is a permanent atrophy. But when the pure typical white kidney undergoes this metamorphosis there is but little diminution in size, and the exudative matter deposited in the tubules and interbular substance seems to undergo a still further degeneration into fatty and oily matter. This condition will generally be found in persons who have been addicted to intemperance—especially in the use of undiluted spirits—as brandy, gin, and whisky.

The symptoms indicative of the fatty kidney will more or less be modified according to the conditions of the organ with which this metamorphosis is associated, and also with the greater or smaller amount of this degeneration in other organs of the body, especially in the heart and arteries. There is generally very considerable anasarca, although great fatty degeneration of the organ has been found where no anasarca was observed during life. The same may be said, also, with regard to albumen. When the white kidney is the seat of the metamorphosis you will invariably find considerable anasarca, and more or less of albumen, and of fat or oily casts in the urine.—*Med. Times and Gazette, Dec.* 1, 1860, *p.* 523.

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## 29.—ON THE EFFECT OF ALCOHOL IN THE INDUCTION OF KIDNEY DISEASE.

By Dr. S. J. Goodfellow, Physician to the Middlesex Hospital.

[Dr. Goodfellow considers it an unquestionable fact, that alcholic compounds are a very frequent cause of kidney disease and albuminous urine. Moreover these compounds act similarly, whether taken by the stomach or inspired along with the air; hence, disease of the kidney is frequent amongst painters from constantly inhaling the vapour of turpentine, and in tapsters from constantly inhaling the vapour of alcohol, whilst serving at the counter. Dr. Goodfellow proceeds to relate the case of a tapster, in whom nothing had any effect in relieving his albuminous urine until he left off his occupation, and this occurred several times, the albumen returning every time he renewed it. What is the physiological and pathological action of alcoholic compounds?]

*Their Physiological Action.*—According to current notions, which are founded for the most part upon the theory of Liebig, supported by the experiments and researches of Bouchardat and Sandras, and since also by Duchek, the alcohol passes in the system, principally in the blood, through several

oxydising stages, until it ultimately becomes metamorphosed into carbonic acid and water, which are exhaled principally by the lungs. This theory, so plausible, and so satisfactory in many respects, and so calculated to explain many acknowledged effects of alcohol, and supported as it is by the results of direct experiments and chemical analyses made by the first chemists of the day, was generally received as true. According to this theory, alcohol was regarded as an aliment—one of the tertiary, non-azotised aliments,—and therefore subserving the processes of respiration and calorification. The successive changes that alcohol was supposed to undergo in the capillary vessels of the system from the oxygen brought by the blood, were into aldehyde, acetic acid, and carbonic acid and water. The carbonic acid resulting from the decomposition of the acetic acid was supposed partly and chiefly to get out of the system in a gaseous state by the lungs, partly to become united to several bases, and eliminated by the kidneys and other emunctories. Duchek goes so far as to say that it sometimes becomes converted into oxalic acid. It has been generally observed that the quantity of carbonic acid in the air expired soon after the ingestion of alcohol or any spirituous liquors is very perceptibly diminished, and considerably less than before. To explain this diminution, it has been stated that alcohol, by the action of an oxydising body, loses two equivalents of hydrogen, and gives rise to aldehyde:—

$$C_4 H_6 O_2 + O_2 = 2 H O + \underbrace{C_4 H_4 O_2}_{\text{Aldehyde.}}$$

At a second degree of oxydation, the alcohol loses two equivalents of hydrogen, which are replaced by two equivalents of oxygen, and acetic acid is produced:—

$$C_4 H_6 O_2 + O_4 = 2 H O + \underbrace{C_4 H_3 O_3, H O}_{\text{Acetic Acid.}}$$

This transformation of alcohol into acetic acid takes place out of the body, as you know, under the influence of ferments, or dry platinum-black. By a farther oxydation the acetic acid becomes converted into carbonic acid and water, which are ultimately carried out of the system in the way I have already pointed out.

Nothing apparently could be more satisfactory than this explanation, and you perceive how completely it accounts for the diminution of the quantity of carbonic acid. The alcohol takes all the oxygen, and, therefore, the fatty matters cannot be burnt off; they remain in the system to give rise to the drunkard's fatness, whether deposited as fat, or taking the place of the proper organic principles, and leading to fatty degeneration. The whole of the oxygen is used up in gradually converting the alcohol into carbonic acid, which as it is slowly evolved, unites with the different alkaline and earthy bases, to be finally eliminated by the kidneys and liver. This was supposed very likely to happen when, from the deadening, numbing, paralysing influence of the alcohol upon the nervous and muscular systems, the respiratory movements became so reduced in frequency and extent, that but little oxygen could be introduced into the blood, from the small quantity of air gaining admission into the lungs.

Now all this theory, plausible and satisfactory as it is, has been completely, and, I think, successfully proved to be false. It will not bear a searching inquiry into the true facts of the case. And after all, as the sequel will prove, we are obliged to come back to the old opinions, as derived from unbiassed experiments, and before facts were made to square with chemical theories. MM. Lallemand, Perrin, and Duroy have made this inquiry in the true spirit of philosophy, and in the most searching manner. It is impossible to read the account of their experiments and analyses without being convinced that they had one object alone in view, and that was truth, apart from any preconceived views or theories. It is altogether out of the scope of these lectures to quote at *length* the beautiful, ingenious and very satis-

factory experiments by which they have been led irresistibly to their conclusions. Suffice it to say that the results of their experiments admitted of no other conclusions than those which the authors came to, and which are of great value in explaining the pathogenic action of alcoholic and allied substances. These gentlemen then have found upon evidence, which I do not see how any one can gainsay, that whether alcohol, or its compounds brandy, rum, gin, or whisky, be taken into the stomach, or inhaled by the lungs, it is only found as alcohol in the blood and in the tissues, especially in the nervous substance, for which it would seem to have a special attraction; and that it has no claim to be regarded as an aliment. When taken into the stomach some small portion may become converted into acetic acid, by the gastric juice and the mucus acting as ferments. But even this small quantity does not enter the blood. In this list fermented drinks which contain more or less nutrient matter mixed with the alcohol must be excluded, such as wine, beer, cider, perry, &c. Wines contain even nitrogenous matters, also colouring and fatty matters, and salts; cider contains glucose, mucilage, vegetable acids, &c.; beer also contains glucose, dextrine, and allied substances in considerable proportions, nitrogenous matters, bitter and aromatic principles, and salts. According, then, to MM. Lallemand. Perrin and Duroy, alcohol is neither transformed nor destroyed in the organism, and is ultimately eliminated without undergoing any modification. They have detected it in considerable quantity in the blood, brain-substance, when freed from membranes and blood, and in the urine by means of distillation; they have shown afterwards, by the aid of exact doses analogous to the process of analysis by the method of volumes, that alcohol diffuses itself in the tissues, and that it accumulates in the brain, and in the liver, where it is found in larger quantities than in the blood and other organs. They have proved by multiplied experiments, verified by counter-proofs, that alcohol does not undergo any modification in the economy, and that it does not give rise consequently, to any bodies resulting from its oxydation, such as aldehyde, acetic acid, &c. It is only in the stomach that it is susceptible of experiencing any modification, for a small fraction of alcohol ingested is there converted into acetic acid by the action of the gastric juice and the mucus, which act then as a ferment; but this action, altogether local and special to the stomach, ceases the moment the alcohol penetrates the venous radicles. These authors have shown, moreover, that it is eliminated by the lungs, the skin, and the kidneys, as alcohol. It is not only after the ingestion of a *great* quantity of alcohol that they met with it in the organs, for they found it in the blood of a dog, nine hours after he had taken only 30 grammes (3 drachms, 37½ grains) at 21°; they met with it in a man who had drunk about 30 grammes (about 3½ drachms) of brandy; they observed finally, that the pulmonary exhalation of a man who had taken a litre (7-8ths of an Imperial quart) of wine, of a middling alcoholic richness, contained alcohol for eight hours after taking it, and that the urinary secretion gave evidence of its presence during fourteen hours. The authors may well ask,—"Is this the mode of action of an aliment?" All the tests for the detection of aldehyde, and acetic acid, were had recourse to after alcohol had been administered in various ways, and in every dose, but without avail; whereas when very small quantities of these substances were administered, evident indications of their presence in the blood, and in the organs, and in the exhalations from the lungs, were at once observed. I have already described how the diminished quantity of carbonic acid exhaled after the ingestion of alcohol was explained under the old theory. It remains to show how the diminution can be accounted for under these observed, indisputable facts. Now, it appears, from the researches of MM. Lallemand, Perrin, and Duroy, that alcoholic substances exert a very remarkable action upon the blood, which presents in animals alcoholised numerous globules of fat, like cholesterine, visible to the naked eye, and swimming on the surface of that fluid. This is of such interest in connexion with our subject, that I shall again refer to it when I come to speak of the *pathological* effects of alcohol. At present it is in its physiological aspect

that they are of importance. Since alcohol produces a modification so singular, may it not offer also, when present, an obstacle to the disengagement of carbonic acid, or delay even the combination of oxygen with the carbon of the blood? If this be the case, alcohol contributes to nutrition, not actively as an alimentary substance, but in an indirect manner in exercising a moderating influence upon organic decomposition. With respect to its influence independently of this separation of the fatty principles, and perhaps their conversion into a nonsaponifiable state, but little is positively known. It is conceivable, however, from the properties which alcohol is known to possess,—its great diffusiveness through, and attraction for, water,—its power of dissolving some very important animal principles, and of coagulating others,—that it does exert a considerable influence upon the physical, and also probably upon the chemical qualities of the blood, and blood-corpuscles. Nothing definite, however, has been observed. Dr. Addison, of Brighton, whose able researches have thrown light upon some physiological and pathological processes, has observed some very curious effects upon adding sherry wine to blood out of the body; and it is possible that alcohol, when taken into the system in large quantities, may in time work such changes, and even destroy the red corpuscles already formed, and hinder the full development of others. MM. Lallemand, Perrin, and Duroy, however, saw no alteration in the corpuscles, even when alcohol was added to the blood out of the body, and also in blood taken after large quantities had been imbibed.

Poiseuille's experiments proved that its mixture with the animal fluids, both when directly injected into the blood-vessels, and after being taken into the stomach, retards the circulation through the capillaries, although its first effect is to excite the heart to increased action. It diminishes the want of food, and impairs or destroys the appetite for it. Bouchardat remarks that with drinkers of brandy and other alcoholic liquors, the alcohol acts by diminishing and suppressing probably the functions of absorption by the stomach in respect of every other substance; it augments, on the contrary, the secretion of that organ; and from these conditions arise the increased secretion of mucus, the disgust for food, and the emaciation. Of course such liquids as beer, some wines, and cider, and other nutritious and true alimentary and fattening drinks, are not included.

That it affects the nervous system, and indirectly, if not directly, the muscular system also, I need scarcely mention; it is too often rendered obvious to us. A moderate quantity produces an excitation of the nervous system, which extends over the whole economy; a still larger dose produces great disturbance of the cerebral functions, which another and still larger dose completely annihilates. The same effects nearly are observed upon the muscular system. A moderate dose seems to impart strength to the muscular contractions, while a very large dose destroys all voluntary contractility, and a poisonous one that also of the involuntary muscles. Flourens' experiments, upon the effects of alcohol upon birds, are very instructive. Its effect upon them resembled that produced by the removal of the cerebellum, except that the intelligence remained. With alcohol, I need not say it was destroyed. In poisoning by alcohol, the respiratory movements and those of the heart were the last affected,—those of the heart the last. Even for some considerable time after respiration had ceased, the heart continued to beat. In the experiments which I made upon the frog, which some of you witnessed, the heart continued to beat, the circulation went on, for some time after respiration had ceased.

*Their Pathological Effects.*—That alcohol is a local irritant is unquestionable, and that it produces its effects upon the system partly in this way is very probable. It may act remotely by sympathy to some small extent, as Orfila believed. But we have seen from the very able researches of MM. Lallemand, Perrin, and Duroy, from whose book I have already quoted so largely, that it is rapidly absorbed by the venous radicles, and that its principal action is directly upon the different organs which it irritates, and eventually inflames. Especially has it been proved to be present in greater

proportion in the nervous tissue than elsewhere, which it more particularly excites. It disturbs its functions; it perverts and ultimately destroys the intellectual faculties, and even the emotional faculties; it disturbs the function of the sensory nerves, both common and special, as shown by subjective tactile phenomena, strange perversions of taste, double vision, and other disorders of the optic nerves, tinnitus aurium, and other disorders of the auditory nerves. It equally disorders and destroys the function of the motor nerves, as shown in irregularity, and absence of consentaneous action of the movements. From these effects upon the cerebro-spinal system, it is more than probable that it disturbs and impairs the functions of the organic nervous system, as evidenced by defective nutrition and secretion. When taken in the form of brandy, whisky, gin, and such fluids, it impairs nutrition, probably from its great attraction for water, inspissating the blood and juices of the body. I need not mention in what large proportion water enters into the composition of the tissues and fluids of the body. It is probably in this way that it acts as a diuretic so far as the increase of the watery part of the urine is concerned, not only from the increased quantity of water ingested with and after the brandy, but from its abstracting it from the tissues. There is no doubt that it tends to harden the brain substance, and produce atrophy of many of the structures, not only by increasing the quantity of connective tissue and other white fibrous tissues, and so leading to undue pressure upon the more important parts, but by condensing the tissues directly by the abstraction of water. There is no doubt of its exerting this destroying influence upon the liver. I shall endeavour to show you that it does so upon the kidney also. As a general rule, it irritates and inflames the tissues of the stomach and duodenum, and even the pancreatic and hepatic ducts, and it probably affects and deteriorates the secretion of these glands. It produces hypertrophy of the connective tissue forming Glisson's capsule, which in its turn, presses upon the small vessels, and upon the hepatic cells, and produces atrophy of these anatomical elements in two ways—first, by cutting off the supply of nutrient materials, and secondly, by absorption from pressure. The digestive processes are probably still more impaired by the bad quality of the bile and pancreatic secretion.

Now, very much the same changes take place in the kidney as in the liver and other organs. We have seen that alcohol passes through the vessels and tissues of this organ as alcohol; it irritates these tissues, as it does similar tissues in other parts; it leads to blood delay; it impairs the influence and function of the nervous system; it produces hypertrophy of the connective tissue, forming the stroma or framework of the organ, and of the capsule; and it produces a granular appearance precisely as it does in the liver. In fact, this alteration is very commonly seen in both these organs in old drunkards, especially and almost exclusively those who take the raw spirit in large quantities, or spirit mixed with only small quantities of water. Those who drink largely of beer, and perhaps of wine, are found to have a somewhat different form of kidney, especially when taken as gin, brandy, &c. But we have seen that alcohol separates and modifies the fatty matters of the blood. MM. Lallemand, Perrin, and Duroy, have seen this. Most pathologists believed that so far as the relation between cause and effect could be traced, it was almost certain that alcoholic beverages, when largely and continuously consumed for any length of time, led to fatty degradation. This separation has now been actually seen and proved. Now, this separation and alteration of the fatty principles of the blood probably plays a very important part in the pathological effect of alcohol, when taken in large quantities, in the form of brandy, gin, whisky, &c. Now, saponifiable fatty matters, that are visible to the naked eye, are calculated to impede the circulation through the capillaries—if not to cut off the blood-supply altogether—and so produce atrophy of the secreting tissues, while the connective tissue, supporting the vessels, would receive an undue supply of blood *plasma*, and therefore become hypertrophied. It is not improbable that *some of* these fatty matters become transuded with the exudates, and thus

lead to the presence of fat in the tubules, and also in the intertubular substance; some may also remain in the walls of the capillary vessels, and replace in time the normal elements.—*Med. Times and Gazette, Nov.* 17, 1860, *p.* 471.

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## 30.—ON THE TREATMENT OF BRIGHT'S DISEASE.

By Dr. S. J. Goodfellow, Physician to the Middlesex Hospital.

[One very important, but difficult and delicate part of our treatment, consists in regulating or removing the morbid action of distant organs produced by the supplementary action imposed on them by the imperfect secreting power of the kidneys. Sometimes the perspirations are profuse: this is favourable; for the skin is the only safe emunctory for the elimination of the urinary constituents. When the vicarious action is in the gastric intestinal mucous membrane, or in any serous membrane, we must try to divert it to the skin. These effusions are perhaps more properly called secretions, as the urinary constituents are really eliminated by them.]

These effusions in Bright's disease afford great temporary relief to the more important symptoms. They are in fact a physical necessity, in consequence of the great distension of the vessels, and the almost stagnant condition of the blood in them. Absorption is all but arrested under these circumstances. To apply, therefore, a roller to the lower extremities, will have for its effect only to remove the fluid to parts, the function of which is much more important, and even immediately essential to life. Great caution, therefore, I repeat, should be observed in the application of this bandage in all cases of dropsy, but especially in dropsy depending upon these diseases of the kidney. As a general rule, it is never justifiable, so long as the anasarca seems to be on the increase. Under such circumstances, it is far better, and less dangerous, that vesication should take place, or even acupuncture be resorted to, and the serum allowed gradually to ooze away. It is singular, but nevertheless true, though not easy of explanation, that spontaneous vesications, indeed actual sloughing sores, on the lower extremities, are much less liable to spread than acupunctures, or similar small punctures made with the lancet. It is not an uncommon occurrence to see patients brought into the hospital with enormous unhealthy-looking vesications, and even sloughing sores of various sizes, from which there is a constant and copious discharge of serosity, and they generally do well; while punctures, however minute, made with the lancet, or with the grooved needle, are very often followed by rapidly-spreading erysipelas, notwithstanding that every precaution is taken to keep up the temperature of the limb, and to absorb the serosity as it flows out, by enveloping the limbs in cotton-wool and flannel.

[The author now proceeds to the treatment of each form separately.]

*First, with reference to the Acute Forms when a part of Scarlet Fever.*—As prevention is, by common consent, better than a cure, a few words on the treatment of scarlet fever, more especially with reference to the prevention of kidney-disease, will not be without their value. Besides, such a mode of treating the subject will be in strict accordance with the general plan upon which I have proceeded all along, and that is, to consider these affections of the kidney like other morbid affections of the system. There is nothing, in fact, specific in these diseases. They are referable, as I have endeavoured to show, to the same causation, and it is only an accidental circumstance which determines the operation of the particular cause to the kidney; or, as with many of the causes indeed, the kidney-disease is only a part of a general disease, affecting more or less, and with greater or less intensity, according to circumstances, other organs of the body. The treatment must be guided, moreover, by the same general principles. In a mild form of scarlatina but little treatment is necessary. To secure a moderate but efficient action of the bowels, and of the skin and kidneys, and protect the patient against exposure to cold, is all that is necessary. The action of the

bowels is best secured by a dose of castor-oil, or the saline draught, that of the skin and kidneys by the haustus ammoniæ acetatis. As long as the disease has every appearance of remaining mild, I do not, as a rule, use any other treatment. If, however, there is the slightest indication of its assuming a more severe or a malignant form, I immediately prescribe iron and quinine, or iron alone, and always in the form of the tincture of the sesquichloride. In all cases of scarlatina (and the same remark is applicable to all diseases which are the result of the action of some animal poison), you should not discontinue your observation of the case until you have the strongest grounds for believing that all the poison has been eliminated from the system, and that the action to which its development and increase were due, has entirely ceased. This is not always—indeed, scarcely ever—the case in that state which we describe by the term Convalescence. In all cases, however mild, this is not accomplished for some weeks after this.

During convalescence, with the view of getting rid of the poison altogether, and during the active stages of the disease to favour its elimination from the body, and prevent its further development and increase in it, it is of great consequence that the atmosphere should be changed often. During convalescence, if the weather be mild, the patient should go into the open air, at the same time being warmly clad. If in winter, the air of the house or chamber should be entirely changed once or twice a day. If he be still in bed, and the fever is of a severe or malignant form, this change becomes essential for the patient's safety. Now, the atmosphere of a room cannot be freed from impurities while the walls remain standing. Don't be startled. New walls, for my purpose, can be built with lime. All animal poisons certainly, all atmospheric impurities probably, have a great attraction—an *adhesive* attraction for surfaces. The rougher they are the greater the attraction. The following fact, among many others of a similar although less striking character, will show this:—When I was a resident at the London Fever Hospital, a patient was brought to the hospital suffering from erysipelas of the face and scalp, which had come on during an attack of fever. In this ward (which was in the form of an oblong parallelogram) were thirteen beds, six on one side, and seven on the other, the space that should have been occupied by the seventh bed being taken up by door-ways leading to, and partitions separating, the latrines, and sculleries, and bath-rooms. The patient was placed in the bed next but one to the end of the ward opposite to the scullery, and on the side on which there were seven beds. The patient in the end bed, on the same side, was attacked, then the patient in the next bed (the third from the end), and then in succession in every bed, until it had attacked the one in the last bed at the bottom of the ward. It then attacked the patient in the last bed on the opposite side, and so in succession up to the fifth bed, and as far as the offices above mentioned. The only patient who escaped was in the bed *beyond* the offices. From this circumstance, and others like it, I determined, on my own responsibility, to have the walls lime-whited every month or so, according to the number of patients constantly in the ward. The records of the hospital will show that previously, erysipelas, pyæmia, and a peculiar form of laryngitis, attacked great numbers, and were very fatal; but afterwards no case occurred, up to the time of my leaving the hospital, which was some two or three years, notwithstanding that during that time several cases of erysipelas were admitted.

There is one other point connected with the treatment, which I feel bound to refer to, because I think that it is influential as a preventive of these diseases, as well as eminently curative of the scarlet fever itself. I allude to the practice of giving tonics, such as quinine and iron, or iron alone, in tolerably large doses, proportioned to the severity or malignity of the disease. I have been in the habit for many years past of prescribing these tonics in bad forms of scarlatina, securing, at the same time, a moderate but still efficient relief to the bowels once or twice daily by castor oil or some other mild aperient, with the most favourable results; nor (which is of more interest in connexion with our subject) has any patient become affected with dropsy, so

far as I have been able to ascertain—certainly not in consulting practice. In hospital practice one cannot be so certain, for it is possible that some may have been attacked after they have been dismissed; but as most of the patients come from the neighborhood of the hospital, it is most probable that they would have returned if they had been attacked with dropsy. But suppose, in spite of all your precautions, a patient is attacked with this acute form of diseased kidney, accompanied by inflammatory anasarca, what steps are we to take, first, to prevent a partial or complete suppression of urine, and our patient from being carried off by convulsions and other modes of death under these conditions; and, secondly, to restore the organ to a perfectly healthy state, and so avoid its merging into the chronic, or large white kidney? This treatment must be modified according to whether the disease follows a malignant form of scarlatina, or is a sequel to a mild form of the fever. If in the latter case the patient will bear and will require a far more active treatment than in the former, and as a general rule, those of you who may settle in the country will find it necessary to treat it more actively by depletion, than those whose future labours may be in London or other large towns. I can most confidently inculcate this from a somewhat large experience in both. In the acute form, then, it may be necessary to take blood from the arm, the quantity being regulated by the urgency of the symptoms. Whenever the patient is threatened by convulsions soon after the commencement of the attack, this should never be omitted. I know no exception to this, except in the case of one who is anæmic, or who has been much weakened previously by the scarlet fever. In this case we must be satisfied with abstracting blood from the loins by leeching or cupping. This local abstraction of blood is of great service. Generally speaking, patients labouring under this form of kidney disease bear bleeding well. Next in efficacy, as in order of action, is the warm bath, great care being taken to prevent a chill after the removal from the bath. To avoid all risk of this the patient should be well rubbed by dry and warm towels, immediately that he rises out of the water, then placed in warm blankets, and put to bed. The warm bath is far more efficacious than the vapour or air bath, and the patient should remain in it until he makes complaint of a feeling of fatigue or of faintness. In country practice, and with the poorer class of persons, except in the case of children, this important remedy is not attainable. Under these circumstances a very efficient substitute can be extemporised, if the symptoms be such as to lead the practitioner to think that this mode of equalising the circulation, and securing a free transpiration through the skin is called for,—let the patient's bed be brought near a fire, or if he be strong enough, let him sit on a chair, and well supported. In either case he should be closely enveloped in flannel—for example, a blanket, and over this, two or three other blankets are to be loosely thrown, so that a sort of chamber may be formed between them. Make a tube of about one inch or one and a-half inch bore, by rolling two or three newspapers, or other large sheets of paper; introduce one end into the chamber so constructed, and place the other end over the spout of a kettle half filled with water in an active state of ebullition, and a most efficient vapour-bath is thus formed. The only precaution to be observed with respect to this extemporaneous vapour-bath is, not to place the end of the tube too near the patient's person, or too long in one situation, but to let the jet of steam fall in different parts. An early administration of an efficient aperient is always called for, even if the bowels have been moved spontaneously before your visit. The only circumstance prohibiting the administration of a purgative is actual looseness of the bowels—or rather actual diarrhœa. In some instances mere looseness may be co-incident with a loaded state of the bowels; if, therefore, you find the belly tumid and not very resonant, you will do well even in the case of there being a slight looseness, to give a dose of the compound jalap-powder: for example, from a scruple to half a drachm or even two scruples, according to the strength of the patient; if with children, the dose must of course be smaller. It is better that you should be called upon to administer a warm stimulant for the weakness, and to support the strength, than that a free action of the bowels and

of the skin, should not be produced, and due depletion resorted to. I could relate many cases where patients—chiefly young persons—have been seized with convulsions a few hours after being attacked with this form of kidney disease and anasarca, and have been placed out of danger in a few hours by a full abstraction of blood from the arm, followed by leeches at the loins and the warm-bath. If the compound jalap-powder be not at hand, or there be a disposition to vomiting, I see no objection, as some appear to do, to the saline aperients. If there be any nausea or vomiting, a dose of Epsom salts, with plenty of water, not only quickly produces several loose and even watery evacuations from the bowels, but also allays the disposition to sickness. These, then, are the great and immediate remedies that will be called for in most cases. The extent to which they should be carried, or whether one or more need be resorted to, will depend on the urgency of the case. This must be left to your own judgment. Bear in mind, as a golden rule, in the treatment of all diseases, that it is better to err on the side of too little activity in the treatment than on that of too much. Now, in most cases, you will have to continue your treatment. You may not have had an opportunity of attacking the disease with these remedies at a sufficiently early period, or at the onset of the attack. Well, then, it is necessary to keep up the action of the bowels daily, either by the compound jalap-powder, or the saline draughts. Two or three loose motions at least should be secured daily. You have seen in my practice at the hospital that in almost every case I order the haustus magnesiæ compositus of the Hospital Pharmacopœia, every four, six, or eight hours, according to the effect, (magnes. carb. gr. v.; magnes. sulph. ʒj.; aquæ menth. pip. ʒxij. Misce), with the most favourable results. You can bear witness that we have not lost a case in this acute form of the disease. Some recommend, and I have tried, the antimonial treatment,—either ten, fifteen or twenty minims of the vinum antimonii potassio-tartratis in the haustus ammoniæ acetatis, every three, four, or six hours; or antim. pot.-tart. gr. ¼ to ½ in a pill, with ext. hyos. grs. iij. to iv. From an experience of both of these medicines, I must confess I prefer the former, more especially as the patients are generally troubled with nausea, and, frequently, with vomiting. When this symptom is urgent, recollect that it is most probably (in the early stage of the acute affection) due to sympathy with the kidneys; and is an index, one may say, of the severity of the process going on there. In many cases it will be better under these circumstances to allow the stomach to remain at perfect rest, and not to administer any internal remedies. You will find the application of a few leeches to the loins, followed by the warm bath, and, if the bowels are confined, a laxative enema, a much more efficient means of arresting the vomiting than the internal administration of any medicines, the presence of which the irritable stomach will not tolerate, and a persistence in which is, therefore, calculated rather to keep up this embarrassing and exhausting symptom than to allay it. This symptom is often associated with severe headache, and tendency to convulsions, especially in children and young persons, and the same treatment will be equally appropriate for both. The constant application of a large linseed poultice, with which a few grains (a pinch or so) of mustard may be mixed, or of a large Markwick's epithem, or a piece of flannel, previously dipped in hot water and well wrung, will be of great service. If the flannel is used, it should be covered with gutta-percha, and over that by a thick layer of cotton-wool. If the haustus magnesiæ comp. after a few days produces great flatulency, and intestinal distension, which it sometimes does, you may then give the haustus ammoniæ acetatis, with one, two, or three minims of the dilute hydrocyanic acid, and a drachm, or two drachms, of the infusion of digitalis. In children the dose must be regulated accordingly, or these latter omitted. The last will be a safe diuretic at this period of the disease, and the hydrocyanic acid will tend to quiet the irritability of the stomach. With respect to diuretics in general, especially the stimulating diuretics, they are altogether contra-indicated: they are calculated in these acute forms to do a great deal of mischief. This does not apply, however, *to the* haustus ammoniæ acetatis; it acts as a diuretic, as I conclude from

experience, and from the results of Poiseuille's experiments, by the rapidity with which it passes through vessels, not seemingly by stimulating their walls, but by some other property. As a general rule elaterium, and other drastic purgatives, are not advisable in these acute affections. It is true that during their action they produce several loose and watery evacuations, and may afford temporary relief to the system, yet they unduly stimulate the mucous membrane, and render it, or its capillaries, unfit for the continuance of profuse secretion. The Epsom, or what is better, although undeservedly but little used, the Glauber's salts, act even more efficiently in the first place, and do not leave the membrane and other capillaries (so to say) exhausted.

At first, the severest antiphlogistic diet should alone be permitted. In general, but little diet is wanted. The patient ought to be restricted to gruel, arrow-root, and such articles of food, and no nitrogenous matters allowed,—such as beef tea, animal broths, and so on. It is impossible, however, to decide for every case. You must exercise your own judgment, and be guided in this respect by the symptoms and state of your patient. After the acute stage is over, and the urine has become abundant, the tube casts—epilethial and bloody,—and the blood-corpuscles have disappeared, the skin has become cool and soft, and the pulse reduced to nearly the natural standard, and the other feverish symptoms subsided: if there be still anasarca, and there be very evident anæmia, you may prescribe the tincture of the sesquichloride of iron in doses of five, ten, or fifteen minims (according to the age and condition of your patient), three or four times daily, in a small wineglassful of water, and you may cautiously increase these doses. I seldom, however, find it necessary in these affections, and under these circumstances, to give more than fifteen minims even to an adult. This preparation (most probably by its styptic properties) will, generally, lead to the disappearance of the albumen, at the same time that it improves the quality of the blood, and gives tone to the system generally.

When the disease is produced by cold, the same general principles of treatment are to be acted upon. The only difference is, that the patients, when affected from this cause, bear depletion better than after scarlatina, and the warm baths should be more frequently repeated.

When it is the result of alcoholic poisoning, or any other direct irritant of the tissues of the organ, the cause acting more locally, general depletion will rarely, if ever, be called for. But local depletion ought to be more freely resorted to, and the action of the bowels promoted by enemata, in preference to purgatives by the mouth.

This, then, is the plan of treatment which I have found most successful in the acute forms of these diseases. In all cases you must be guided by the symptoms. In scarcely any will stimulation be justifiable.—*Med. Times and Gazette, Dec.* 22, 1860, *p.* 601.

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## 31.—ON THE ORIGIN OF UREA.

By Dr. George E. Day, Professor of Medicine in the University of St. Andrews.

[In the following extract from his recent work on 'Chemistry in its relations to Physiology and Medicine,' Dr. Day has succeeded admirably in throwing together, within a reasonable compass, all that is known on the origin of urea.]

It is well known that the origin of urea is still a *quæstio vexata* amongst chemists and physiologists; one party, including the names of Liebig, Bischoff, and others, holding that the urea is solely a product of the metamorphosis of the nitrogenous tissues; while the other party which ranks amongst its supporters Lehmann, Frerichs, and (more especially) Bidder and Schmidt, maintain that the formation of urea is dependent upon two factors, one of which is variable, namely, the amount of assimilated histoplastic or albuminous food; whilst the other is constant, namely, the necessary consumption of the albuminous tissue when the animal is fasting.

It admits of no doubt that urea is formed from the nitrogenous constituents of the organism, its artificial production from such substances affording the strongest evidence on that point; in addition to which we may add the facts observed by Lassaigne, Scherer, and others, of urea being contained in the urine excreted after nearly three weeks' starvation. As the metamorphosis of tissue occurs with the greatest activity in the muscular system, and as, further, increased bodily exercise augments the amount of urea, we are justified in regarding the urea as formed for the most part from the worn-out muscular fibres, although it is most probable that other vital tissues may contribute to the general amount. Whether it is formed in the organic particles at the moment of their disintegration, or whether it is first formed in the blood, is a point which cannot be considered as decisively established; but it is most probable that the latter is the correct view, because Liebig, in his experiments on large quantities of muscular juice, could not detect in it any trace of urea, although he found substances from which he could produce it artificially. It seems, therefore, almost certain that these substances (creatine, creatinine, and probably inosic acid) are decomposed in the blood, by the action of the alkalies, and of free oxygen, into urea and other matters to be excreted. Moreover, the view that the urea is formed in the blood is supported by well-known experiments, showing that gelatin, glycine, alloxantin, theine and other substances, which it is impossible to suppose can form tissue, are converted into urea and other matters, as is evidenced by the fact that this substance occurs in a perceptibly increased quantity in the urine, soon after any of the above-named substances have been swallowed.

Lehmann's view that the urea is in part formed from assimilated nitrogenous food which has never entered into the substances of the tissues, is chiefly based on the following facts:—(1.) on the extent to which its amount is increased by the free use of animal food (nature in this way getting rid of the superfluous plastic material along with that which has become unfit for use); and (2.) on the circumstance first noticed by Frerichs, but mainly established by the investigations of his opponent, Bischoff, that the use of gelatin and gelatinous food so rapidly increases the quantity of urea, that we are compelled to believe that these nitrogenous matters are at once directly oxidised in the blood, without having entered into the composition of the tissues; and if these why not the protein bodies also? (We should observe that Bischoff himself fully grants that the gelatin is directly converted in the blood into urea, but, he adds, it is never a natural article of food, nor is it ever found as a normal constituent of the blood.)

When treating of uric acid we shall show that in all probability a great part of the urea, separated by the kidneys from the blood, had previously existed in the form of that organic acid.

Whether urea exerts any special influence on the fluids of the eye is a question that no one has yet attempted to answer.—*Glasgow Med. Journal, Jan.* 1861, *p.* 464.

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## 32.—ON THE INFLUENCE EXERCISED BY VARIOUS DISEASES ON THE QUANTITY OF UREA EXCRETED.

By Dr. George E. Day, Professor of Medicine at the University of St. Andrews.

To the remarks which have been previously made on the influence of disease in modifying the amount of urea we may add the following. In intermittent fevers it has been found (by Moos, Traube, and Redtenbacher) that, during the paroxysm, the secretion of urea and of urine generally is absolutely increased, while in the period of intermission there is a diminution both of the urea and of the urine generally. In the cold and hot stages about 3·5 times, and in the sweating stage about 0·5, more urea are secreted than in the period of intermission.

Warncke's investigations in reference to typhoid fever are of such importance that we shall borrow freely from his memoir. "The urine of a

patient labouring under typhoid fever is not distinguishable by external characters from any other. It may be pale, it may be excreted in large quantity, and may be clear or may be turbid, opaque, deposit a copious sediment, and be secreted in small quantity (which is particularly the case in the commencement of the disease); but its special characteristic is that it contains *an absolute and relative increase of* urea.

"This property is retained by the urine through the stages to which the disease may be said to increase; when, on the contrary, the latter diminishes the amount of urea is likewise lessened, and the quantity continues smaller after the end of convalescence, until the restitution of the body is completely effected.

"The following table exhibits the average numbers of more than fifty investigations (on thirty men and twenty women), which were repeated daily during the stay of the individuals in the hospital:—

| | Males. | Females. |
|---|---|---|
| In the first week | 43·2 grammes | 34·0 grammes |
| " second " | 39·9 " | 30·2 " |
| " third " | 30·9 " | 24·1 " |
| " fourth " | 23·2 " | 20·5 " |

"If these numbers be compared with those representing the quantity of urea which is normally excreted, especially under the use of vegetable food, and there are very few patients that use even it, it must be allowed that increase of the quantity of urea is a constant phenomenon in typhoid fever. This character, however, is not peculiar to typhoid fever, for in many other acute diseases, as in pneumonia, pleurisy, rheumatic fever, &c., the quantity of urea is increased; but in all these other organic constituents of the urine are simultaneously augmented, while this takes place with the urea in a less degree. We cannot attach too much importance to this circumstance. An increased quantity of organic matters in general indicates an augmented supply of combustible materials; but an increased quantity of urea without any simultaneous augmentation of the other organic compounds in the urine, as is the case in typhoid fever, points, on the contrary, to a more energetic combustion." Warncke goes on to show that the increase of the quantity of urea is not equally great in all typhoid fevers, and that it varies directly with the height of the temperature, the rapidity of the pulse, and the degree of emaciation.

A diminution of the quantity of the urea, though never to a degree below the normal standard, occasionally (but only seldom) took place; it was especially observed after violent hemorrhage from the bowels, and when there was considerable enlargement of the spleen.

In many cases the patients, during the entire course of the disease, took food regularly; but in none of them was the consumption of food followed by an increased quantity of urea.

Warncke lays great stress on the value of the determination of the urea in reference to differential diagnosis. "When I find," he observes, "the increase of urea without any augmentation of the other organic constituents of the urine to be peculiar to typhoid fever, I am easily led to seek in this fact a diagnostic sign between this affection and the diseases which in other symptoms resemble it, especially gastric fever and meningitis."

In gastric fever he found that the quantity of urea was not very different from what is excreted by healthy persons when living on a vegetable diet. From an extended series of analyses he obtained the following mean numbers:—

| | Males. | Females. |
|---|---|---|
| For the first week | 22·1 grammes | 18·0 grammes |
| " second " | 24·2 " | 19·8 " |
| " third " | 25·7 " | 20·4 " |

Thus we have here the reverse of what occurs in typhoid fever. The quantity of urea is less in the first weeks than during convalescence.

In meningitis the differences in relation to the amount of urea are at least equally marked, according to Warncke. In a boy aged seven years, who

died from meningitis, the mean daily quantity of urea was 6·4 grammes, while in a boy aged eight years, who died from typhoid fever, the minimum quantity of urea was 11·9 grammes. Heller mentions meningitis as a disease characterised by an excessive excretion of urea. If Warncke's views are correct, it is possible that in Heller's cases there was an error in diagnosis.

In diabetes there seems (from the investigations of Thierfelder and Uhle) to be a definite ratio between the daily quantity of nitrogenous food and the daily excretion of urea."—*Glasgow Med. Journal, Jan.* 1861, *p.* 467.

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## 33.—ON URÆMIA.

By Dr. Richardson.

[The following is an abstract of a paper read before the Medical Society of London. The author first pointed out the analogies between uræmic poisoning and poisoning by certain common narcotic substances, and then passed on to the symptoms and diagnosis from other forms of disease. Three points in this part of the paper may be mentioned as of interest.]

The pupil, Dr. Richardson has observed, is usually fixed in uræmia, and, in most cases, is dilated; but, he added, this rule is not without exception, for he has seen the pupil contracted to a pin's point in a case of unmistakeable uræmia. There is in some cases, as Frerichs has said, evidence of an excess of ammonia in the breath during the acute attack; but this is not universal, and hence some have denied it altogether. The reason of a difference in this respect in different cases, is very simple. In persons suffering from kidney disease, and in whom uræmia is a probable occurrence, the breath at the best times is charged with ammonia to an extent greater than is normal. In these cases, the lung is supplementing the kidney, and the elimination of the ammoniacal product is, in fact, the saving clause. It is when such persons take congestion of the lung, and are subjected to diminution of excretion by the lung, that the uræmic symptoms advance; and in these cases the breath is not ammoniacal during the attack. But there are other examples, where the uræmia is sudden in its appearance, owing to sudden arrest in the function of the kidney simply. Then the breath is markedly ammoniacal in the period of the acute attack. The third fact, as diagnostic of uræmic poisoning from poisoning by the ordinary narcotics, is, that during uræmic coma the patient will often rally and regain all his consciousness for a time, sinking again into forgetfulness, and even dying unconscious in the end.

The cause of death in uræmia formed the matter of another section of the paper, and was followed by observations on treatment. In the treatment of uræmic narcotism coming on suddenly, in a person not debilitated by previous disease, and not overloaded with fat, Dr. Richardson stated his belief that there was one ready and direct remedy, and that was free bloodletting. He had seen a man who had lain three days comatose and unconscious recover, under the immediate influence of loss of blood, so completely as to transact business affairs, and inquire into all that had occurred since he was struck down. Moreover physiological reasons supported this treatment for the bloodletting not only relieved the body from a portion of the poison, but removed the congestion of the kidney and of the other organs, and gave the permit for recovery, if recovery were possible. Thus, in animals in which artificial uræmia had been produced, the effect of frequent venesection tended greatly to prolong life. That bloodletting should not absolutely relieve in every case was reasonable, for whether relief were obtainable or not in any case would depend upon the degree of mechanical obstruction in the kidney; for if the obstruction were perfect, no treatment would be possible, seeing that no proceeding could be adopted to supplement the kidney altogether; but if in any instance there could be but partial abstraction, increased temporarily by congestion, then the act of abstraction of blood gave

the only chance that remained of removing the burthen from the excreting organ. The last part of the paper dwelt on uræmia in its forensic aspects. In many cases, where death is supposed to have occurred from the effects of small doses of opium or other narcotic, he (Dr. Richardson) believed that the cause was attributable to uræmia, and that so-called idiosyncrasies were probably intimately connected with renal disorder.

He did not believe the poison in uræmia was simply urea, but thought that it was either carbonate of ammonia or one of the ammonia series in which carbon enters as an element.—*Lancet, Nov.* 17, 1860, *p.* 488.

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## 34.—CLINICAL REMARKS UPON DIABETES.

By Dr. W. Hughes Willshire, Assistant Physician to, and Lecturer on Medicine, at Charing-Cross Hospital.

[The patient whose case forms the subject of this paper, is a married woman, 29 years of age. Her symptoms began in 1857. Frequent micturition and intense thirst were as usual the prominent symptoms. A year after the commencement of these symptoms, the sight became impaired, and vision gradually entirely went away. She entered the hospital at first on account of this loss of vision and was under the care of Mr. Hancock.]

We smelt her breath, and noticed that it had a very marked sweet-apple or chloroform-like odour, though not such a strong one as had another diabetic patient of mine up-stairs, the approach to whose bed was something like going near an unstoppered bottle of chloroform. There is no exaggeration in this statement; some of the last session's students will bear me out in what I say, as they were witnesses to the fact.

Now these were general and vital signs, all pointing to the presence of sugar in the patient's urine. Well, then, we examined her urine. We found it voided in quantity between eleven and twelve pints in the day. It was a very pale straw colour, sweetishly faint in odour, clear, and having a specific gravity of fully 1040. Upon applying the tests of Trommer, Moore, and using the solution of Barreswil, we found the presence of sugar abundantly demonstrated. Some of you will recollect that we afterwards put some yeast to the urine, and so availed ourselves of the fermentation test. This test we also had recourse to again only the other day. It was clear, then, that this woman had glucosuria, and was passing a considerable amount of solid matter in the urine during the twenty-four hours. We examined her lungs, but could not detect that she had that common accompaniment of diabetes—viz., phthisis. But if she was not consumptive, she was *blind*. Mind, this is a point of great interest in this case. She was blind, I say; she had *cataract*, and cataract of both eyes. Before, however, I touch further upon this matter, let me tell you what we did for the diabetes. I ordered her a grain of quinine and a grain of opium three times in the day, an occasional warm bath, and purgatives when necessary. All vegetable matters, except coarse brown bread with the bran in it, and which is made, I may remark, purposely for us by one of our governors, (Mr. Robb in St. Martin's lane,) were prohibited. A rigorous embargo was laid against sugar, but *four* meat diets, with extra milk and eggs, were allowed her to make up for these denials. Soon after this plan of treatment was put into force, she began to improve; she got much stronger, and the quantity of urine lessened, and the gravity of it became often much lower. She ceased to rise to micturate in the night, the bowels became more manageable, and the thirst much less intense. Sufficient for me to say, she now passes (Oct. 15th, 1860) four pints and a half of urine during the day, the specific gravity of it being from 1038 to 1040. This quantity, with an occasional lower degree of gravity, has been the rule for some months past. Once the quantity went down to three pints, and had a gravity of 1026, but sugar was still contained in it. Then both quantity and gravity rose again, and do what I may, I cannot get it less than four pints, with a gravity of 1038, for any length of time. The

only addition to her treatment up to quite recently has been, that the opium and the quinine were each increased to two grains three times in the day. Once for a few days the patient felt so well, that she could not refrain from eating some potatoes. The urine rose in quantity and gravity, and she had a slight attack of diarrhœa. But she has undoubtedly gained flesh since she has been under the treatment I have mentioned.

Now to revert to her blindness. You hear the patient had cataract of both eyes, for the relief of which she had sought Mr. Hancock's skilful assistance. Let me now inform you that an American physiologist, Dr. S. Weir Mitchell, published in January of the present year a memoir showing that cataract could be produced in frogs by introducing sugar into their system; and his farther experiments proved that a peculiar form of cataract was a pretty constant attendant upon "sugar poisoning." This paper coming across the notice of a gentleman whose genius for physiology is only equalled by his acuteness and persevering energy—namely, Dr. B. W. Richardson,—served at once as a hint to him for undertaking what he has called the "synthesis of cataract." From March last up to the present time, I believe, he has been more or less occupied in its investigation. He first began by introducing syrup-of-cane sugar into the living system of fishes and frogs, and, as a rule, it may be said that "sugar poisoning" and opacity of the lenses were produced. Dr. Richardson then experimented with grape-sugar and with syrup-of-milk sugar, and the like results followed. He then passed on to mannite, and the effects were the same; but in the next experiments with liquorice there was a want of agreement. A grand point now with Dr. Richardson was to be enabled to introduce true diabetic sugar into the living organism. I am glad to think that my colleague, Mr. Hancock, and myself were enabled to place our patient, Phœbe E——, at Dr. Richardson's disposal for this purpose. We had the pleasure, some of you will recollect, of seeing Dr. Richardson at this hospital during the early part of the summer. He obtained from us several pints of the patient's urine, with which, to use his own expression, he "made numerous interesting experiments;" some of these I may add, were performed at Bedford on June 15th, before the members of the South Midland Branch of the British Medical Association. As a rule, again, symptoms of "sugar poisoning" were produced, and the cataractous condition was "markedly brought out." Other series of experiments have also been continued by him, but for further information about this gentleman's inquiries I must refer you to his papers now publishing in Dr. Brown-Séquard's *Journal de la Physiologie* (No. 12.)

Attention was now directed to the chronicles of *pathology*, to discover if they gave any support to this physiologic view of a causal connection between diabetes and cataract. The most concise *résumé* of what is to be obtained in this field you will find in the last volume of Guy's Hospital Reports (Third Series, vol. vi., p. 266), in a paper by Mr. J. F. France. He there tells us that he himself drew attention to "diabetic cataract" in January, 1859, in the Ophthalmic Hospital Reports, but that Dr. Mackenzie, of Glasgow, had alluded to it in 1854. More recently Messrs. Duncan, Wilde, Walton, Veasey, Sloane, Newman, and Barton have instanced cases bearing upon the point. These you will find specifically in Mr. France's paper. I have referred to Schauenburgh's "Ophthalmiatrik" (p. 114); there I find it stated that diabetes is very often followed by soft cataract, and Unger's name is brought forward in support of the assertion. In the *Lancet* of April 28th, 1860, (p. 424), there is a note to the effect that Graefe had stated, in the *Deutsche Klinik*, that diabetes was very frequently the cause of cataract. It is right to mention, also, that Dr. Richardson admits both Dr. Mitchell and himself have been forestalled by Kunde, who some years ago travelled far in the same direction—the synthesis of cataract (*Journal de la Physiologie*, p. 450). With regard to "diabetic cataract," Mr. France generalizes thus: the cataractous condition is symmetrically developed upon both sides; the lenses increase very largely in their antero-posterior diameter; the cataracts are of soft consistence; and the ocular affection comes on only after the diabetic state has existed for some time, and

there has not in any case been reason to suspect further disease of the eyeball (*op. cit*).

With regard to our patient, I may remark that Mr. Hancock first operated upon her on June 9th; and he informs me that the cataracts were double and of soft consistency, and that he did not observe that the lenses were larger in any direction than usual. He likewise says that absorption did not by any means proceed so rapidly after the operation as it usually does. The operation performed was that called "breaking up." Mr. Hancock again "broke up" the lenses upon October 8th, and he tells us that although the lens in either eye is not as yet absorbed, the patient is beginning to see very satisfactorily, particularly with the left eye; so that the result of the operations may be considered favourable. Soon after the last operation was performed some inflammation of the left eye occurred, followed by a slight adhesion of the iris. Mr. Hancock and myself had some talk together about suspending the quinine and opium and giving a little mercury. Although not without some misgivings regarding the possible effects upon the diabetic condition, the last mentioned drug was had recourse to, and certainly with benefit to the visual organs. I have been very anxious about the urine, therefore, and have several times examined it. To-day we made some inquiries, you will remember, about this excretion in the wards. The specific gravity of the urine we found to be 1040; the quantity rather more than four pints; and the presence of sugar was indicated by the various tests we employed.—*Lancet, Oct.* 27, 1860, *p.* 404.

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## 35.—ON CERTAIN POINTS CONNECTED WITH DIABETES.

By Dr. Frederick W. Pavy, Professor of Physiology and Assistant Physician to Guy's Hospital.

[It is but ten years since the discovery of a sugar-forming function for the liver was announced. It rapidly became an established doctrine in physiology. At first Dr. Pavy was a strong advocate of this glucogenic theory, attributing a result of his own experiments to some source of objection in the experiment. Other reasons, however, gradually convinced him of the erroneousness of the usually held opinions, being convinced that the liver is not specially intended as a sugar-forming organ. He says:]

Of the varieties of sugar, it is the glucose, or grape-sugar, that we shall have to deal with in these lectures. Now, as it is of the greatest importance that we should possess information about the means adopted for recognising our agent, I will first speak of the tests that are resorted to for determining its presence and its quantity. Happily, here we do not encounter any difficulty, for grape-sugar is almost as easily detected as any inorganic material, even when present in an exceedingly minute quantity. Numerous tests have been from time to time recommended, but the following are those that, on account of convenience or otherwise, are generally used:—

Moore's test consists in treating the specimen with a solution of potash and then boiling. The alkali decomposes the sugar, resolving it into a dark coloured substance, which renders the liquid more or less of a sherry or brownish hue. I do not consider this test a desirable one for physiological purposes. Where the quantity of sugar is large, as in diabetic urine, the effect is so strongly marked that there cannot be any mistake in recognising it; but where only a minute quantity of sugar is present, there is only a shade of difference to discriminate, and, with the best intentions, we may often be deceived, where only shades of difference in colour are concerned. What, in physiology, we require, is something giving a more decided "aye" or "nay," and this we get with the various copper tests, and with fermentation.

In using Moore's test for the detection of diabetes, a drachm, say, of urine is treated in a test tube with about half its bulk of liquor potassæ, and boiled for a couple of minutes over a spirit lamp. It must be borne in mind that a fallacy—and I have frequently seen it—may occur. If the liquor potassæ have been kept in a white or flint-glass bottle, it becomes contaminated with

lead. Lead may also be abstracted from the glaze of an earthenware dish, in which the potash may have been boiled or evaporated down. At all events, from whatever source lead may have been derived, on boiling liquor potassæ contaminated with it with any organic compound containing sulphur, the sulphur is liberated, and, combining with the metal, produces a more or less dark-coloured liquid, according to the amount of sulphuret formed.

Amongst the copper tests we can have our choice; but in all, the principle of action is the same. Free protoxide of copper is present, which, on being boiled with grape-sugar, loses half its oxygen, and is thrown down as a yellow, orange-yellow, or orange-red precipitate, according to the amount of sugar present, which possibly determines the state of hydration of the deposit. A liquid containing a large proportion of sugar gives an orange-red deposit; whilst, when there is only a slight proportion of sugar present, the sub-oxide thrown down assumes a yellow colour.

In Trommer's test, the oxide of copper is set free at the time of application. A drop, or a couple of drops, of a moderately concentrated solution of sulphate of copper are let fall into the specimen to be tested, and then liquor potassæ is added in considerable excess. The precipitate at first thrown down on the addition of the alkali is redissolved if sugar be present, and a deep-blue liquid is the result, which, on boiling, deposits the reduced or sub-oxide precipitate. The objection to this test is, that it is not so convenient in practice as the copper solutions to which I am about to refer; and, where only traces of sugar are present, they may escape detection, on account of undissolved protoxide obscuring a slight production of the reduced oxide.

Of the solutions of copper, Barreswil's liquid is by far the most celebrated. It is this form of copper test that has been employed by Bernard in his researches. Reduced to our English scale of weight and measurement, it is thus composed:—

| | |
|---|---|
| Bitartrate of potash (cream of tartar) | 960 grains. |
| Carbonate of soda (crystallized) ... | 960 " |
| Caustic potash (potassa fusa) ... ... | 640 " |
| Sulplate of copper ... ... ... ... | 320 " |
| Distilled water ... ... ... ... | 20 fluid oz. |

These materials are to be dissolved, mixed, and the resulting blue liquid to be filtered.

Barreswil's liquid is thus a rather complex solution. Essentially it consists of oxide of copper dissolved in an alkali by the presence of an organic material (tartaric acid), which does not occasion its reduction at the temperature of ebullition. It is the property of the protoxide of copper not to be soluble in an alkali unless some form of organic matter be present. Hence the necessity of the tartaric acid, or some such agent, in the cupro-potassic solutions. But why use the bitartrate of potash, which calls for the employment of the carbonate of soda to neutralize its excess of acid? It is more simple to take at once the neutral tartrate of potash, and then the carbonated alkali may be dispensed with. Fehling's solution is of this description, and consists of sulphate of copper and tartrate of potash, with soda as the alkali.

I can recommend the following as a simple method of preparing a copper solution for use in clinical practice in the detection of saccharine urine. Take five grains of sulphate of copper and ten grains of tartrate of potash, and dissolve in two drachms of liquor potassæ. A clear deep-blue liquid is formed, which is quite as efficient as any other kind of cupro-potassic test. The liquor, however, that I use and keep ready prepared in my laboratory is made with a larger proportion of potash than what is contained in the liquor potassæ—a condition necessary to enable the test to be preserved in an efficient state. The following are the proportions of the ingredients used:—

| | |
|---|---|
| Sulphate of copper... ... ... ... | 320 grains. |
| Tartrate of potash (neutral)... ... | 640 " |
| Caustic potash (potassa fusa) ... | 1280 " |
| Distilled water ... ... ... ... | 20 fluid oz. |

Dissolve each in a separate portion of the water, then mix the sulphate of copper and tartrate of potash, and afterwards add the alkali.

The precipitation of sub-oxide from any of these copper solutions on boiling must not be looked upon as affording an infallible indication of the presence of sugar; neither, on the other hand, must the absence of a precipitate be regarded as absolutely proving the absence of sugar. Such a remark must seem materially to detract from the value of testing with a copper liquid; but bearing in mind certain considerations, I do not think there is much chance of falling into error, and certainly the copper is the most useful test that we have.

It is said that glycerine, tannine, cellulose, leucine, uric acid, and chloroform are each capable of producing, in different degrees, a reduction of the oxide of copper. Chloroform certainly exerts a strong reducing effect. Uric acid I have observed to occasion some deposit of red oxide; and from cotton I have obtained just a trace of precipitate. But the glycerine I have tested has not yielded the slightest vestige of reaction. A circumstance, however, of great importance is, that if a copper solution be kept for any considerable period, and particularly if exposed to light, it will of itself deposit some red oxide on boiling. Hence, unless the solution have been recently prepared, it should be tested, by boiling a little, alone, from time to time; and if it should be found to give a precipitate, a fresh addition of potash will restore it, rendering it again as fit as ever for use.

But under certain circumstances no deposit of sub-oxide will take place, although sugar may be indisputably present. Ammoniacal salts have the property of occasioning this. I have frequently noticed where an ammoniacal odour is evolved on boiling a specimen with the blue liquid, that at first there has been no perceptible change; then a change of colour, without any precipitation, has taken place; and if the boiling has been continued so that the potash of the blue liquid has expelled the whole of the ammonia, a considerable deposit of sub-oxide has afterwards fallen. The presence of albumen interferes with the proper reaction of our copper test: I expect, on account of the ammonia evolved, as the result of destruction by the potash in the test. In a case of apoplexy where the urine was albuminous I tested for sugar, expecting to find it, because there was sudden and great congestion of the circulation. The action of the test was obscure; but on boiling the urine and filtering before applying it, a neat precipitation of sub-oxide occurred.

With a fluid like blood, a process of preparation for testing is of course absolutely indispensable. Boiling does not get rid thoroughly of the albuminous and colouring matters; its alkalinity enables the liquid to retain some of these. The cautious addition of acetic acid to neutralization will effect their entire precipitation; but a little too much acid produces the condition that was intended to be removed. I consider this process troublesome, and always resort to the use of the sulphate of soda—a material which does not in the slightest degree interfere with our test, and which furnishes us with a perfectly clear liquid from the blood. The blood is simply mixed with about an equal weight of sulphate of soda, and heated to effect coagulation in a small porcelain capsule. Thrown on a filter, a limpid transparent liquid immediately runs through.

Bernard has more recently recommended animal charcoal for separating albuminous and colouring matters from a liquid, where we are desirous of applying the cupro-potassic test. According to Bernard, it is the property of animal charcoal to abstract certain materials and not others from a liquid. Glucose is a substance which it does not remove, but the albumen and colouring matters are completely separated by it from the blood, albumen and uric acid from urine, and even the caseine and fatty matter from milk. It is sufficient, therefore, to treat an animal fluid with the necessary quantity of animal charcoal; and upon filtration a clear liquid will run through, which retains any sugar that might have been present, although deprived of many other substances.

A solid substance may be prepared for testing, either by making a plain

decoction of it, or else by pounding it in a mortar with the sulphate of soda, heating, and filtering in the same manner as with blood.

Although just a point of doubt might be attached to the indications afforded by the other tests, yet with the fermentation test we have been hitherto accustomed to look upon its reaction as perfectly characteristic of the presence of sugar. It seems, however, from the researches of a French chemist, M. Berthelot, who has recently devoted much time to the chemistry of the sugars, and whose statements are entitled to our greatest consideration, that there are other substances besides sugar—viz., glycerine, mannite, dulcine, and sorbine, which are capable of undergoing the alcoholic fermentation with yeast. Alcoholic fermentation, therefore, can no longer be regarded as affording an infallible indication of sugar.

In applying the fermentation test, it is necessary that the yeast should be thoroughly washed before use. Yeast, as purchased, will undergo a considerable fermentation alone, and even after being well washed, I have found it give rise to the evolution of a slight amount of carbonic acid. In fact, the fermentation test in my hands has not proved susceptible of that delicacy and absolute certainty I could have desired. The nature of the gas evolved may be determined by the action of the potash, and the presence of alcohol in the liquid may be shown by distillation, and then mixing the distilled product with unslaked lime, and heating in a convenient apparatus for igniting the vapour that passes over.

It now only remains for me in this preliminary part of my subject, to speak of the mode of conducting the quantitative determination of sugar. The best process I know of is with the copper solution, and this, I am satisfied from considerable experience with it, is susceptible of the utmost delicacy and precision. We cannot separate and weigh the sugar as the chemist does with an inorganic material; but we estimate its amount by its reducing or deoxidizing effect on a copper solution of determined strength. The liquid I employ—that composed of potash, tartrate of potash, sulphate of copper, and water in the proportions I have already given—is of such a strength, that 100 minims of it are exactly decolorized by half a grain of the purest well-dried grape sugar that I have been able to obtain. In other words, half a grain is the exact amount of sugar required to convert the oxide into the sub-oxide contained in 100 minims of my blue liquid. To give an example, I will describe how I proceed in the case of diabetic urine. The quantities are measured in graduated tubes, drawn at one end to a point so that the liquid may be dropped as required. 100 minims of the blue liquid are taken, and a mixture of one part of urine with five of water (because the urine is too concentrated alone) allowed to fall into it drop by drop whilst kept boiling in a small porcelain capsule over the flame of a spirit-lamp or gas. As soon as the blue colour of the copper solution has been completely removed, the operation is at an end. The amount of diluted urine employed can be read off, and will contain an equivalent to half a grain of glucose.

Should the fermentation test be used quantitatively, according to Dr. Christison, every cubic inch of carbonic acid evolved corresponds, in round numbers, to a grain of sugar; or, more precisely, 47 cubic inches of gas are equivalent to 45 grains of sugar.

There can be no doubt that sugar is an important material both in regard to animal and vegetable life. It exists most abundantly in the vegetable kingdom, and almost every article of our food derived from this kingdom contains it, in some form and to some extent or other. Being soluble, and of a highly diffusible character, it readily passes, by a physical process, from the stomach and intestine into the circulatory system. The action of the bloodvessels, in respect to absorption, is purely physical; whilst the special absorbent system of the intestine, as it commences in the villi, has a peculiar selective power, picking up certain materials (oleo-albuminous), and rejecting others, in a manner that cannot be explained by physics. Sugar, *salines*, and the such-like, in conformity with the laws of endosmose and *exosmose*, pass into the blood of the portal system, and are carried to the

liver before entering the general circulation. It appears that the sugar is arrested in the liver, and converted by it into a material which is found pretty largely in the healthy organ.

In the vegetable world, there is a substance most extensively found, which presents far different properties to sugar, yet is transformed into it with the greatest rapidity when the requisite conditions are present. This substance —starch—is of no use, I apprehend, as such, either to the animal or the plant. It forms a store of carbonaceous material, which may be kept for centuries without undergoing change; and which, on the other hand, is most easily changed when placed in a condition to be made use of in the active processes of life. A grain of wheat, excavated with the mummy belonging to ages past, retains its starch unchanged. Supply this grain of wheat with warmth, air, and moisture, and active life immediately commences. It is now that the starch is transformed; the condition that has created a demand for it, leads to its transformation into a body which is capable of being appropriated to the wants of growth.

Diastase is the principle developed during germination, which occasions the metamorphosis of starch into dextrine and sugar. It seems to me exceedingly doubtful, however, if starch passes on from dextrine into sugar during germination to the extent that is imagined. Certainly the quantity of sugar contained in malt must not be taken as an index of the quantity of sugar existing actually in the germinating grain. I have allowed barley to grow in a temperate atmosphere, fully to the extent that is done by the maltster, and on making a cold infusion of it, have obtained an astonishingly slight indication of the presence of sugar. On the infusion being warmed, or on the growing barley being exposed to an elevated temperature—as is done in malting on the kiln—a large production of sugar is the result. It is true, that without the application of heat, when barley has grown to much extent, a quantity of sugar is to be found in it.

In the animal kingdom starch must likewise be transformed before it is fit for appropriation. The pancreatic juice is the secretion which seems specially intended for this purpose. At 90° to 100° the pancreatic juice rapidly converts starch into sugar. This is most easily shown with the secretion obtained from the dog during life. Human saliva converts starch into sugar with an equal rapidity; but it is a curious fact that the saliva of the dog, and of the horse—animals in which the examination has been made by Bernard—does not possess a corresponding power. It is a fact that I can attest from my own experience, that the secretions collected from the large salivary glands of the dog, either taken separately or mixed together, have no action whatever towards converting starch into sugar. It appears, however, that the fluid derived from the mouth possesses, to a slight extent, a transformative power.

That it is not the special purpose of saliva to act upon the amylaceous constituents of our food is to be inferred from other considerations besides the difference just alluded to in the extent of power it enjoys amongst different members of the animal kingdom. The starch is seldom ingested as food under such favourable circumstances for transformation as in our operations out of the body where a decoction is employed. The food is delayed but a comparatively short period in contact with the saliva in the mouth, and, supposing any action to take place in this cavity, it must be checked upon the arrival of the mixture in the stomach; for it can be shown by experiment that our saliva exerts no transformative effect in the presence of an acid.

In the case of the pancreatic juice, the conditions are most favourable for the exercise of its transformative power. The food has been reduced to a semi-fluid state, and can be thoroughly incorporated with the secretion that is poured out. The acid of the chyme is neutralized by the alkali of the bile. The temperature is elevated and equable, and the passage along the intestine is slow, so that the starch is delayed in contact with its metamorphosing agent. Probably the secretion from Brunner's glands, and possibly that from the surface of the small intestine, assist the pancreatic juice.

which also has an office to perform in connexion with the emulsification or preparation of fatty matters for absorption.

Such are the sources of sugar that were known previous to Bernard's time. Now, after an attentive examination of the circumstances connected with diabetes, Bernard was led to think that there might be something besides amylaceous matter—something unknown to chemists and physiologists, which might give rise to the production of sugar in the body. This he particularly inferred from the fact that the quantity of sugar escaping from a diabetic patient, under a mixed diet, is much greater than can be accounted for by the starch and sugar ingested; and that, although saccharine and amylaceous materials may be completely abstained from, still sugar does not cease to appear in the urine. Here was his motive for undertaking an experimental investigation, out of which has sprung such unanticipated results. In his first experiments, animals were kept upon a diet of a strongly saccharine character, with the view of ascertaining how far the sugar could be followed in the circulatory system, after its absorption into the branches of the portal vein. His results led him to the conclusion that it might be detected as far as between the liver and the right side of the heart. To show that the sugar encountered at this point was what was derived from the food, a counter-experiment—an experiment where starch and sugar were excluded from the diet—was made, with the expectation of obtaining a negative result. Instead of this, however, to Bernard's great astonishment, sugar was found as before; and thus was laid the foundation of the theory which has given to the liver a gluco-genic function. It was in the year 1848 that the discovery of this function was announced.

Bernard's celebrated experiment, which was regarded as establishing the existence of a sugar-forming function in the animal system, was this:—A dog was kept for some time on a purely animal diet, and was, therefore, prevented from receiving any sugar from an external source. Its life being suddenly destroyed, the abdomen was immediately opened, and a ligature placed on the portal vein. The blood flowing from the chylo-poietic viscera towards the liver was obstructed, and distended the vein below the ligature. An incision into the distended vessel allowed the blood to escape, and this on being collected and tested, gave no indication of the presence of sugar Blood was now procured from the hepatic veins on the other side of the liver, and was found to be highly impregnated with saccharine matter. The liver tissue itself was examined, and gave a strong reaction, whilst all the other solid organs of the body and the contents of the stomach and intestine gave no reaction with the sugar tests.

Nothing, certainly, seems more conclusive than such an experiment as this. There is no sugar in the blood going to the liver, and there is an abundance in that escaping from it, as there is also in the tissue of the organ itself. Such evidence really appears in unmistakable language to indicate that sugar is formed by the liver, and carried away from it in the hepatic blood. And, from the extent to which the sugar is found, it might be reasonably inferred that its production is connected with some very important purpose in the animal economy. Now, the experiment establishes beyond all question that sugar can be produced in the animal system without being derived directly from the food; but the great point of interest in physiology, and likewise as regards the pathology of diabetes, is whether this production is really taking place during life, in accordance with the inference that has been drawn from Bernard's results obtained after death. Does, in fact, the examination, as it has been hitherto conducted, afford us an indication of the physiological state? or can it be only taken for what it is really worth—namely, as the representation of a post-mortem condition? This I shall specially touch upon when I have proceeded a little further, and brought the history of the subject up to the present time.

It was at first considered that the sugar found in the system of the animal feeder was formed from an albuminous material. Bernard's next step—and *an important* one it has proved—was to discover and isolate the substance *which is* the source of animal glucose. In September, 1855, he communi-

cated to the Academy of Sciences of Paris his discovery that the formation of sugar in the liver might be shown to be capable of taking place after death. On passing a stream of water through the vessels of the liver after removal, the whole of whatever sugar impregnated its tissue was washed out, so that it then gave no reaction with the copper or fermentation tests. Upon placing it aside, however, and examining it again some time afterwards, a strong indication of the presence of sugar was to be obtained. All question of a special vital process was by this experiment removed, and the production of sugar resolved itself into a simple chemical action. The source of the sugar was not as yet disclosed; but it was evident that in the liver it must be, and that it must consist of something not so easily taken up by water, otherwise it would have escaped from the organ with the sugar. In 1857 the isolation of the sugar-forming material was announced, and as sugar-formation was supposed to constitute its physiological destination, it was called by Bernard the Gluco-genic Matter of the liver. The gluco-genic theory was considered to remain unaltered by this discovery, except in so far as it was rendered more complete by the detection of a recognisable source for the animal glucose.

My researches have led me to look upon the term gluco-genic as objectionable, if applied to this newly-discovered substance upon physiological grounds. It is true that, after death, and under certain unnatural conditions, it is a sugar-forming substance; but under natural circumstances it does not seem intended for the production of sugar. On account of its chemical resemblance to a product of the vegetable kingdom, it has also been called the Amyloid Substance. To this term, however, an objection might be raised, on the score of confusing this substance with a body of a different nature met with in the system that has long since received the same name. In speaking of it myself, I have called it Hepatine, simply from its connexion with the liver. When our knowledge of its physiological relations is more perfect, it can then be named upon scientific principles; but until then its provisional name should certainly be one which is not exposed to the chance of leading to error by implying a purpose I have good reason to believe it is not naturally intended for.

The situation of hepatine in the liver is in the hepatic cells—at least, such is to be inferred, from what is seen by the use of the microscope, with the employment of chemicals. As it is not present after death from disease, but is always present under healthy circumstances, its production may be supposed to result from the exercise of the normal functional activity of the liver.

In its chemical properties, hepatine is allied to starch, but even more closely so to dextrine. When pure, it is a neutral, colourless, tasteless, and inodorous body, presenting only an amorphous granular appearance under examination with the microscope. It is largely soluble, although not with rapidity, in water; and the aqueous solution presents an opaquely lactescent character. It is curious that in the form of a highly-concentrated solution it is transparent, the lactescence appearing only on dilution. It is insoluble in alcohol and glacial acetic acid, by which agents it may be thrown down from its aqueous solution. It is unaffected at a boiling heat by caustic potash. It is devoid of nitrogen, and is composed of $C_{12}H_{12}O_{12}$. With iodine its behaviour is like dextrine, producing a deep wine-red coloration. Its most important property is its susceptibility of transformation into sugar. It does not itself react with the cupro-potassic or fermentation tests; but, after boiling for a short time with a mineral acid, or contact with many animal products, as saliva, blood, liver-tissue, &c., at a moderately-elevated temperature, the characteristic indications of glucose are discernible. For studying its relations, it is easily procured by plunging a piece of liver removed from a recently-killed animal into boiling water; then pounding in a mortar, and boiling afresh with a fairish quantity of water. The strained or filtered liquid is to be poured into five or six times its bulk of spirit to precipitate the hepatine, which, after being collected on a filter, is to be dried. To obtain it pure, it must be boiled in a solution of potash. After reprecipitation with spirit, it is to be thoroughly *washed with* that agent to effect the removal of foreig

matter. But traces of the alkali cling to it with great tenacity, and these can only be got rid of by neutralizing with an acid, such as the acetic. The acetate formed is carried away by the spirit in washing.

[It will be observed that Dr. Pavy does not in the least impeach the accuracy of Bernard's experiments, but simply affirms that the existence of a certain state after death, does not prove that this state existed during life. The position of the matter is this:]

Experiments have been performed, and results obtained, about the conclusive character of which there cannot be a difference of opinion. But these results have been derived from *post-mortem* examinations, and taken to represent the *ante-mortem* state. Now, it happens, as can be shown by a different mode of experimenting than that hitherto adopted, that the two do not correspond; and here is my ground of opposition to the gluco-genic theory. A physiological condition has been inferred from the state observed after death, and the inference proves to be fallacious.

The knowledge required is of the state belonging to life, and this we fail to obtain unless we operate on blood, cardiac as well as arterial, that is collected before, or at the very instant of death.

I was so strongly impressed with the notion that the gluco-genic doctrine was indisputably established, that I was for some time disposed to think there might be some fallacy in my experiment, rather than call in question the truth of our views. Seeking in vain for a source of fallacy, I afterwards found that what had been regarded as the natural state of the blood of the right side of the heart was not in reality such; that the blood collected, as had hitherto been done, after death, did not behave like the blood removed during life; and, therefore, that an inference of the condition of the one from the examination of the other must be discarded as erroneous.

When blood is collected from the right side of the heart, as in an ordinarily conducted examination after death, it yields a strong indication of the presence of sugar. In five quantitative analyses I found the proportion of sugar to vary from half a grain to nearly one grain per cent. in defibrinated blood. When collected, on the other hand, during life, under natural circumstances, the amount of sugar is certainly not more than is encountered in the arterial system.

It is not absolutely necessary to resort to the operation of catheterism of the ventricle to obtain a specimen of blood presenting the natural character belonging to life. With the information now in our possession, the experiment may be so conducted as to display the physiological state after death, and I do not know that this is not the most preferable mode of operating, on account of the result being less liable to be influenced by accidental disturbing circumstances. Life is destroyed by pithing, and instantly afterwards the chest is as rapidly as possible opened, the heart seized, and a ligature firmly applied around its base. The blood is then collected from the right ventricle; and if the operation have been expeditiously effected, it will be found as free from saccharine impregnation as if catheterism of the heart had been performed under a tranquil state during life. The object is to collect the contents of the ventricle before the blood has become contaminated with the sugar, which is produced in the liver with such astonishing rapidity after death. After the destruction of life by pithing, the circulation continues for a short time, whilst the process of respiration is instantly stopped. By this continuance of the circulation, the opportunity occurs for the impregnation of the blood with the sugar formed in the liver, as a post-mortem occurrence. Now, unless the steps of our experiment be quickly performed, the sugar will have reached the heart, and even have been carried through the lungs into the arterial system. There is nothing to be wondered at in this, seeing how quickly foreign agents introduced into the circulation have been found to be transported from one part of the system to another—seeing, as I shall hereafter point out, how quickly the saccharine principle is produced in the liver, as the result of the destruction of life.

As far, then, as we learn from what has preceded, there is not as a natural process of life, that flow of sugar into the circulation from the liver, for

purpose of destruction in the lungs, which the former mode of experimenting led physiologists to believe. After death, and under certain unnatural states during life, it is true, there is a large escape of sugar from the liver; but, as a normal condition, there is only a trace of sugar in the blood between the liver and the lungs, and this trace is also met with on the other side of the lungs, in the blood returning from the system at large, and even in the blood in its passage to the liver. The blood, therefore—namely, that returning from the liver—which was formerly looked upon as affording evidence of the exercise of a gluco-genic function by this organ, has nothing special belonging to it. The same character is met with, as far as I can discover, to a precisely equal extent, in the blood of every part of the system.

Although I had discovered the facts I have pointed out respecting the blood, yet I still continued to look upon the liver as strongly impregnated with sugar during life. Analysis of the organ after death displayed abundance of the saccharine principle, and I had nothing yet before me to lead me to suppose that this was not the true representation of the living state. At a loss, however, to account for the blood escaping from the liver not being charged with sugar during life as it is after death, I began to seek for a cause, and thus turned my attention to the liver itself.

The first idea that suggested itself was, to try the effect of imitating, by artificial means, the circulation under different degrees of tension. For this purpose, defibrinated bullock's blood was injected through the liver at different pressures. Nothing satisfactory, however, was elicited by these experiments.

It now occurred to me to examine closely the chemical relations of hepatine, as far as regarded its transformation into sugar. But, in the first place, knowing that hepatine was convertible into sugar by a kind of fermentation, a foreign substance—a strong solution of sulphate of soda—was injected into the liver instantly after death, to see if it influenced this fermentation, and altered the amount of saccharine matter found. The result obtained induced me to proceed, and led me to consider it possible that the sugar hitherto found in the liver might be in reality due to a post-mortem occurrence, just as in the case of the blood.

For, looking to the rapidity with which saliva acted in converting hepatine into sugar, I could perceive it to be possible that a sufficient transformation *might* be effected, during the interval that elapsed between the period of death and the exposure of the liver to the requisite degree of heat to coagulate it and prevent further change, to account for the sugar found in the tissue of the organ by the mode of examination that had hitherto been adopted. Experiments were now conducted upon a solution of hepatine, making use of saliva as a ferment. Various agents were tried in order to see if anything could be discovered to check the saccharine metamorphosis without destroying either of the principal bodies concerned. The alkalies were found to possess this property; a very small quantity of potash, added to a solution of hepatine, sufficed to prevent the operation of the transformative action of saliva. Upon this fact, I was led to perform the experiment of injecting the liver with potash. The alkali having prevented the action of saliva as a ferment, I thought it would exercise a similar effect upon other ferments, and that by injecting a strong solution of it through the portal vein into the liver as expeditiously after pithing as practicable, I should meet with an organ, as far as hepatine and sugar were concerned in the condition belonging to life,—or, at least, much nearer to life than had hitherto been done—and thus decide the question that was agitating my mind.

Upon this injection of potash being performed, it was in vain I sought for the accustomed behaviour of the liver. I could now satisfactorily account for the difference that had been observed in the state of the blood before and after death. But the fact required collateral proof, as it led to so important a conclusion. That the potash had not destroyed the sugar, but simply prevented its formation, was shown by practising the injection upon a liver that had been allowed to *remain a short* time after death to permit the production

of sugar to take place. The result, under these circumstances, was, that the presence of sugar was as readily discoverable as if the injection of the alkali had not been employed. Another modification of the experiment adopted was, to operate on the liver instantly after death, and to practise the injection on a portion only of it. On analyzing the two specimens thus obtained, the one gave no reaction with the copper solution whilst the other occasioned a strong reaction indicative of sugar.

Acids act in a similar manner to alkalies in checking the transformation of hepatine by ferments. I have employed a strong solution of citric acid for injection into the liver, and have found it occasion an absence of sugar.

There is yet another and a far more preferable process for enabling us to alight upon the natural condition of the liver belonging to life. Conclusions derived from the employment of chemical agents such as acids and alkalies might be regarded with suspicion by some, but the effect produced by the simple influence of a variation of temperature cannot, I think, be open to any source of objection. Now, it happens that either of the two extremes of temperature will answer our purpose. Freezing suspends organic chemical changes of every description, and boiling destroys the ferment without exercising any possible influence upon the hepatine or sugar.

For the application of the freezing method, a mixture of ice and salt is employed. The mixture must be allowed to liquefy before being used, its power of rapidly extracting heat being then much more perfect. The object is to instantly reduce the temperature to a degree at which the ferment ceases to possess activity. The life of the animal being suddenly destroyed, the abdomen is immediately opened, and a piece of the liver excised as hastily as possible, and plunged into the freezing mixture, in which it is afterwards moved about. In the course of a very short time the liver is frozen quite hard. Being removed, it is cut into thin slices, reduced to a pulp in a mortar, and thrown a little at a time into a small quantity of water contained in a capsule, which is to be kept thoroughly boiling during the process. If the specimen were allowed to pass through a gradual elevation of temperature to obtain the decoction for testing, the experiment would be vitiated, as sugar would be formed during the process of preparation. The liquid procured is a concentrated decoction of the liver. It contains plenty of hepatine, but gives no indication, or, at the very most, the merest trace of indication of sugar. With several specimens, where the experiment has been happily conducted throughout, I have not been able to discover the slightest reaction of sugar; whilst the remainder of the liver, which had not been submitted to the influence of cold, but tested in the manner it was formerly the custom to do, gave a copious reaction with the copper solution.

To prove that merely a suspension of change is effected by the freezing, it is only necessary to expose the frozen liver for a short time to a temperature of about 90° or 100°. When prepared for testing, the most abundant evidence of the existence of sugar is to be obtained.

I need scarcely say, that a liver which has been allowed to become saccharine after death loses none of its saccharine quality by immersion in a freezing mixture.

I have conducted several experiments, both during digestion and at a moderate period afterwards, but have failed to discover any difference in the result. I usually operate on the rabbit, because I find, from experience, that the steps of the experiment can be more rapidly carried out; and the liver being thinner, the portion excised is sooner acted upon throughout by the cold. With a thick piece of liver, I have before now noticed, that whilst the outside portion was devoid of sugar, the central part contained it in moderate quantity. It is obvious that, with a thick mass of liver substance, the centre must escape being influenced for a greater or less period. Indeed, I have seen it, after some minutes' immersion, still quite soft.

The application of heat, although acting in a different way, leads to the same result as the application of cold. Instantly after occasioning death, in *as speedy a* manner as possible, a piece of the liver is removed, and plunged *into about* a quart of water that has been placed over a flame or the fire, and

is in a state of active ebullition. The piece of liver should not be too large or too thick, and I usually make a few incisions into it, so that it may be more rapidly acted on throughout. The boiling water coagulates, and thus destroys the ferment; and after a minute or two's immersion, all that is necessary for testing is, to pound the liver in a mortar, to add a little water, and then make an ordinary decoction of it. This process is perhaps not quite so efficient as that of freezing. It will be easily understood that every degree of elevation of temperature, until the point is attained at which the transformative agent loses its activity, is more and more favourable to the production of sugar; so that, unless the arrival at the requisite degree of heat be almost sudden, the object we have in view escapes being attained. On following, however, the directions I have given, specimens are easily obtained, yielding only a barely recognizable indication of the presence of sugar.

In these experiments upon the influence of temperature, as in those with the injection of an acid or alkali, everything depends upon the rapidity with which the required condition of the liver is produced after the destruction of life has been effected. The transformation of hepatine into sugar takes place so immediately after death, in the liver of the warm-blooded animal, that unless the greatest promptitude is exercised an unsuccessful result is sure to be obtained.

Amongst cold-blooded animals the operation is easy enough for procuring a representation of the natural condition of the liver. According to the lowness of the temperature, the post-mortem change is proportionately retarded. Hence, with ordinary precautions, especially during cold or moderately cool weather, the liver of a cold-blooded animal is readily alighted upon in a state that belongs to life.

The frog is an exceedingly convenient and suitable animal for experimenting on, for this, as for so many other purposes. When it is in good condition, the liver is large, pale-coloured, and abounds in hepatine. For an examination four or six livers are sufficient. The animals are killed, and the livers excised and plunged into boiling water. A concentrated liver decoction is then made, which is opaquely lactescent from the quantity of hepatine it contains. Boiled with the copper solution, it gives no reaction of sugar. If the livers, however, are kept for some time, or exposed for a short period to moderate warmth before preparation for testing, then a strong reaction of sugar is obtained. Also, if the experiment is conducted when, from an artificial cause or otherwise, the temperature of the animal is at a rather elevated degree, a small amount of sugar will be found, unless the steps of the operation have been expeditiously performed, in order to prevent any post-mortem transformation occurring.

The influence of temperature on the liver of a frog has not escaped the observation of Bernard. My own knowledge was obtained from independent observation; but in 1857, Bernard, in a communication to the Academy of Sciences, mentioned that lowering the temperature of frogs caused a disappearance of sugar from the liver, whilst a reappearance took place on afterwards exposing them to heat. He added that it was possible to produce this singular alternation of appearance and disappearance of sugar several times without any food being given, and by acting solely on the phenomena of the circulation through the medium of the temperature. Such was his explanation of this phenomenon—namely, to refer it to an alteration in the activity of the gluco-genic function, brought about by increasing or diminishing the activity of the circulation. My own explanation has to be inferred from what has preceded. It depends simply on the influence which a high or a low temperature exercises over the rapidity of the post-mortem production of sugar.

The oyster and the mussel are animals which likewise serve well to illustrate the true state of the liver in regard to sugar formation. In both the liver is charged with a very large quantity of hepatine, but is entirely devoid of sugar when the animal is taken for examination in a fresh and healthy condition. Keep it and allow it to die, and sugar is immediately discover-

able in abundance. Also, on exposing the healthy ex-saccharine liver to an elevated temperature for a few minutes, sugar is produced in large quantity

In the mussel (*Mytilus edulis*), besides hepatine being contained in the liver, the mantle is exceedingly rich in an identical material under a good conditioned state of the animal. When the animal is poor, as after spawning, the mantle is thin, transparent, and watery; but at other times it is thick and opaquely white or yellowish, and, in this state, is highly charged with the amyloid substance, which thus seems to form a store to be drawn upon when required. No sugar can be detected in the mantle at the time of death: and, what is more, there is scarcely a proneness to the production of sugar after death, unless a ferment, such as saliva, be added, when a copious formation of sugar is the result.

There are means for placing a warm-blooded animal in the same position as our frogs in the experiments to which I just now referred. After division of the spinal cord, as high up towards the brain as is consistent with the persistence of life—that is, just below the origin of the phrenic nerves, the temperature of the body rapidly falls if the animal be placed in a cool situation. Now, Bernard noticed that under these circumstances the liver was found devoid of sugar when examined immediately after death, but became strongly saccharine afterwards. He endeavoured to account for this phenomenon under his gluco-genic theory; but the different reasons assigned show the difficulty he was under in giving a satisfactory explanation of it. The fact itself I have often verified, and the correct interpretation of it, I conceive, consists in the lowness of temperature at the time of death diminishing the rapidity of the post-mortem formation of sugar, and so producing the condition noticeable in the cold-blooded animal. Maintaining the temperature of the animal after division of the cord, by immersion in a heated atmosphere, causes the liver to behave just as if no division of the cord had been effected. Circumstances now exist, as in the warm-blooded animal under an ordinary condition, conducive to the almost instantaneous production of sugar after death.

Again, on oiling the coat of an animal so as to favour the rapid escape of heat from the body, the temperature by exposure to cold may be made to fall until it becomes too low for the persistence of life. By this process, the same results are obtained on examining the liver as when the temperature has been reduced by division of the cord.

Facts such as these I have now mentioned show that the saccharine state is not natural to the liver, as has hitherto been supposed in accordance with the doctrine of gluco-genesis. But there is another aspect concerning the liver which remains to be considered. Hepatine is formed by the liver from sugar. Now, that hepatine should be intended to come back again into sugar, certainly seems, to say the least, improbable. The fact taken alone does not of course actually prove anything against the gluco-genic theory: but such a process as the conversion of sugar into hepatine, and hepatine back again into sugar, certainly does not accord with what we should expect from the notion we possess of the manner in which the operations of nature are conducted.

In eleven examples, taken indiscriminately, the average relative weight of the liver in a dog, fed upon a purely animal diet was as 1 to 30. The amount of hepatine given as the average of my analytical examination of the livers in these cases was slightly over 7 per cent.

Under a vegetable diet the five dogs taken gave me the proportion of 1 to 15 as the relative weight of the liver. The organ was thus exactly double the size of that of the animal feeder. Three of the livers were analyzed quantitatively, and the average amount of hepatine given was over 17 per cent.

In four examples where animal food, with a large admixture of cane-sugar, was given, the average relative weight of liver was as 1 to 16½. The analyses in four cases yielded 14·5 per cent. as the average amount of hepatine present.

The experiments made on rabbits have most fully confirmed these results obtained upon a dog.

In one experiment a couple of full-grown animals were taken which as closely as possible resembled each other. One was kept fasting, whilst to the other was administered an ounce of starch, with three-quarters of an ounce of grape-sugar per diem. On the fourth day both rabbits were killed, and the following is the result of the examination made:—

| | Weight of Animal. | Weight of Liver. | Amount of Hepatine in the Liver. |
|---|---|---|---|
| Rabbit fasting .... | 3 lb. 1 oz. .... | 1, say $\frac{7}{8}$ oz. .... | 1·3 per cent. |
| Rabbit fed on starch and grape-sugar .... | 3 lb. 4 oz. .... | $2\frac{1}{8}$ oz. .... | 15·4 " |

In a second experiment two half-grown rabbits were taken. One, again, was made to fast, whilst to the other was given an ounce of starch with a similar quantity of cane-sugar daily. On the fourth day the animals were killed, and an examination made. The following is the result:—

| | Weight of Animal. | Weight of Liver. | Amount of Hepatine in the Liver. |
|---|---|---|---|
| Rabbit fasting .... | 1 lb. 14 oz. .... | 1 oz. .... | 1·4 per cent. |
| Rabbit fed on starch and cane-sugar .... | 1 lb. 14$\frac{3}{4}$ oz. .... | 2$\frac{3}{4}$ oz. .... | 16·9 " |

In another experiment a rabbit was allowed to take its ordinary food, and, in addition, three-quarters of an ounce of loaf-sugar and half an ounce of starch were administered daily. The rabbit was killed on the fourth day, and the hepatine amounted to 22·7 per cent., notwithstanding the analysis of the liver was not made until the day after death, when more or less loss must have taken place from a transformation into sugar going on.

All these results are so striking and so corroborative of each other, that it seems to me nothing further can be required to prove that the sugars and starch are made use of by the liver for the production of hepatine. The marked variation observed in the size of the liver under different diets is a point of much physiological interest, and possibly may prove of pathological importance also.

[Dr. Pavy is convinced that sooner or later the present gluco-genic theory must be abandoned. The theory is founded upon the condition of the liver and blood, discoverable by an ordinary examination after death. But the result of such an examination is not in accordance with the true ante-mortem state.]

Briefly to consider them, these are the grounds that have led me to deprive the liver of the special sugar-secreting function which has lately been attributed to it. The organ itself is found free from sugar, or at the most is only impregnated with it to the minutest extent, when treated so as to come down upon it in a condition the nearest possible to that of life. The blood, under normal circumstances, merely contains more or less evident traces of sugar; and the blood in different parts of the system does not present, as far as I can discover after adopting the necessary precautions in the examination, any appreciable variation in constitution. In some recent most carefully conducted examinations, I have been unable to distinguish, by any difference in behaviour, the blood of the portal vein from that of the right side of the heart. I have sought for but been unable to obtain, evidence of the susceptibility of sugar to undergo destruction, to any extent, in the circulation. The result of my experience decidedly is, that if sugar escape from the liver, it is to be detected alike in every part of the circulation; and there is never any extent of sugar in the blood without its being immediately drawn upon and separated by the kidneys. This being the case, the object of sugar formation becomes a most fruitless one; and it is not, I think, to be conceded as probable that the liver should produce it for the kidneys afterwards to get rid of it.

It is possible that, without being susceptible of appreciation by our tests, there may be a slight amount of transformation of sugar into lactic acid

going forward in the blood as it is circulating through the different parts of the system; for if saccharine blood be allowed to decompose, the sugar, when decomposition has fully set in, gradually disappears, and the blood becomes acid, which is the reverse of what takes place when a specimen has been taken that is free from sugar. The changes of decomposition, I apprehend, have excited the lactic acid fermentation precisely as in the case of milk that has turned sour. Now, I see no reason, although I can bring forward no actual proof to support it, why the changes going on in the blood during its circuit through the body may not act in the same way as the changes of decomposition, and occasion a slow production of lactic acid from the traces of sugar that I have mentioned as being found in every part of the circulation. Whatever, however, may occur of this kind, must be of an exceedingly trifling nature, for the disappearance of sugar is not to be detected in any particular portion of the system by a difference in behaviour of the blood.

To proceed with the arguments that oppose the gluco-genic theory: We have evidence that the so-called gluco-genic matter is actually formed in the liver from sugar as one of its sources. Now, if sugar were its natural destination, the process would be sugar into glycogene, and glycogene back again into sugar—a repetition that certainly appears upon the face of it to be extraordinary and improbable as a designed occurrence of nature.

Again, if there were the constant production and flow of sugar from the liver through the hepatic veins, as is implied under the gluco-genic doctrine, on interrupting the principal stream of blood through the organ, it ought certainly to be expected that less sugar should escape, and less sugar be found in the circulatory system. Yet, as the result of actual experiment, precisely the reverse is the case. A ligature applied to the portal vein causes the blood circulating through the system to present a strongly saccharine character. Notwithstanding the current is so reduced which is supposed to be naturally carrying sugar into the general circulation as a part of a particular functional operation, the amount of sugar now reaching the circulating current is actually very greatly increased. The fact appears to me to be, that the ligature of the portal vein places the liver in an unnatural condition; its functional working does not proceed, with a supply only of arterial blood, in the manner it does when there is also the stream of portal blood to exercise its influence. As the result of this, the hepatine descends into sugar, like it does under many other unnatural states of the liver.

Ligature of the hepatic artery as well as the portal vein causes the liver to become strongly impregnated with sugar during life, as is the case with ligature of the portal vein only; but the blood belonging to the general circulation remains uninfluenced. The flow of blood through the liver being entirely prevented, no opportunity is afforded for the passage of the sugar into the system.

If I have concluded, as my experiments have led me to do, that sugar is not the natural product—that sugar is not specially intended to be formed as the result of the natural transformation of hepatine in the liver, it may reasonably be inquired of me, what is the material, into which the hepatine is really destined to be transformed? It is generally more easy to say what the nature of a particular operation does not, than to pronounce what it does consist of, when our knowledge regarding it has not been completely made out. I do not wish, however, to avoid this question; indeed, I have it constantly before me. But, as I am not in a position to give what I consider a sufficiently satisfactory solution at present, I must content myself by saying, that I have grounds for believing that the product of the normal transformation of hepatine, instead of escaping through the hepatic veins into the circulation, passes down through the biliary passages into the intestine. I am actively engaged in working out the connexion of the liver with the production of fat—a statement that may serve to give a hint of the direction in which my views are tending with regard to the object of hepatine.

*In some* other parts of the body besides the liver, there is to be found a *material* which has characters identical with hepatine. Now, if hepatine

give rise to sugar in one situation, why should it not do so in others; and why should there not be in the muscles and the cells of the cutaneous, intestinal, respiratory, and genito-urinary surfaces of the fœtus, and in the muscles and lungs of the adult, a gluco-genic function?

It is not necessary that the liver should enjoy a special sugar-forming function to account for the presence of that minute trace of sugar which is met with under natural circumstances in the circulation. The production of sugar may result from the operation of a chemical tendency which hepatine happens to possess, and which appears to have no connexion with its physiological destination. When chemistry alone comes into play, as after death, the tendency of hepatine to undergo a saccharine metamorphosis is allowed to take effect; but normally, during life, this mode of transformation is held in check by a force or condition peculiar to the living and healthy liver. There is the material which is so susceptible of undergoing change into sugar, and there is, also, the ferment or agent for exciting it. Both are present in the liver, and yet the process escapes being carried out, except to the most infinitesimal extent. Although I cannot precisely account for this, nevertheless the fact remains the same; and we are not altogether unprepared for it, as we have long been familiar with a parallel in the phenomenon of the coagulation of the blood. Whilst circulating in the living body, the fibrine exists in a fluid state, but with an astonishing rapidity assumes the solid form after the blood is withdrawn. It is the property of fibrine to have a strong tendency to coagulate. As long as it remains under natural circumstances in the living body, this tendency is held in check, but immediately it escapes from the influences thus naturally controlling it, its coagulating property comes into play, and a solidification is the result. Even here, although the phenomenon has been so long familiar to us, it has not yet received what can be considered as a satisfactory and sufficient explanation. There is, then, nothing supernaturally wonderful, nothing unparalleled, in the position of hepatine before and after death—namely, as to its resisting during life the exercise of its chemical tendency to transform into sugar, and a transformation taking place immediately the controlling influence of life is removed.

I imagine that under the existence of natural circumstances the circulation takes place through the liver without disturbing the hepatine contained in the hepatic cells further than just the minutest trace of it passing into the bloodvessels, and thus accounting for the trace of sugar constantly met with in the different parts of the circulatory system. Hepatine cannot be mixed with the blood without being transformed into sugar. The blood, whether circulating in the body, or experimented with out of the body, acts like saliva as an energetic ferment in converting hepatine into sugar. The injection of hepatine into the circulation of the living animal occasions a saccharine state of the blood, and if enough have been employed, the urine is also impregnated with sugar. This is shown by the specimen of urine before me, derived from a rabbit, in which fifty grains of hepatine were introduced into the jugular vein. It gives the characteristic orange-yellow reaction with the copper solution. Hence it is only necessary for hepatine to pass from the liver cells into the blood, and a saccharine state of this fluid is the immediate result.

The consideration has not been overlooked, that although the quantity of sugar found in the blood of the right side of the heart is extremely small, yet, supposing this small quantity to be constantly escaping from the liver, it would form a considerable amount in the course of the twelve or twenty-four hours. The sugar, however, small in quantity as it is in right-ventricular blood, has not just been poured into the circulation from the liver. It must be remembered that the blood flowing towards the heart through the general systemic veins, contains sugar, and as far as I can learn from the results of our tests, to about the same extent as the cardiac blood itself. So that sugar once in the circulation circulates over and over again, and it would be an error to calculate, *from the amount* found in the heart, and the *quantity of blood passing through the liver*, the extent of sugar production

going forward in the blood as it is circulating through the different parts of the system; for if saccharine blood be allowed to decompose, the sugar, when decomposition has fully set in, gradually disappears, and the blood becomes acid, which is the reverse of what takes place when a specimen has been taken that is free from sugar. The changes of decomposition, I apprehend, have excited the lactic acid fermentation precisely as in the case of milk that has turned sour. Now, I see no reason, although I can bring forward no actual proof to support it, why the changes going on in the blood during its circuit through the body may not act in the same way as the changes of decomposition, and occasion a slow production of lactic acid from the traces of sugar that I have mentioned as being found in every part of the circulation. Whatever, however, may occur of this kind, must be of an exceedingly trifling nature, for the disappearance of sugar is not to be detected in any particular portion of the system by a difference in behaviour of the blood.

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*In some* other parts of the body besides the liver, there is to be found a *material* which has characters identical with hepatine. Now, if hepatine

give rise to sugar in one situation, why should it not do so in others; and why should there not be in the muscles and the cells of the cutaneous, intestinal, respiratory, and genito-urinary surfaces of the fœtus, and in the muscles and lungs of the adult, a gluco-genic function?

It is not necessary that the liver should enjoy a special sugar-forming function to account for the presence of that minute trace of sugar which is met with under natural circumstances in the circulation. The production of sugar may result from the operation of a chemical tendency which hepatine happens to possess, and which appears to have no connexion with its physiological destination. When chemistry alone comes into play, as after death, the tendency of hepatine to undergo a saccharine metamorphosis is allowed to take effect; but normally, during life, this mode of transformation is held in check by a force or condition peculiar to the living and healthy liver. There is the material which is so susceptible of undergoing change into sugar, and there is, also, the ferment or agent for exciting it. Both are present in the liver, and yet the process escapes being carried out, except to the most infinitesimal extent. Although I cannot precisely account for this, nevertheless the fact remains the same; and we are not altogether unprepared for it, as we have long been familiar with a parallel in the phenomenon of the coagulation of the blood. Whilst circulating in the living body, the fibrine exists in a fluid state, but with an astonishing rapidity assumes the solid form after the blood is withdrawn. It is the property of fibrine to have a strong tendency to coagulate. As long as it remains under natural circumstances in the living body, this tendency is held in check, but immediately it escapes from the influences thus naturally controlling it, its coagulating property comes into play, and a solidification is the result. Even here, although the phenomenon has been so long familiar to us, it has not yet received what can be considered as a satisfactory and sufficient explanation. There is, then, nothing supernaturally wonderful, nothing unparalleled, in the position of hepatine before and after death—namely, as to its resisting during life the exercise of its chemical tendency to transform into sugar, and a transformation taking place immediately the controlling influence of life is removed.

I imagine that under the existence of natural circumstances the circulation takes place through the liver without disturbing the hepatine contained in the hepatic cells further than just the minutest trace of it passing into the bloodvessels, and thus accounting for the trace of sugar constantly met with in the different parts of the circulatory system. Hepatine cannot be mixed with the blood without being transformed into sugar. The blood, whether circulating in the body, or experimented with out of the body, acts like saliva as an energetic ferment in converting hepatine into sugar. The injection of hepatine into the circulation of the living animal occasions a saccharine state of the blood, and if enough have been employed, the urine is also impregnated with sugar. This is shown by the specimen of urine before me, derived from a rabbit, in which fifty grains of hepatine were introduced into the jugular vein. It gives the characteristic orange-yellow reaction with the copper solution. Hence it is only necessary for hepatine to pass from the liver cells into the blood, and a saccharine state of this fluid is the immediate result.

The consideration has not been overlooked, that although the quantity of sugar found in the blood of the right side of the heart is extremely small, yet, supposing this small quantity to be constantly escaping from the liver, it would form a considerable amount in the course of the twelve or twenty-four hours. The sugar, however, small in quantity as it is in right-ventricular blood, has not just been poured into the circulation from the liver. It must be remembered that the blood flowing towards the heart through the general systemic veins, contains sugar, and as far as I can learn from the reaction of our tests, to about the same extent as the cardiac blood itself. So that sugar once in the circulation circulates over and over again, and it would be an error to calculate, from the amount found in the heart, and the quantity of blood passing through the liver, the extent of sugar production

in a given time. Even, as I have stated, the blood before entering the liver is charged with sugar, and in experiments, where the greatest care has been exercised, where every precaution has been taken to obtain the specimens in the state naturally belonging to life, there has been no appreciable difference of reaction discoverable between the portal and the cardiac blood.

It is interesting to notice how hepatine, in the feeble property of diffusibility it enjoys, presents us with an exemplification of the harmonious adaptation that exists in the exercise of the operations of nature. Looking at the hepatine as contained in the hepatic cells, and reflecting on the close proximity that the blood must hold to it in circulating through the liver, it may be reasonably inquired how it is that the hepatine does not pass, as it might be expected to do, by diffusion into the bloodvessels—a circumstance that would conduce to render the blood extensively saccharine. Now, it happens, as I have discovered, as can be seen by the experiment before me, that hepatine is the antithesis to sugar as regards extent of diffusibility. If sugar were contained in the liver cells, the circulation could not take place through the organ without largely carrying it away, on account of its being so readily diffusible through membranous septa. With hepatine, on the other hand, there is a resistance to traverse animal membranes. A solution of sugar and hepatine placed together on one side of an animal membrane in an endosmotic apparatus, the sugar diffuses, leaving the hepatine behind. I have even found that under the pressure of a six-feet column of fluid, hepatine has resisted passage through the pores of a piece of bladder. It is on account of this low diffusibility, I presume, that we experience the difficulty we do in filtering a solution of hepatine. The coarse pores of an ordinary filter seem, indeed, to offer some amount of obstruction to the passage of this principle. That hepatine *can* pass through a membranous septum is, I think, sufficiently proved by the fact, that in injecting a stream of water through the vessels of the liver, the hepatine is gradually washed out. Under this operation the liver become sunusually swollen and œdematous, and its cells must be submitted to a great amount of pressure.

Albumen is another material which, like hepatine, enjoys a low diffusibility. When a solution of it is submitted to experiment, it does not pass through a membrane, such as a piece of bladder, except under the influence of pressure. With a column of fluid, three feet high, the albumen passes, but not at all when there is no pressure employed. This property of albumen is doubtless connected with its retention in the bloodvessels. It is not intended that a material so necessary as a pabulum for the nutrition of the body, should have a tendency, like urea and sugar, to escape from the blood by virtue of the simple physical property of diffusibility. For a corresponding reason, I apprehend, hepatine agrees with albumen, and is not endowed with a tendency to diffuse, because it is not intended to be drawn by the blood out of the hepatic cells.

Looking, then, to this physical character which hepatine possesses, I think it must be admitted that the explanation is clear, why, under natural circumstances, it resists, as it does, transudation into the circulating current, where being transformed into sugar, it would prove, if not entirely, certainly in great part, useless to the system; for, as I have before pointed out, sugar cannot exist to any notable extent in the blood without being recognisably eliminated by the kidneys. I should say even, that the trace of sugar which is natural to the blood throughout the body, is constantly being drawn upon by the kidney; but that the amount for removal is so small, that it is not susceptible of detection in the urine. Did we possess more perfect means of detection than we do, I imagine a minute trace of sugar would be reckoned as a normal constituent of the urine. I am speaking now a little problematically, but what I have said accords with a principle that is being forcibly recognised in analytical chemistry—namely, that the minuteness of diffusion of materials through the different constituents of the universe exceeds or surpasses our powers of detection.

*That* hepatine can, under certain circumstances, pass from the liver into *the bloodvessels*, has been proved by what I mentioned to be the effect of

injecting a stream of water through the organ after death. Now, may it not be that we have here an illustration of the manner in which saccharine urine is occasioned in some instances? Violent muscular efforts, by which the liver is compressed—and obstruction to the breathing, by which the circulation is interfered with and congestion produced—will occasion a highly saccharine state of the blood. In the case of the latter, by keeping up the respiratory disturbance I have rendered the urine strongly saccharine. Both disturbances, as far as the liver is concerned, are of a physical character, and is it not purely to physical circumstances that the unnatural condition observed is attributable? When the liver is compressed, a favorable condition is established for promoting the escape of hepatine; and when it is congested, the hepatic cells being, I conceive, submitted to compression, the condition will, in a similar manner, be favourable to a transudation of hepatine, and its admixture with the blood. In the human subject, saccharine urine has been noticed in cases attended with an obstruction to the flow of blood towards the heart. For example, it has been observed in whooping-cough, coma, and pneumonia; and I look upon it here as entirely referable to a physical alteration of the conditions in the liver. In whooping-cough the protracted paroxysms of coughing not only occasion a great amount of venous congestion, as is indicated by the countenance of the patient, but the liver must, at the same time, be compressed externally by the violent action of the abdominal muscles. In coma, with stertorous breathing, the impediment to the circulation of blood through the lungs produces general congestion of the system, and in this the vessels of the liver must participate. A case of this kind only recently fell under my notice. A man, one of the carpenters of our hospital, suddenly fell down in the street in a fit of apoplexy. As there was great lividity of the face, with slow, stertorous breathing, I suggested, about eighteen hours after the fit had occurred, that the urine should be tested for sugar. It was highly albuminous, and gave, after the albumen had been coagulated and separated, a copious reaction with the copper solution. There was no evidence that this person had saccharine urine previous to the occurrence of his apoplexy. In pneumonia, where a large portion of the lung is rapidly involved, the patient is likely to suffer from a great dyspnœa; and thus, through the circulation being influenced, saccharine urine may be occasioned at the commencement of the disease.

After division of the pneumogastrics in the lower animals, I have often observed the production of saccharine urine. It depends upon the activity of the respiratory function being so diminished as to offer an impediment to the natural flow of blood through the system; and it is only where this is prominently the case that sugar is to be recognised in the urine.

The theory founded on the doctrine of gluco-genesis referred diabetes either to a production of sugar in excess of its capacity for destruction, or, the production remaining the same, to an interference with its normal extent of disappearance. The production of sugar specially for subsequent destruction in the system must, if what I have brought forward prove substantial and true, be now left out of sight. There ought to be no sugar of any consideration in the blood, and, should there be any, it may be considered as resulting from some unnatural circumstance leading to its production. I have pointed out how I consider that a physical disturbance of the circulation may occasion a flow of hepatine into the blood, and so lead to the presence of sugar in the urine. The same occurrence may, I think, arise from an alteration of the natural forces in operation in the liver. Suppose, for instance, that the chemical tendency of hepatine is allowed to come into play, as is the case after death, then sugar will be formed in the liver tissue, and diffuse itself into the contents of the bloodvessels; it is only necessary for the circulation to be kept up, and a saccharine quality of the urine will be produced. On destroying, for example, the life of an animal, and adapting a contrivance for maintaining artificial respiration in a regular and efficient manner—which I effect in my laboratory by means of a double pair of bellows connected with the trachea, and so arranged that in working, one pair forces the air into the lungs, whilst the other afterwards draws it

out—the action of the heart continues, and the sugar which is formed as the result of a post mortem occurrence in the liver, is distributed by the circulation throughout the body, and being thus carried to the kidneys, is in part eliminated with the urine. One hour I have found to be quite sufficient to give to the urine a highly saccharine character in this way.

The method employed for destroying life in obtaining the result I have just mentioned has been by pithing the medulla oblongata. As far as my observation has gone, it does not signify what particular part—upper, lower, or middle—of the medulla oblongata forms the seat of injury or division, the result in each case is the same. But the experiment has been also attended with a similar production of saccharine urine when life has been removed by poisoning with agents capable of acting rapidly through the cerebro-spinal system. Bernard has stated, and my own experience confirms the accuracy of the statement, that after killing with the woorali poison, if the circulation is kept up by the performance of artificial respiration, the urine is rendered strongly saccharine. I have also found that in substituting strychnine for the woorali, the effect is perfectly identical.

Although division of the medulla oblongata leads to the production of saccharine urine when the circulation is kept up artificially, yet such effect is not observed after division of the spinal cord. I have repeatedly divided the spinal cord, and without producing diabetes. I have gone as high as between the second and third cervical vertebræ, after which the employment of artificial respiration is required to maintain the circulation, as after division of the medulla oblongata.

Again, the brain does not seem to exercise the same influence over the state of the liver as the medulla oblongata. It is true, I am not enabled to speak in quite such positive terms with respect to this organ; but my experiments induce me to believe that its functions may be destroyed without placing the liver in the condition noticeable after actual death, or after division of the medulla oblongata. In operating on the brain, circumstances may arise to complicate the result. Where, however, there has been the least complication, I have observed that the brain may be completely separated from the medulla oblongata without occasioning a saccharine state of the urine. In the last experiment I performed in reference to this point, the effects were as simple and the result as conclusive as any I could hope to obtain. The crura cerebri were divided just in front of the pons Varolii, so that the cerebrum was completely separated from the other parts of the encephalon. The breathing continued without assistance. It was quick and short, and frequently attended with a sigh. The heart's action was extremely rapid. The urine remained devoid of sugar.

The natural inference to be drawn from what I have been stating is, that the cerebro-spinal system gives to the liver during life a force or a condition which it does not possess after death, and that the part of the cerebro-spinal system particularly connected with this object is the medulla oblongata. In Bernard's celebrated experiment of puncturing the fourth ventricle for producing diabetes, the effect was supposed to depend upon a stimulation of the nervous centre. I cannot now coincide with this. My opinion is that the diabetic effect following the puncture of the medulla oblongata is produced in the same way as that following the destruction or division of the same part. I regard it as due to a loss of the proper nervous influence. In the one case the lesion is not sufficient to check also the respiration; in the other it is, and here the breathing has to be maintained artificially for the effect on the urine to become manifest.

Looking upon the medulla oblongata, then, as a centre, either directly giving to the liver a force capable of preventing the saccharine metamorphosis of its hepatine, or possibly indirectly maintaining through its influence on other organs such a state of the portal blood as is conducive to the fulfilment of this end, I determined to look for the channel through which its action might be exercised. It was working with this view before me which *brought* me to discover that injuring certain parts of the sympathetic system *most rapidly* occasioned a strongly saccharine state of the urine.

Supposing a force to start from the medulla oblongata for the object I have mentioned, I premised that its transit to the liver could not be, at least, exclusively through either the spinal cord or the pneumogastrics, as division of each had been frequently practised without any diabetic effect being produced. But the result of dividing both cord and pneumogastrics had not, as far as I was aware, been ascertained. I therefore started with the performance of this operation, which I found, however, was unattended with the appearance of sugar in the urine. In the experiment that was performed, the division of the cord was effected between the third and fourth cervical vertebræ, so that artificial respiration had to be resorted to.

Although no diabetic effect was observed after division of the cord and pneumogastrics together, yet, after division of everything belonging to the nervous system in the neck, as by decapitation, the urine became in a very short time strongly saccharine, the circulation being kept up by the performance of artificial respiration.

The result in these experiments tended to confirm the notion I had commenced with. Complete separation of the medulla oblongata from any possible connexion with the liver produced a saccharine state of the system, whilst division of the cord and pneumogastrics did not. But might not the sympathetic which also runs through the neck, form the medium of communication sought for? This was the next question that naturally suggested itself, and had to be answered by experiment.

Division of the carotid sympathetic—that portion of the sympathetic system which passes down from the superior cervical ganglion in front of the vertebral column towards the chest—is an old experiment, and one that is often repeated by physiologists on account of the interest it has in connexion with the production of heat. It does not, however, occasion any perceptible effect on the urine. But there is yet another portion of the sympathetic system running through the neck, and this, previous to the prosecution of the present inquiry, has, so far as I know, enjoyed immunity from attack by the scalpel of the experimentalist. This, however, consisting of the filaments accompanying the vertebral artery, might constitute the channel I was searching for. I therefore determined to effect a division of it, and my first experiment resulted in rapidly occasioning a highly saccharine state of the urine. Thus consisted the origin of my experiments on the sympathetic in connexion with the production of artificial diabetes. Such was also the process of reasoning that led me to this point. The evidence afforded by certain experiments had induced me to believe that the medulla oblongata in some way presided over the functional activity of the liver, so as to overcome the tendency to the production of sugar in concurrence with the operation of the unopposed forces of chemistry. I thought there must be a channel of nervous communication between centre and organ, which, if interrupted, would place the liver in the same condition as it is after death; and, on experimenting, I found that dividing the vertebral portion of the sympathetic seemed to occasion this. I at once naturally concluded that everything had now become clear. The fact, indeed, that lesion of the vertebral filaments of the sympathetic produces saccharine urine is indisputable. This certainly is the truth, but it is not the whole truth. There remain behind certain other facts in connexion with the sympathetic which show that our knowledge is still incomplete—which cast a shadow of uncertainty over the notion that I first looked upon as so clearly made out. Lesion of many other parts of the sympathetic, besides its vertebral filaments, occasions saccharine urine; and I really must admit that I am scarcely in a position yet to give a satisfactory explanation of the phenomenon. I will, however, furnish a summary of the results that have attended my experiments on lesion of the sympathetic in different parts of the body; it being understood, unless otherwise stated, that the animal experimented on was the dog.

Division of the vertebral filaments of the superior thoracic ganglion just before entering the vertebral canal on both sides of the neck, has given rise to urine, in *half an hour's* time, so highly charged with sugar as to pro-

duce a copious yellow reaction with the copper solution. On the following day the urine has lost its saccharine quality, and on the day succeeding this the animal has died.

In another experiment, the filaments, on one side of the neck only, were at first divided. Up to *an hour and a half* after the operation, the urine gave only a trace of saccharine reaction. The filaments on the other side of the neck were now divided, and in *half an hour's* time the urine was intensely charged with sugar. Death took place on the following day. There was no urine in the bladder to test; but the liver was found entirely free from hepatine and sugar.

Although in these experiments on division of the vertebral sympathetic before entering the vertebral canal the pleura is not opened, yet the most intense pleuritic inflammation is set up, which has always occasioned a fatal termination. The inflammation evidently results from injury to the sympathetic, because, when, owing to a difficulty in seeing them, it has proved that I have failed in dividing the filaments sought for, there has been no pleurisy set up.

I have followed the vertebral filaments upwards, and divided them in different parts of their course through their canal. A curious circumstance is here to be noticed. In my first experiments, as I thought it useless to attempt to tie the vertebral artories after entering their canals, and as I could not isolate the sympathetic so as to effect its division without dividing also the associated vessels, I considered it best to ligature the two vertebrals before reaching their canals, and, likewise, the two carotids, on account of the freedom of anastomosis existing above. It might be imagined that on suddenly arresting these four streams to the head, considerable disturbance of the cerebral functions would be produced. Such, however, is not the case; indeed, scarcely any visible effect is produced. Now it happened, where these vessels had been tied, that on afterwards cutting or tearing through everything in the vertebral canal on each side of the neck, a strongly saccharine state of the urine was rapidly induced. For the production of this effect, however, the deligation of the arteries is an indispensable part of the experiment, although when this operation alone is performed, no diabetic result is produced. The conclusions furnished by my experiments on this point stand thus: Careful deligation of the two vertebral (below their canal) and two carotid arteries does not occasion saccharine urine, nor does simply tearing through everything traversing the vertebral canal on either side of the neck—an operation that I find may be performed without any material loss of blood being occasioned; but when these operations are combined, sugar rapidly appears in the urine. I must confess I do not precisely see the *rationale* of this, but it was some time before I could even make out anything at all definite on the subject.

Although division of the carotid portion of the sympathetic in its course through the neck does not produce saccharine urine, yet removal or injury of the ganglion above—the superior cervical ganglion—occasions in a very short period a most strongly marked effect. The exposure of this ganglion, after a little experience, is an operation that is easily performed, and nothing of importance besides need be disturbed. In one experiment, I removed the ganglia of the right side only at first. The urine collected in an hour's time was intensely saccharine. I may observe, as an effect of the operation also, that the ear on the right side was rendered 3° (Fahr.) warmer than the other, the pupil more contracted, and the nostril drier. On the following day the urine was still saccharine. The next day, however, the sugar had disappeared, and on the day succeeding this, the urine not being saccharine, the ganglion of the left side was removed. In half an hour's time the urine had again become strongly charged with sugar.

The following is the amount of sugar found upon analysis in two experiments, where both ganglia were removed at once. In one, the urine half an hour after the operation contained 20·5 grs. of sugar to the fluid ounce, and *two* hours after, 11·4 grs. In the other, 22·86 grs. per ounce were given one *hour and twenty* minutes after, and 34·08 grs. half an hour later.

I have conducted several experiments upon the sympathetic system in the chest. Division on one side has sometimes produced as rapid and strongly-marked a diabetic effect as has occurred after the other lesions of the sympathetic to which I have referred. At other times, however, there have been only traces of sugar discoverable in the urine, and at other times still none at all. Precisely the same remark applies to division of the sympathetic on both sides of the chest. Seeing this diversity in the results here obtained, I have purposely operated on different parts of the sympathetic trunk in its course through the chest, but have failed in finding anything connected with the mode of performing the experiment to account for the variety observed.

In the case of the rabbit, diabetes is not so readily established by injuring the sympathetic as it is in the dog. The superior cervical ganglia are conspicuous bodies and easily reached. Their removal in the healthy and vigourous animal occasions saccharine urine, although the effect is not produced with anything like the rapidity that I have noticed in the dog. I have tried to divide the sympathetic at the lower part of the neck, but have met with difficulties in the performance of the operation arising from the animal being so easily killed, and the nervous filaments being so small and difficult to find, that I do not yet possess a record of a fair experiment to speak of.

The diabetes resulting from all these operations on the sympathetic is quite of a temporary character. What the chain of phenomena is that determines the effect constitutes the important problem that yet presents itself for solution. I have strong reason to believe that it does not depend, as I originally started out with the notion of, on a simple interruption of transmission of nervous force between the medulla oblongata and the liver. The introduction of a sufficient quantity of carbonate of soda (200 grains) into the circulatory system, previous to injuring the sympathetic, checks the production of diabetes. This I consider a most important fact, and I have over and over again verified it, having selected generally the superior cervical ganglia to operate upon. It is true, a diabetic effect does not universally follow removal of these ganglia where no injection of carbonate of soda into the blood has been resorted to; but it generally does so. Where, however, the injection is previously employed, I can confidently look for an absence of any diabetic effect. It has been the same when I have operated on the vertebral filaments ascending from the chest, instead of the superior cervical ganglia.

Although, however, the carbonate of soda thus prevents the appearance of sugar in the urine after lesions of the sympathetic that otherwise occasion it, yet it has no such power where destruction of the medulla oblongata is effected, and the circulation is maintained artificially. The introduction of carbonate of soda into the circulating current is quite ineffectual in preventing the production of sugar in the liver as a post-mortem occurrence, and, therefore, in keeping the urine free from saccharine impregnation when pithing and artificial respiration are performed.—*Lancet*, *Nov.* 24, *Dec.* 1, 8, 15, 22, *and* 29, 1860, *pp.* 503, 529, 555, 579, 607, 629.

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36.—*On Sugar in the Urine.*—"Sugar in the urine," writes M. HENRY MUSSET, "does not necessarily imply the existence of diabetes. One may pass sugar with the urine, and yet enjoy perfect health. Dr. Blot has shown that sugar exists normally in the urine of all women during parturition, of nurses, and of a certain number of women during pregnancy. Dr. Leudet has shown in paraplegic patients that there is a constant relation between the appearance of the nervous cerebral accidents and glycosuria. Dr. Itzigsohn relates a remarkable case of traumatic diabetes, occurring in a blacksmith who had received a blow on the top of the head. Dr. Todd has also given a case of diabetes which was observed in a woman after she had received a blow on the head. Prout observed sugar in the urine of dyspeptics and aged persons. And Dr. Goolden in children during dentition. Thus,

then, there are numerous circumstances, physiological and pathological, which directly or indirectly concur in the formation of sugar in the economy.

"When we consider that in everybody there are products which are returned by the lymphatics into the general circulation; that the transformation of starch goes on normally in the intestines; and that it is accomplished even in the mouth under the influence of the salivary diastasis; and if, moreover, we recollect—that glycosuria accompanies dentition, dyspepsia, certain cerebral disturbances, that it may be caused by irritation of the brain at the origin of the eighth pair of nerves; that it exists in pregnant and parturient women and nurses—are we not naturally brought to the conclusion, that diabetes is a neurosis troubling the harmony of the assimilating functions?"—*L'Union Méd.—Med. Times and Gazette, Dec.* 10, 1859, *p.* 594.

# SURGERY.

## AFFECTIONS OF THE BONES AND JOINTS, ETC.

### 37.—ON AMPUTATION AT THE KNEE-JOINT—AT THE KNEE—AND EXCISION OF THE KNEE-JOINT.

By Richard G. H. Butcher, Esq., Surgeon to Mercer's Hospital, &c., Dublin.

[The ordinary mortality after amputation of the thigh is estimated by Prof. Syme at not less than from 50 to 80 per cent. This immense mortality has awakened a spirit of emulation amongst practical surgeons towards perfecting a better method. The circular, the oval, and various modifications of the flap operation have each been in vogue, and temporarily praised as each superior to the other. The cause of the mortality can, in most cases, be traced to the division of the femur, the compact tissue of which is peculiarly dense, and liable to exfoliate, and its lining membrane peculiarly extensive and vascular, and hence liable to inflammation. These dangers are avoided in amputation at the knee-joint, the credit of bringing which operation into notice is due to Professor Syme, though performed so early as 1764, by Hoin. Mr. Fergusson first performed the operation in England. The following case shows the way in which Mr. Butcher amputates just above the knee, and it shows also what may be done by pains and attention in cases of secondary hemorrhage. The whole of Mr. Butcher's paper is so valuable and so full of practical matter that we have felt it difficult to abridge it much without diminishing its value—hence the length of the following extract.]

February 25, 1860. The man being carried into the operating theatre, he was quickly brought under the influence of chloroform; when so, he was drawn down gently to the edge of the operating table, so that his pelvis rested thereon. The right leg was then secured to the leg of the table, so as to guard against any struggles, and permit me free access to the affected limb. The femoral artery was compressed at the groin by an assistant, and the leg steadily supported by another; standing on the right side of the patient, I drew a long, narrow-bladed amputating knife in a curve way across the fore part of the articulation, the heel of the knife being laid on the outer and back surface of the external condyle, at where I thought its cartilage ceased; it was then carried downwards and outwards across the lower margin of the patella, and corresponding to the insertion of its ligament, and then upwards to the internal condyle, at a point opposite to that where the knife was first placed; the flap was then raised off the patella by a few touches of the point of the knife, and then came directly back at its upper edge, severing the rectus and muscles attached to it, and freely opening the joint in front. So well back were the angles of the wound, and so accurately placed, that transfixion was accomplished with facility, the flat of the knife lying evenly against the bone, as it travelled from within outwards; the leg was then very slightly bent, so as to permit the instrument to slide more readily over the posterior projection of the internal condyle; having passed this point, the leg was again straightened, and the knife kept close to the bones throughout the entire extent of the calf, and then made to cut directly out; the anterior flap was next freed from its synovial lining, and lifted from the sides of the condyles, and in front, just free from the margin of the cartilage of incrustation. The flaps being well held up before and behind, and the limb carefully steadied, I laid the fine blade of my own saw on the healthy osseous tissue close to the cartilage, but not infringing upon it, and then cut the bone in a curved manner from before backwards, thus securing even a longer stump, more of the bone free of cartilage, and exempt from sharp and irritating edges, better in every way for adaptation to the soft parts. I was the

*first, I believe, to lay stress upon this method of sawing the bones in certain amputations;* thus the limb was severed; the arteries were then secured—seven in all—and the patient conveyed back to bed; he quickly awoke from the effects of the chloroform, which acted most favourably, the patient not having experienced any pain; he bore the operation well, not having lost, I think, a drachm of arterial blood; the flaps were dealt with, and left to glaze; in seven hours I dressed the wound, and it was astonishing how admirably the parts came together; at first it might have been supposed that too much soft parts had been kept, but this proved not to be the case; it will be seen that the same complicated way of forming the posterior flap, as described in the *recent amputation*, was not followed out or necessary here; here the calf was wasted, attenuated from want of exercise, from general emaciation; and here, too, the lower end of the flap was cut straight out, so that, when doubled up, the additional muscular tissue preserved was beautifully covered in by the thin anterior flap; all the surfaces were gently freed from clots; all the surfaces were accurately brought together; the ligatures chiefly being at the posterior angles; several points of the interrupted suture were had recourse to, and some wide straps of adhesive plaster, extending from high up behind the thigh, carried over the face of the stump, and up upon the forepart of the limb, so as to afford a long, gentle traction forwards; the line of juncture secured well in front, and altogether the covering in of the bone was abundant and admirable; pillows were adjusted also to favour this end.

On the 26th, the day after the operation, the pulse had come down from 125 to 100; and in every way the man was greatly benefited by the operation; so quickly did everything go on, so little pain, so free from discharge, that I only removed one or two of the plasters near where the ligatures lay; so little redness or resentment to the presence of the stitches, that I suffered them to remain until the 8th of March, when I cut them all out, union being effected by first intention throughout the whole track of the wound, and the entire flap well up and fixed in its berth; great caution was employed in giving it due support, both by adhesive straps and pillows nicely arranged.

On the 13th of March, the smaller ligatures came away, and, on the 16th, the main ligatures (two in number) came away without a trace of blood; this was at 9 a.m.; however, I was sent for at 6 p.m., as bleeding was reported to have come on; the dressings were all stained with scarlet blood, and a few drops occasionally escaped from beneath and through them; on exposing the inner angle, the blood came up quickly from the track through which the ligature was drawn in the morning; I elevated the stump nearly at right angles with the trunk; put a few evenly-adjusted compresses over the face of the stump, and made more forcible pressure upon it by wide adhesive straps brought from behind forwards, and then it was propped by pillows; the pad of the compressor used in aneurism was brought to bear gently on the femoral artery, just after it passed off of the pubes; ordered twenty drops of tincture of opium, and fifteen of digitalis, to lower the action of the heart and throbbing vessels; ice was placed in bladders over the dressings, and along the course of the artery in the thigh; thus the bleeding was arrested. This bleeding was to be attributed to the man suddenly sitting up in the bed, and keeping this posture immediately after I left the ward in the morning. No doubt, the sudden change of position excited the violent emotion of the heart's action towards hastening, propelling onwards the current which broke down the recently-glued parts, washed away the, as yet not perfected, plasma. Visited at 9 p.m.; no return of the bleeding; flushing of the face and heat of body gone; he lies in the same position, perfectly horizontal, with the thigh propped up in almost a perpendicular direction; gentle pressure over the femoral artery near the groin, and ice over the dressings; the digitalis and opium to be continued every third hour.

March 17th. Slept quietly; no return whatever of the bleeding; circulation quiet; to continue the opium and digitalis; all drinks cold; milk, a little chicken tea.

18th. No return of bleeding; treatment as on yesterday; parts undisturbed.

20th. No return of the bleeding; so dressed the stump and placed it almost in the horizontal position, slightly raised and supported at the end; the man complaining very much of the fatigue consequent upon the last day's constraint in the raised posture.

On the morning of the 22nd, at six o'clock, I was hastily summoned to see the man, and found him with arterial blood flowing from the same point as it occurred before, but far more rapidly, and in greater quantity. I elevated the limb again at right angles with the trunk, steadied it effectively in this position, put an aneurism clamp with moderate tightness over the femoral artery, high up, and then removed all dressings off the point through which the blood welled up, and put a small dossil of lint in the little hole, a larger over it, and then another, wider and firmer than the former, and upon this steady finger pressure; this I kept up for half an hour myself, and then had relays of students, who unflinchingly gave me their assistance for nine consecutive hours. During all this time the pressure was never suffered to relax; during all this time the same assiduous care characterized their efforts. The digitalis and opium were given internally in somewhat larger doses; and ice-bladders were laid over all the greater portion of the stump, and along the sides and forepart of the limb; to these combined, continuous, and well-devised means, may be ascribed the staunching of the blood, the permanent sealing of the wound, the healing of the part, completed and perfected for ever. After this time no casualty occurred; and in one month after the operation the stump was perfect in every way; at this time I had a photograph taken of the man by a talented young friend; the stump is in every way beautiful in proportions; the flaps which, as before observed, seemed at first too large, now constitute a firm, compact, well-arranged cushion, amply covering the bone and their line of union well in front, and out of the way of pressure or injury. Soon after this, I had an artificial leg made, upon which the stump rested firmly, and with which the man could walk about without the slightest pain or inconvenience; the end of the stump rested firmly on a nice soft cushion, and he did not suffer or complain from jar or pressure. Thus he was going about, when ulceration of a very unhealthy character attacked the cicatrix after the burn on the right leg; it spread rapidly, and assumed a warty character; the entire surface, however, was destroyed, and the part is now just healed, and the poor fellow will be able to get about as well as ever: nothing can be more satisfactory than the state of the stump.

The plan and method of the operation which I have described are, I think, superior for many reasons to amputation at the knee-joint, independent altogether of those cases where the tibia is *extensively diseased*, and where the femur is only in a minor degree affected in its articular surface; but I think it is preferable in all these cases where the leg is so extensively diseased, necrosed, or shattered by violence, that amputation at the knee-joint would be selected, as it is now, by many. I have no doubt, whatever, that in all cases the cartilaginous surface of the femur should be removed; according to the curved sections I have described, cut in this way, the surface for support is just as extensive, wide, and expanded as when the cartilage is untouched; we find that in many of those cases where it has been retained, unpleasant symptoms have originated from its being attacked by ulceration, and sometimes death. Now, as to the preservation of the condyles to act as a guard to the cicatrix puckered into the hollow behind, a flap being made from the forepart of the limb; even should this security be deemed advisable, it is afforded by my section of the bone being curved a little more; there is no necessity for the risk being incurred of preserving the cartilages, for if the posterior flap be properly formed and brought up nicely in front, the cicatrix will be altogether out of the way of injury; it should not be forgotten, too, that some little difficulty may in certain cases pertain to getting a good covering for the condyles; yet in the hands of a skilful surgeon, this, I think, will not form an objection. The necessity for removing the articular car-

tilage in amputation at the tibio-tarsal articulation is admitted by all, for the very reason that I urge it in the knee. In the operation at the knee-joint, the patella also should be cut out; it is only in the way if retained, and may demand such a procedure at a later period. However admirable a measure amputation at the knee may be considered, it can never take the place of *excision of the joint:* it would then, indeed, cease to be a conservative measure. It is a source of the greatest gratification to me to see how steadily the latter operation is holding its ground, and bearing ample fruit.

Since the publication of my memoirs on excision of the knee-joint, I have been watching with anxious care the results of the operation. The periodicals abound with cases. In the number of the Lancet, which has just appeared while I pen these observations (Aug. 4, 1860), there are nine cases recorded, out of which number one died, a miserable and unpromising case for any operation—"a strumous, delicate man, aged 37." "He had disease of the sternum, co-existing with the ulcerated state of the cartilages of the knee-joint." Evidently the creature was surcharged with scrofula. The remaining cases are reported as most successful in their results, in every instance a limb being secured of seemly proportions, and capable for its several requirements, save flexion. From carefully reading over and maturely considering the several reports of cases published within the last few years, I think I am borne out and confirmed in the several practical deductions which I have arrived at, antecedent to this time, and which I laid down, as some supposed, in rather a peremptory manner; some of those opinions have been copied in my very words, and, I regret to say, without due acknowledgment.

I cannot conclude these observations in a better way, I conceive, than by again briefly condensing those directions which should guide the surgeon when he contemplates excision of the knee-joint, and advert to those truths which should give him a confidence in success.

The first and most important point to be considered is *the judicious selection of the case.* "In order that excision of the knee-joint may prove successful, it becomes imperative that the cases are carefully selected; by 'successful,' I imply not only the preservation of life, but also the saving of a limb, better than any artificial substitute, no matter how beautifully contrived. It is not applicable, for obvious reasons, where the bones entering into the articulation are very extensively diseased; for though I admit a better chance of preserving life would be secured by excision, even in this case, than by amputation, yet the member would be faulty as a means of progression; it would be short, and a useless appendage. I need scarcely make allusion to organic disease of the vicera as affording insurmountable objection; but this applies, with equal force, to the non-propriety of amputation. In such a condition either operation becomes only justifiable to alleviate excruciating torture. I do not lay much stress upon the integuments being extensively disorganized, or think that such a state militates with any force against excision, for in numerous instances, after removing carious and dead bones, I have been forcibly struck by the remarkable rapidity with which the soft parts set up healthy action and recover themselves; parts undermined and sinuous, when relieved from irritation, and set at rest, become very amenable to simple management. It has been objected to, that after resection the discharge is very great, and runs the patient rapidly into hectic. No doubt, the discharge from the divided surfaces is considerable—may be profuse—yet is healthy: it is essential to the process of reparation, and diminishes, day by day, according to judicious management; it is not so with the ichorous discharge created from the morbid action aroused by the presence of diseased and deadened bones, which, acting as the poisoning supply of the constitutional disturbance, seals the doom of the being unless relieved by operative surgery. It should never be forgotten that resection of the knee-joint is only advocated as a substitute for amputation in certain cases, and not for the simple mode of incising the joint, to which practice *Mr. Gay* has recently directed the notice of the profession." Again, the

same caution upon the necessity of carefully selecting the cases for excision is insisted upon in my second memoir, and published two years later, Feb., 1857. "In my former essay, I forcibly dwelt upon the *necessity of carefully selecting the cases for excision*, and pointed out the prominent features which should influence the surgeon; but I believe the caution has not been applied in every instance. I fear the panting after *éclât* has charmed away some from the stern dictates of judgment; yet I trust this may never be the case—infinite mischief is done by such rashness. Not only is the life of the patient jeopardized by an operation which cannot secure a useful limb for the purposes of life, but the operation itself is brought into disrepute, and open to the sarcastic criticism of those who know but little about it." "The applicability of the operation of excision of the knee-joint to cases of recent accident and gun-shot wounds will depend upon the extent, and be estimated by *the amount of injury the soft parts*, including the vessels and nerves, have sustained, as well as that inflicted upon the bones. The practical surgeon will be able to compute with accuracy, after careful examination of the parts, how far this method of preservative surgery can be put into practice, and trusted to with hopes of success."

I shall next consider the best mode of operating. In my first memoir I have mentioned the several ways recommended by Park, Mülder, Moreau, MM. Sanson and Begin, Jaeger, Syme, Fergusson, Jones, and Mackenzie, exception alone being taken to that of Syme, and in these words:—"Amongst the many modes of incising the soft parts which I have mentioned there is not one to which exception can fairly be taken except that of Mr. Syme. He advises an elliptical piece to be cut from the anterior wall of the joint, included in the arms of the ellipse, the patella. I do not for a moment doubt the propriety of removing the bone when diseased; but I know no condition that can warrant the cutting away of the flaps; if they are in a properly healthy state, they will not be found too great; after a little time, they will adapt themselves by contraction to the altered state of the parts beneath; if they are perforated by sinuses, and present an appearance which by some may be called disease, they will recover themselves after the carious bones are removed, and not be found too extensive, but still constitute an accurate involucrum for the divided osseous surfaces." The mode of proceeding which I consider best is that described in my first memoir, at page 23, in reference to my first case. "The patient was placed upon a table, lying on his back, and in a few minutes brought under the anæsthetic influence of chloroform. The leg, at extreme of extension, was steadied; the sole of the foot being planted upon the table and held so, forcibly, while the thigh was rigidly fixed by a second assistant. Standing on the left side of the patient, I leant over the knee, steadying its outer side against my chest, and with a strong scalpel cut along the inner side of the joint to about the extent of five inches. This incision was commenced below, at a point about two inches lower than the articulating surface of the tibia, and corresponding to a line a little anterior to its inner sharp edge; the knife was at once thrust down to the bone, and, holding the same relationship, was carried upwards along the femur for three inches; the saphena vein was thus gradually left behind the track of the wound. A similar incision was rapidly made on the outside, commencing below the head of the fibula, and carried upwards above the external condyle; through the entire extent of this, too, the knife was swept along the bones. Both vertical incisions being completed, they were connected with a transverse one, passing an inch above the attachment of the ligamentum patellæ to the tibia; the latter wound opened the joint fully; the lower flap was freed downwards a short way, while that containing the patella was directed upwards, but with some difficulty, owing to the thick matted cellulo-fibrous tissues which constituted its bulk. The internal and external lateral ligaments were next divided, together with the adventitious bands, the result of organized lymph deposits; the anterior crucial ligament was destroyed, but the deeper fibres of the posterior remained intact, and incorporated by dense structure with the posterior ligament of the joint; these, in turn, were divided, but much difficulty was experienced in detach-

ing them from the posterior surfaces of the bones, with due consideration for the popliteal vessels, which not only lay upon the dense elastic material, but were embedded in it. The knife was then rapidly swept round the condyles of the femur, the disease not extending higher. In the same way the articulating end of the tibia was freed from the soft parts around its circumference; the ligamentous structures being thus cut through, the leg was forcibly flexed, and the ends of the bones thrust forward.

"And now the accuracy of the diagnosis was fully verified. The femur presented its external condyle nearly all removed by caries, while the internal was not at all so extensively diseased, its posterior half being stripped of its cartilage of incrustation, and carious behind the intervening space; between the condyles was likewise carious: the external condyle of the tibia had its cartilages of incrustation removed, which was replaced by a thick fibrous substitute, while the internal presented its normal appearance (we had here strong evidence of the efforts of nature to check disease); the patella was quite carious, hollowed, and reduced to a complete shell; therefore it was dissected carefully out, the integuments in front and covering it being preserved together with the fibrous attachments implanted at its upper and lower edges. The bones being sufficiently exposed, I next proceeded to cut off their extremities, and for this purpose used the saw, which I prefer for amputations, its blade being turned in the supports and steadied so, it was easily passed behind the condyles of the femur, and made to cut forwards; a few movements were sufficient to complete the section. In a similar way its serrated edge was placed behind the tibia, and urged forwards so as to remove a thin osseous slice, together with the surface of the bone. The head of the fibula was not diseased; lying below the surface of the tibia, it did not prevent the opposition of the bones; it was therefore left intact for this special reason, as well as that the attachment of the biceps should not be interfered with. The entire amount of bone removed measured two inches."

There are some points more minutely dwelt upon in the *operation* in *my second successful case*, published in my Reports in Operative Surgery, and which I shall quote. The operation was performed April 15, 1857, on a man aged twenty-seven years.—"The patient being placed under the influence of chloroform, I adopted the H incision, the cross line passing beneath the patella; the flaps were with rapidity dissected back, and the shreds of the crucial ligament spared by disease were divided, and next the lateral ligaments; in freeing the ligamentous attachments to the bones behind, the greatest precaution was adopted; all being separated to the extent required, I swept the knife around the tibia and the femur, close to the attachment of the soft parts, and then took the saw bearing my name, and cut the bones from behind forwards."

"It is necessary here to lay caution on the operator in using the saw. He should ever remember the altered position of the limb, to facilitate protrusion of the ends of the bones, and, according to the angle of elevation must the direction of the blade of the saw traverse. The simple rule I would lay down for the correct execution of the section is this:—The blade of the saw must pass in a direction parallel with a line drawn in the transverse axis of the articulating surface; accordingly, this procedure was carried out. Thus, when the limb is placed in a horizontal position, the one in which it is to be maintained for cure, the cut surfaces of the bones will lie evenly together, no space will intervene between them behind or before; the wide surfaces oppose each other; all disposition to gliding one from the other is guarded against, and the most favourable circumstances are insured for intimate union. In the published record of cases it will appear that in some instances the surgeon has had to apply the saw a second and third time, to make the bones meet: if this be so, I am warranted in enforcing my advice.

"By section planned after this method the condyles of the femur, with their connecting osseous bond, to the depth of a quarter of an inch in thickness, were cut off. To warrant the removal of these parts, I may just state, *the incrustating* cartilages of the condyles were entirely removed; the head

of the tibia was similarly affected, and, in addition, deep pits were excavated by caries in each condyle, to the depth of a quarter of an inch. This being effected, all the thickened and diseased synovial membrane was clipped away, and the disorganized fatty mass below the patella; not a trace of the interarticular cartilages remained; the patella was coated with lymph beneath, and appeared to have struggled healthily from the disease around; it was therefore suffered to remain. Thus, then, the accuracy of the diagnosis was established, and examination of the osseous surfaces pronounced them healthy. Three arteries which bled rather freely were next tied; the flaps at the transverse incision were brought together, and maintained so by five points of interrupted suture; and the lateral incisions were left open for the ready escape of blood and serum, the purging of the cut parts. The leg was with ease put into the straight position, and placed at once in the padded box splint I had prepared for its reception. A splint was then laid over the interior part of the thigh, and the tapes fastened, sustaining upwards the hinged sides of the box; the foot was steadied by a foot-board, falling into the grooves within, and thus the leg was pressed upwards so as to keep the divided osseous surfaces in contact; lint steeped in cold water was laid along the lateral incisions, and maintained accurately in position by the sides of the box when elevated."

In my *third successful case of excision of the knee-joint* will be found some further precautions which should be borne in mind by the surgeon about to perform the operation.

I shall transcribe the case. Patient, aged 15:—

"On the 11th of January, 1858, I excised the joint, adopting the H incision; quickly the soft parts were divided and reflected, and cautiously they were freed from the bones behind. The ligamentous structures within the joint, the cartilages of incrustation of the three bones were entirely removed, and the contiguous surfaces of the bones most extensively destroyed. The condyles of the femur were hollowed out and eaten away to about an inch in extent, while the surface of the tibia was also deeply excavated, corresponding to each condyle, the intervening part being irregularly removed, the patella was also deeply carious; hence I removed it at once from the upper flap. The diseased extremity of the femur was cut with the saw invented by myself, and the section made from behind forwards; and so likewise the unhealthy surface of the tibia. The section of the femur revealed a beautifully healthy aspect, while the section of the tibia showed a softened irregular patch, about the size of a shilling, with a vascular fringe running round its confines, and separating it from the healthy bone outside. This appearance occasioned me to cut off another slice from the tibia somewhat more than a quarter of an inch in thickness, and the section exposed a perfectly healthy condition, the normal arrangement of the bone. In all, there were about three-quarters of an inch, or a little more, of the tibia removed, and an inch and a half of the femur. The divided bloodvessels were small; no ligatures were required; and I at once placed the limb in the extended position, resting upon its posterior surface in the apparatus which bears my name. Let it be borne in mind that, previous to the operation, the leg was considerably flexed and the hamstrings rigidly contracted. This condition offered serious opposition to the required posture at first, but by gentle, steady, and gradually increased force, they were compelled to yield. Now, in the adjustment of the bones, there is one practical point which I have not before alluded to, *neither is it noticed, so far as I am aware, by any writer on this subject*, namely, the caution that is requisite, when straightening the limb, *to guard against any portion of the soft parts from behind projecting forwards, and thus interposing between* the bones. Such an occurrence took place in this instance during my efforts to get the limb into a horizontal posture, but I perceived and rectified it at once; however, it might be easily overlooked, and, I have no doubt, would interfere in a very material manner with the firm union of the bones, or their growth into each other—a consummation so ardently sought for in these cases. Now, this untoward circumstance may be guarded against by the following manœuvre; namely, to adjust accurately, while the

leg is yet flexed, the posterior margin of the cut surfaces of the tibia and femur, and while the sharp edge of one bone rests upon that of the other, the tibia and femur are gently forced backwards until the opposed surfaces rest fairly against each other. During this manipulation, then, the bones are pressed up firmly against each other, as well as with an equal force backwards. By this precaution the interposition of any softened structure will be effectually prevented. After this manner, then, the bones were fairly applied to each other, and afterwards the flaps laid down. They were brought together throughout the transverse incision by a few points of the interrupted suture, while some folds of lint were laid in the lateral ones, the object being twofold: first, to prevent any immediate flow of blood, and, secondly, at a later period, to permit a free escape from the part, so that purulent matter should not be pent up; the front splint was laid down, and each side of the box was then lifted, and an additional compress of lint placed over the wound at either side, so that an even, equable support was given, and maintained. During this entire adjustment, from first to last, the leg was pressed up against the thigh; and, to secure it in this position, the foot-board slid into the grooves for its reception was most efficient, steadying also the foot at a right angle with the leg. An additional pad, wedge-shaped, was placed at the outer ankle, and projecting upwards, so as to maintain the foot in a straight line, and prevent any dropping outwards or eversion. The straps were then buckled round the box, and the patient, still under the influence of chloroform, removed carefully to bed. She, however, awoke from her sleep quite unaware that the operation had been performed."

The next question to be considered is, *what should be the mode of managing the limb after the operation?* I have answered this in each of the foregoing cases. *It should be at once forced into the horizontal position, and put up before the patient is removed from the operating table.* I claim priority for this advice and most valuable maxim, because the very words I have written have been copied, and without acknowledgment. In my first memoir, 1855, p. 58, it is written: "Immediately after excision of the joint is accomplished, *before the patient is taken from the operating table, the limb should be placed in the extended position, and retained so immovably in a solid case*, such as I have described. In some instances it has been shown that the difficulty to force back the femur, and prevent its projecting in front, has been very great; by proper manipulation, this difficulty can be overcome without violence, by one or other of the following measures. Generally speaking, this, the milder method, will succeed; when the end of the bones are cut away, gradual yet powerful extension, continued for ten or fifteen minutes, and longer if necessary, should be made upon the leg, so as to counteract, tire out, and subdue the violent contraction of the hamstring muscles. During this manœuvre the thigh should be gently, yet steadily, pressed backwards, so that the cut surfaces of the bones may be opposed to each other, and then, being pressed back into a straight line, and retained so by proper supports, they offer mechanical resistance to each other, and thus displacement is prevented; but if this method fails, and it will most likely do so in those cases where the leg has been for a length of time flexed upon the thigh, and the muscles have assumed a spastic contraction of a settled character—here I would most certainly recommend the surgeon to *divide the hamstring tendons, in preference to cutting off another piece of the healthy bone.* This becomes more imperative when the head of the fibula has not been removed, and the tendon of the biceps interfered with; it is the powerful and spasmodic action of these muscles, dragging the leg upwards and backwards, that creates to a great extent the deformity, by the thrusting of the thigh bone forwards; *by their division, then*, not only is reduction easily secured, but all tendency to after-displacement checked. *The limb*, I repeat it, *should be secured from the very first in the extended position*, and rigidly maintained so through the entire cure. In my mind the same arguments apply here as in the treatment of fractures of the thigh—the object of the surgeon being in either case to avert spasm, and to obtain, as quickly as possible, a permanent osseous union between the disunited bones." And again, in my second memoir, the

practice is as forcibly inculcated at page 62: "I do not believe one word of the 'impossibility' of placing the limb in the straight position at once, and retaining it so: it can be done if the surgeon is up to his work; and this first adjustment can be made without any pain to the patient, as chloroform annihilates sensibility. I cannot find words to enforce, with the power I desire, the importance of this measure. Amongst the foregoing cases there are lamentable instances where it was not adhered to, and which, I trust, will appeal forcibly to the mind of every thinking man. Independent of the advantages of steadying the cut surfaces, the prevention of the divided bones from irritating the surrounding tender parts, the subjugation of all spasm, the limiting of the inflammation to the bounds only necessary for repair,—we have still another great benefit accruing from *putting up the limb at once*, namely, the mind of the patient is at rest; that protective watchfulness over it is removed, and which probably, prior to the operation, had caused nights of restlessness and want of sleep. The same apprehensiveness, when the bones are divided, will remain, aye, be increased, if the limb be not immediately fixed in the straight position: but if the control of the patient over the part be checked, he feels confident in the security; repose quickly follows, and sleep is generally induced; if not, opiates will act more certainly, pain being subdued."

*I am confident that the box-splint which I have used in all my cases is by far the best apparatus for securing immobility of the divided parts* in a proper axis, preventing projection forwards of the thigh-bone, keeping the divided osseous surfaces in contact, permitting a safe and ready way of dressing the limb without disturbance, and affording, at the same time, the greatest amount of ease and comfort to the patient under existing circumstances. The apparatus is figured as applied to one of my cases, in my Reports on Operative Surgery, and a description is thus given in my first memoir, two years before, at page 25: "The limb was placed in a horizontal position in a wooden case which I had made for the purpose. The sides were attached to the back part by hinges, so as to allow of being let down at the time of dressing; they were likewise of unequal lengths, the internal extending nearly as high as the ramus of the pubes, while the external passed up to the axilla, similar to the long splint used by me in fractures of the thigh; the lower end of each lateral piece presented on the inner surface a number of grooves about an inch apart, so that, when the sides were elevated, the foot-board was received into any opposite pair of them, according to the distance required; this lower piece acted in two ways; not only did it maintain the foot at a right angle with the leg, but it steadied the sides, and prevented their being pressed inwards from their vertical direction by the tapes and buckles which girthed the apparatus on the outside. The box was supplied with hair cushions, carefully adapted to its entire extent, some being covered with oiled silk. In addition to the posterior, lateral, and foot support, a broad splint, well padded, had to be placed over the anterior surface of the thigh, extending from a little below Poupart's ligament as far as the junction of the upper and middle thirds of the leg, and secured firmly down by the surrounding web-belts, so as to counteract the powerful tendency towards the distortion of the limb forwards. I have already mentioned that the external side of the case passed up to the axilla, the object being to secure the straight position for the limb. It was kept in contact with the trunk by a wide girth passed around both. The patient expressed himself as being very comfortable with the limb done up in this way, and it was satisfactory to the surgeon to behold it, every requirement seemed so entirely fulfilled."

Here, again, I wish to claim *priority* for *the application of the anterior splint and likewise for the extension of the external one along the trunk, as also for the foot-board*, gently retaining the foot in a proper axis, and the tibia in contact with the femur; *it is a great error to suppose that this arrangement is not most advantageous, just as great as to suppose that the fractured extremities of a broken bone should not be placed in contact, or to argue that, because adopted, ulceration would be likely to follow.*

*The more rigidly the limb can be united the better*, for the reasons express-

ed at page 59, Memoir I.: "Some operators contend that a slight degree of flexion and extension is desirable after excision of the knee-joint. To this opinion I cannot subscribe for two reasons; first, because as a means of sustentation the limb would be found inadequate; and not so sightly in appearance; secondly, the very motion perpetuated between these bones, already prone to carry on unhealthy action, would become an exciting cause for a renewed development of disease. No; I conceive, for perfection as a result of excision, *the tibia and femur should be grown into each other and bound by callus:* and, far better still, when the patella, deprived of its cartilage, and undisturbed from its berth in front, becomes fused into this connexion. A limb cured in this way will harmlessly endure the fatigues of travel. An objection has been made to uniting the limb in one solid piece, on the grounds of its liability to fracture being far greater than when some motion is permitted between the bones. My answer is, such a result has never taken place; and I am equally certain that the force required to break the callus union or limb in any part of its length would entail far more grave and serious mischief if applied to a limb enjoying partial motion; in the latter, the fibro-ligamentous structures would be all torn up from their connexions, and, as a sequence, high and active inflammation readily set up, formed by the rapid formation of matter, and all its train of serious consequences. Even suppose this burst of mischief checked by energetic treatment, the danger would not be removed; a slow, insidious action would rouse up the latent disease, which in turn would prey upon the general health, until the only chance of preserving life would centre in amputation. Now, in the former case, no such dismal consequences present themselves; the fractured limb might be brought to its full length, and maintained so by the application of the long splint, until union be again accomplished." I have laid great stress upon *the careful dressing of the wound, and the free administration of sedatives, stimulants, and support at suitable times in these cases*, and most strongly insisted upon the practice. "The amount of success following all severe operative measures greatly depends upon the care and assiduity bestowed on the after treatment, both locally and constitutionally. Probably, in the entire range of operative surgery, there is no class of cases which demands such close and attentive looking-after as that now under consideration. It will not be sufficient for the surgeon after he completes a severe resection, be it in ever so masterly a manner, to delegate his duties to another, and, after he puts aside the knife to consider his part done. No; if he is desirous of success, he must use his own hands, and be the dresser, while by his watchful eye he will readily discover the early threatening of incipient mischief. After all severe surgical operations I am in the habit of supplying stimulants and sedatives very freely, together with nutritious food apportioned to the assimilating powers of the individual. This treatment is more imperatively urgent when the patient is advanced in life, and has endured lingering disease for a length of time. It is, I would say, equally necessary to the infant and the child; whereas, in reference to the middle periods of life, the diminution or increase of supply must to a great extent rest upon the judgment of the practitioner. By the copious administration of stimulants the flagging powers of life are upheld; by the free exhibition of sedatives, pain, and irritation, and spasm are subdued." Relative to the shock of the operation and the administration of chloroform, I have thus written: —"*Happily, the shock now need not be so greatly estimated or dwelt upon.* Chloroform protects the sufferer; and I certainly attribute much of the success which in latter days has attended this formidable operation, to the beneficial agency of this potent medicine. In nearly all the cases which I have collected, it has been used; in every successful instance narrated it has been given. In the case upon which I operated, the man was reduced to the lowest state, and would almost to a certainty have sunk from the shock either after amputation or excision; yet by this powerful means all violent struggles were prevented, and, of course, the patient saved from subsequent exhaustion." How different this tranquil sleep from the writhing torture of the sufferer under a similar operation described by Sir Philip Crampton: "The

poor girl, who, in coming into the operating room, exhibited the greatest fortitude, and even cheerfulness, on the instant that the knife was applied to the skin became so ungovernable that four strong assistants could with the utmost difficulty retain her upon the table. This necessarily prolonged the operation, and no doubt very much increased its severity; the removal of the divided extremity of the femur was here rendered a work of infinite difficulty and danger, as, when the knife was passing between the popliteal artery and the bone, and actually in contact with the former, no entreaty could induce the poor girl, *whom terror seemed to have deprived of her reason*, to remain for one moment at rest, she struggled so violently with both limbs." *In every case of resection which I have performed I have used chloroform, and to its administration I again repeat is due, I think, in a great measure the invariable success which I have met with.*

Briefly to recapitulate those directions for excision of the knee-joint, which I have laid down, and would still insist upon:—

1. *The judicious selection of the case.*—The bones not being diseased far beyond their articular surfaces; while, if upon section found to be a little more than had been expected, the part should be gouged out, or an additional thin slice removed; but, if to a greater extent, amputation should be at once resorted to, and, as recorded in my first memoir, with a hope of excellent success. Again, the Report goes on to show that amputation may be performed some days after excision, should any unfortunate circumstance in the management of the case have arisen to demand it. In this same paper seven instances are recorded of amputation of the thigh, and all made rapid recovery, save one.

2. *The H incision should be preferred.*—The perpendicular strokes placed well back, so as to allow all fluids and discharges to drain off—far more effective and safer than any opening made in the popliteal space.

No portion of the flaps to be curtailed, though they may be thinned of any thickened fibrinous matter or diseased synovial membrane; the latter, particularly, should be clipped away with a strong scissors. All ligamentous fibres, both around and within the joint, should be cut through, and the extremities of the bones fairly freed and exposed.

3. *The patella should be taken away in all cases, whether diseased or not*, and then the section of the bones, well thrust out in front, should be made with "Butcher's saw" from behind forward, due attention being paid to the axis of the thigh bone at the time of its division.

4. *All bleeding vessels should be tied, or any that have sprung and retracted should be drawn out and secured*, so as to guard against intermediary hemorrhage.

5. *While the patient is yet on the operating table, the limb should be placed in the horizontal position, either by gentle and steady traction, combined with pressure of the cut surfaces of the bones backwards, or, if necessary, the division of the hamstring tendons.* Their support behind, in every case, I look upon as of great value; therefore their section must be looked upon as a last expedient towards straightening the limb.

6. *During the adjustment of the bones, great caution should be exercised, that their surfaces be throughout their extent in contact, and that no soft parts intervene.* The flaps should be then laid down, and connected by suture closely throughout their transverse division, while the lateral incisions should be brought together only at their extremities by one or two points, and the central portion of each, that corresponding to the division of the bones, should not be brought in contact, but dressed lightly with lint soaked in oil, thus securing a ready outlet for the escape of fluids. The extremity should next be cautiously laid upon "Butcher's box splint," padded to the natural configuration of the limb, its sides elevated, foot-board applied, suitable pads introduced, and then the anterior splint laid on, taking the place of the assistant's hand, which, from the first, restrained the femur from projecting forward; then the straps buckled, the waistband applied, and the patient may, with safety, be removed to his bed. The bed should be prepared in this way, and consist of a couple of hair mattresses laid one upon

the other, evenly supported, and intervening between the upper one and the sheet, a folded blanket, feather pillows for supporting the head and shoulders; the bed should be, likewise, moderately warmed, so as to prevent the patient being chilled when put into it.

7. *The limb should not be disturbed for several days;* the length of time depending a good deal on the season of the year when the operation is performed, whether it be in the heat of summer or the cold of winter. After five or six days it may be necessary to let down the sides of the box-splint, to sop up discharge, change lateral pads, and soiled dressings, &c. By the apparatus named, the facilities for cleansing the limb are so efficient that it may not be requisite to lift the member from its support for even so long a period as three weeks, as evidenced in my own practice. Should, however, it be considered expedient to change all the dressings, the anterior splint should be steadily held back by an assistant, and the limb pressed up to it, thus guarding against any starting of the femur forwards, or displacement laterally, when lifted from its bed. When the box is prepared, freshly arranged, the limb, controlled after the manner mentioned, should be laid down, the side splints elevated, foot-board secured, and the straps over the anterior splint first tightened so as to maintain it in that position, from which it was never suffered to change. I would impress this advice still further—if the straps be unloosed for any purpose, *the hand of an assistant should steadily keep the anterior splint in its position, and well pressed back, until the artificial support is again brought to bear upon it, and fastened.*

8. *In cases where large abscesses form in the vicinity of the excised joint, or up along the thigh, Chassaignac's drainage tubes may be used with the best hopes of success.*

9. *The free administration of stimulants and sedatives, imperatively demanded in all cases of excision*, regulated to a certain extent, by age, sex, temperament, and habits.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 257.

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## 38.—PROGNOSIS AND TREATMENT OF SEVERAL VARIETIES OF CHRONIC DISEASES OF THE JOINTS.

By William Pirrie, Esq., Professor of Surgery in the University of Aberdeen.

[We take the following extract from the 2nd edition of the author's work on "The Principles and Practice of Surgery." The work itself is practical, and the descriptions are concise and clear.]

I have been at great pains, and have spent a considerable sum of money, for the purpose of arriving at a correct conclusion as to the curability of this disease of the joints (scrofulous chronic synovitis), and the firm belief at which I have arrived is, that in the event of the disease not being so far advanced as to have given rise to disorganization of the joint, almost every case is perfectly curable, on the expenditure of a sufficient amount of money in procuring the use of proper remedies. I have taken a great interest in this class of cases for many years, and have been long in the habit of treating some at my own expense among the poor, and have not a stronger belief in almost any surgical subject than in this, that unless total or very extensive disorganization of the joint has taken place, most cases, however unpromising, are curable. The treatment from the use of which I have seen such gratifying results consists in residence in the country, in a dry, bracing atmosphere, with much exposure in the open air to the light of the sun; in maintaining the atmosphere in the patient's room perfectly pure by day and by night; in sleeping in a room in which there is free admission of the light of the sun during a reasonable portion of each day; in guarding against anything which could cause any impurity of the atmosphere; in sleeping in a bed so arranged as to allow free circulation of air around the patient, and as much exposure to the light as possible; in the daily use of animal food, *cream*, and cod-liver oil, along with some of the preparations of iron; in *maintaining* a healthy condition of the skin, by the means proper for that

purpose; in watching the condition of the digestive organs, and maintaining them in a vigourous state; and, if necessary, giving some alkaline preparation to correct the acidity of stomach, which is so injurious in the scrofulous diathesis, and so certain, in the way explained in the chapter on tubercle, to keep up the error in the constitution of the blood, which constitutes so great a part of the scrofulous diathesis. These means, along with cheerfulness, mental occupation, encouragement, and suitable local treatment, certainly lead in most unpromising cases, with very few exceptions, to the best results that can be desired. It is exceedingly important to encourage the patient, and to produce a firm conviction that benefits will result from treatment. I was much struck with the manner in which this was expressed to me by a patient some years ago. The patient was a female, who had a bad knee; she had made up her mind to allow it to be removed, and as it was a very unpromising case, I thought it an excellent one for testing the usefulness of the above mentioned treatment. I got the treatment instituted to the utmost extent I could desire; the knee got perfectly well, and when the woman, who, I have no doubt, was a sincere Christian, called on me to thank me for the great interest I had taken in her case, she remarked:—"I find medicine, like the Gospel, must be received in perfect faith to get the full benefit of it; I had such comfort and pleasure in following all your directions, even at my worst, because I felt sure they would do me good, as you were always confident that I should get better." The reason why so many poor people lose their limbs from this disease is, that their poverty renders them unable to place themselves in circumstances to obtain the remedies essential for cure. I have often thought it a matter of the deepest regret that persons who leave much for benevolent purposes, seem not aware how beneficially funds might be appropriated if left for maintaining and treating in the country, in healthy situations, hundreds of the poor of our towns, who are constantly dying or losing limbs from the effects of scrofula.—*British and Foreign Med.-Chir. Review, Jan. 1861, p. 127.*

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## 39.—ON THE TREATMENT OF CONTRACTIONS OF THE KNEE-JOINT.

By Holmes Coote, Esq., Assistant-Surgeon to St. Bartholomew's and to the Royal Orthopædic Hospital.

[The usual inflammatory affections of the knee-joint, whether acute or chronic, strumous, gouty, or rheumatic, terminate naturally in by far a greater number of cases than it is commonly thought. The patient is left with the utility of the limb lessened, but not destroyed. There are, however, various complications, especially extension of inflammation to the cancellous head of the bone, from imprudent use of the limb, which prevents the disease coming to its natural termination.]

In the present lecture, one great object which I have in view is to urge you, even in the severer cases, to be in no hurry to operate, whether your inclinations may lead you to amputation or excision. Study rather the processes of repair which nature can effect, and then see how, by mechanical after-treatment, you may rectify either weakness of the proper structures or faults of position.

I have a great objection to quote general statistics, because they include patients of every age, and diseases of infinite variety; but I believe that the favorable returns presented by those who have written of late upon both these operations proceed from the fact, that many of the patients have been young subjects; indeed a considerable proportion have been infants.

Now I protest against the practice of excising a knee-joint, except in very exceptional cases, in children. Even the practice of amputating the thigh in such cases is nearly out of date, and you do not often witness either of the operations here. Out of 46 cases of amputation of the thigh, the total number performed in St. Bartholomew's Hospital since January, 1857, to the

present January, 1861,—a period of four years,—seven only of the patients have been under fifteen years of age, and out of that number, three of the operations were undertaken on account of severe accidents, leaving only four operations on account of diseased joints,or about one a year. And of those four children, two were in a state of extreme emaciation; a third was suffering from hæmoptysis.

Upon mentioning these statistics to a friend of considerable experience, I was told that the explanation of the rarity of operations for diseases of the knee at this hospital proceeded from the fact, that the severe cases were no longer sent to us; for surgeons now-a-days undertook their management elsewhere. I endeavoured to disabuse his mind of the idea, but I fear, without avail. But, perhaps I may be more successful with you. The reason is to be found in the greater patience on the surgeon's part; in the application of sounder principles of treatment. The cases are of the same character as formerly, but somehow patients themselves have made the discovery, that time works wonders in matters of restoration and repair. The records of the Orthopœdic Hospital will show case after case in which patients have persisted in retaining limbs condemned three or four times. They present themselves at that institution after all morbid action has ceased, with a limb contracted, and perhaps, shrunken, but yet admitting of replacement in the extended position. The rarity of amputation of the thigh for the removal of a diseased knee in the upper ranks of life has long been a common remark amongst surgeons.

In the last number of *Braithwaite's Retrospect*, (vol. xlii., p. 168,) there are given the particulars of nine cases of resection of the knee-joint, performed in different hospitals, (originally reported in the Lancet,) and we there learn that 6 out of the number were under 18 years of age; indeed, 4 were between the ages of 12 and 6. All these children recovered from the operation, although in some cases the after-symptoms were serious. The remaining 3 patients were respectively of the ages of 23, 33, and 37. They recovered from the shock of the operation, but one died subsequently. In order to give a fair statistical return of the results of these operations, we should put them into two classes—the young (in whom I deny the propriety of operating) and the middle-aged (in whom, *under some circumstances*, the question may, perhaps, be raised); and we then see that in the latter, the mortality, from the above report, is 1 in 3. But these data alone are insufficient for any useful conclusion, though not very far from the truth. Mr. Butcher and Mr. Price, avowed advocates for excision, give, after a general summary of cases, the fatality (as applied to the knee) as 1 in 5, and this, as Mr. Bryant remarks, shows the most favorable aspect.

But we must not confine our remarks to the immediate results of the operation. It is no argument in its favor to say that we kill only 1 in 5, or 1 in 3. It is incumbent on its advocates to show that the patient derives some substantial benefit in return for the risk which he has run; that the limb which remains to him is a good and useful one. Allow me to direct your attention to this diagram. It illustrates one of the results following excision of the knee, performed in infancy—namely, arrest of development of the entire limb.

The particulars of the case have been published by Mr. Oliver Pemberton, surgeon to the Birmingham General Hospital. E. F., aged twelve, a pale, strumous-looking boy, of much intelligence, was admitted Dec. 20th, 1853, into the General Hospital, Birmingham, suffering from disease of the left knee of fourteen months' duration. The leg was bent at right angles; the circumference of the joint larger by three inches and a half than that of the opposite one, the integuments were shiny and painful; the apertures of three or four sinuses were apparent, and three communicated with carious bone. His sufferings were very great. Mr. Pemberton excised the joint Feb. 8th, 1854. The amount of bone removed measured rather more than three inches and a half. About two inches and a half belonged to the femur, and one inch *to the tibia*. The fibula was untouched. The patella was left, its under surface having been scraped. The case went on favourably. When discharged,

eight months subsequently to the operation, the wound had entirely healed. He could walk, with the aid of a stick and a high-heeled shoe, the knee being supported by a leather case. "After the lapse of six years," continues Mr. Pemberton, "I place on record the following condition of this patient's limb: He had grown in height, but was still diminutive for his age. The lower limbs presented a wonderful contrast in appearance. The one was strong, with the muscles, bones, and joints well defined; the other, feeble and blighted. The sound limb, from the anterior superior spinous process of the ilium to the outer malleolus, measured thirty-four inches; the one subjected to operation, twenty-five!—thus showing a difference of nine inches. The femur, tibia, and fibula of the limb were not larger than the same bones in a child of ten or twelve."

In confirmation of this important fact, I may quote the authority of Mr. Syme. "I tried the operation," he remarks, "nearly twenty years ago on a boy, who perfectly recovered from it, and seemed at first to possess a limb little inferior to its fellow, except in so far as it was stiff at the knee. But in the course of time it was found that the growth of the two limbs was not equal, and that the one which had been the subject of operation gradually diminished in respective length, until it wanted several inches of reaching the ground when the patient stood erect."

In 1858 I saw a similar case of withering of the limb after excision, in a boy in whose history I could not understand the necessity of this operation, and in whom a favourable issue was reported in some of the journals.

When inflammatory disease of a joint, whether acute or chronic, has subsided, it often leaves the limb in a state of anchylosis—that is to say, the leg is bent on the thigh at an inclination more or less marked; the tibia and fibula are retracted and generally rotated outwards, so that the condyles of the femur, and especially the internal, present an under projection.

It is in this condition of the limb that we are called upon to make use of orthopœdic appliances, and it is therefore desirable to ascertain, as far as possible, the exact condition of the morbid parts and the character of the resistance.

In the first place, true bony anchylosis—that is to say, a firm osseous blending of two separate bones, is of uncommon occurrence, especially in the knee. The late Mr. Langstaff used to say that it never occurs after scrofulous disease, and I believe that his opinion is correct. I once heard a distinguished surgeon affirm that preparations of osseous anchylosis were to be found hanging "by the dozen" in every museum. That statement is quite at variance with my investigations both here and elsewhere. True osseous anchylosis, I repeat, is very rare, especially in the knee, and is in general one of the consequences after acute rheumatic arthritis. In the museum of this hospital (Ser. II., Nos. 29, 48, 55) there are a few specimens of firm bony anchylosis. In Series II. we find two of the knee and one of the hip. In the knee-joint the patella is in one case (No. 55) united by bone to the outer condyle; in the other case (No. 29) the patella is also united to the outer condyle, and "the cancellous tissues have coalesced." In Sub-series B, consisting of dried dried preparations, there are but nineteen specimens—namely, five of the hip, four of the knee (in which, again, union of the patella to the outer condyle is the prominent change), three of the sacrum and ilium, and one of the shoulder, three of the elbow, two of the carpus, and one of the finger. But these are not all cases of firm bony union. In the greater number only a light spongy osseous material, easily broken down, is thrown out, as in the specimen now before you. You find, in general, anchylosis to depend on displacement of bones combined with the formation and organization of ligamentous bands. In one specimen (Ser. II., No. 7) the opposed surfaces of the synovial membrane have become united by the organization of lymph. In other cases (No. 13) anchylosis has taken place both by adhesion of surfaces and consolidation of surrounding parts. In a third class (No. 29) the union is by false membrane and by friable bone. It is important to bear these several points in mind, that you may decide how and with what means the resistance is to be overcome.

Before proceeding to put a contracted limb straight, you must be certain that all morbid action has ceased. There must be neither heat of skin, swelling, nor pain; the disease must have run its course, during which it generally produces degeneration of the more highly organized tissues. You next ascertain the history of the case, and bear in mind that the connecting bands of adhesion are in all probability fibrous. If you doubt, make gentle extension with one hand, while you feel the flexor tendons in the ham with the other. If these tendons start up at each attempt to move the limb, you may be sure that there is some amount of movement, whatever be the apparent rigidity, and that consequently the union cannot be osseous.

Remember also, in reference to treatment, that in nearly all these cases the tissues peculiar to a joint have been destroyed or injured. You can do, therefore, little more than overcome contractions, and place bones in more favourable positions. The power of restoring motion is limited to a small class of cases—namely, those in which the loss of motion depends on thickening *external* to the joint, the cartilage and synovial membrane being in a normal state; I have little personal experience in such cases; but several have been reported by my colleague, Mr. Brodhurst, whose work "On Anchylosis" you may peruse with interest. He affirms that, in certain forms of disease, such as rheumatic synovitis, gonorrhœal rheumatism, &c., "the inflammatory product is poured out into the cellular tissue around the joint, giving rise to induration, with thickening and consolidation of the soft structures into and about which it is effused. It is not uncommon to find that the cellular and fibrous tissues around the articulation alone have suffered from the inflammatory deposit, the synovial membrane remaining unaffected and clear, and the interior of the joint free from all effects of inflammation." Acting upon these conclusions, he is in the habit in such cases of forcibly flexing and extending the joint, the patient being under the anæsthetic influence of chloroform, and of endeavouring to preserve such mobility by subsequent passive exercise. He relates many cases; but the results of my own experience are that the interior of the joint suffers far more frequently than he supposes. There are cases in which a healthy joint may be contracted by cicatrices external to the articulation: here free motion may be restored. Or a joint may become stiff from external thickening; but, in general, such thickening disappears as the morbid action ceases. I have often had recourse to forcible extension: but then only to break down some strong band of adhesion, after which I prefer to make extension gently. Motion cannot be restored when the articular surfaces have been injured.—*Lancet*, *March* 23, 1861, *p.* 283.

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## 40.—ON CONTRACTIONS AT THE SHOULDER-JOINT.

By Holmes Coote, Esq., Assistant-Surgeon to St. Bartholomew's and to the Royal Orthopædic Hospital.

[The merits of excision of the joints of the upper extremity have been overrated, and too hastily named *conservative*. They are often performed when it would have been better to abstain from surgical interference altogether. Diseases of the joints of the upper extremity are less dangerous to life, than when the lower extremity is the seat of the affection; for the patient is not of necessity confined to bed, and may avail himself of the advantages of change of air and scene.]

If you ask me what are those cases in which the operation of excision is indicated, I must reply by beginning with a process of exclusion. Certainly not in the case of young subjects, nor as a rule even amongst adults when suffering from strumous disease, or under any circumstances where the disease is recent and active, nor where the patient does not suffer much pain. There is nothing to be proud of in cutting out a joint. To use an old expression, "we mangle that which we profess to be unable to cure," and, *believe me*, the proceeding is not wholly free from danger. In July, 1857, a *man about* forty years of age came under my care in this hospital suffering

from rheumatic disease of the right shoulder-joint. About that time the surgical mind was a little upset by the glowing accounts which prevailed of the benefits which ensued from the removal of diseased articular surfaces, and I confess that mine was unduly influenced also. This patient had been a sufferer for many months; abscesses had formed round the part and burst, leaving sinuses which led down to denuded bone; the slightest movement of the joint was productive of pain. Well, I excised the head of the bone. The operation was performed in the usual way and without difficulty, and I anticipated a favourable issue; but abscesses formed along the limb, and the man died in the course of a few weeks of hectic. Upon examining the head of the bone after removal, I felt convinced that there were no morbid changes irreparable in character; and surely this man would have had a better chance had I left the diseased parts *in situ* and trusted to rest and soothing treatment.

The operation of resection may be undertaken with advantage in cases of inflammatory disease of the shoulder joint, when the local ailment seems likely to wear out the patient's strength, without offering any prospect of immediate improvement. We meet with cases in which three or four years or more pass away, the parts remaining in an unhealthy state, preventing the patient using the limb or following any profitable employment. On examination, the cancellous texture of the head of the humerus would be found soddened, discoloured and dead, though not separated from the living bone; or there might be an abscess surrounded by a cyst and hardened bone. In cases like these you may succeed in shortening the period of the patient's sufferings, but pray use caution in your selection, and proceed with care.

Now, when inflammatory disease of the shoulder-joint has run its course, and the patient is pronounced well, it generally happens that the movements between the humerus and scapula are found to be lost. These two bones move as one piece; the deltoid becomes atrophied, and the shoulder presents a flattened appearance. The question will be asked you in practice,—Can you by forcible extension rupture the adhesions, and by subsequent passive movements restore the mobility which has been lost? I have never seen a case in which a favorable result has been thus obtained; and even the late Professor Bonnet, who was a warm advocate for this sort of practice, spoke of its success as applied to the shoulder in moderate and guarded terms:—"After the disappearance of the pain which follows these trials (of forcible extension)—an event of speedy occurrence—you must proceed to artificial movements; ameliorations, which *though very limited* have been real, have ensued from this combination of treatment (*des améliorations réelles quoique très bornées.*)" Nevertheless, Mr. Brodhurst has related a case in which he affirms that much good resulted from these measures. A young woman, aged twenty-four, who had suffered from urethral rheumatism, came under his care, with the shoulder-joint stiff from extra-capsular adhesions. On the 15th of October, Mr. Gerrans and he administered chloroform and ruptured the adhesions, which were soft. The motions of the joint were instantly free, and five days later free movement was found.

It fortunately happens that where there is want of success nature compensates the patient for the loss sustained, for the scapula acquires a compensating sphere of action. You will recollect that even in the healthy joint the humerus cannot be raised beyond a right angle to a line drawn through the axis of the glenoid cavity, and the backward movement is very limited. The loss of this amount of movement after disease is easily borne by the patient. A hairdresser was once under my care whose shoulder-joint became completely and permanently stiff, but after six months he could again raise his hand so as to follow his usual employment.

In conclusion, I would call your attention to a class of cases in which there is complete paralysis and atrophy of the deltoid muscle, commonly a result of disturbance to the nervous centres during dentition. The shoulder loses its roundness; the parts become lax; ultimately the humerus falls from the scapula, and the head of the bone leaves the socket completely. This condition of parts has been observed in cases where the rest of the muscles of

the upper extremity have retained their normal power and law of development. Whenever there is loss of nerve power, atrophy or imperfect development ensues; and to this cause must be attributed some of those instances, of which I lately showed you a specimen, in which the whole upper arm is, in an adult, cold, flabby, and four inches shorter than the opposite,—while the forearm, sustained by nerve power, is of its proper size and strength. No treatment is here of avail.—*Lancet, April* 20, 1861, *p.* 381.

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## 41.—ON A PECULIAR AFFECTION OF THE KNEE-JOINT.

By Dr. Francis James Lynch, Physician to the Longhrea Workhouse and Fever Hospital.

[This form of disease of the knee-joint was first noticed by Mr. Mayo, and since by Sir Benjamin Brodie, Mr. Hawkins, Mr. Key, and Mr. Wickham. Mayo in his work on Human Pathology, alludes to it, under the heading of "A class of cases of rare occurrence."]

The disease is mostly observed in persons who have been subject to rheumatism, or to rheumatic gout, and is often brought on by exposure to damp and cold; the patient, after suffering from more or less acute synovial rheumatism of a migratory character, in the wrist, elbows, knees and other joints, will suddenly complain of intense pain in one knee, where the inflammatory action seems to concentrate itself. The limb is almost always in the extended posture, the heel resting on the bed. The swelling, even at the commencement, is considerable, occupying the knee, the lower third of the thigh, and the upper third of the leg; it is not circumscribed above or below, but gradually tapers away, and is lost in the thigh and leg. The skin covering the affected part of the limb has a remarkably glossy, bloodless hue, resembling in colour white marble. It is exquisitely tender to the touch, and the swelling is found to be uniform, firm, and elastic; sometimes a crackling is felt under the examining finger, and at times slight œdema, or pitting on pressure, exists. The joint is the seat of constant pain, sometimes dull and throbbing, at other times lancinating and severe, with frequent and intensely painful exacerbations. The tenderness is very great, even in the skin, and the least pressure cannot be borne. Moving the toes, or shaking the bed, or walking heavily across the room,—in fact, whatever occasions the slightest disturbance or movement of the limb,—causes intense agony. There does not seem to be much, if any effusion into the joint, and the usual prominences about it cannot be easily detected, owing to the thickened condition of the soft parts, and the extreme sensitiveness of the skin. Frequent spasmodic twitches in the limb increase the patient's sufferings, and interrupt the snatches of sleep, which anodynes, or exhausted nature, at times induce. From the commencement, there is a good deal of febrile excitement; nausea, anorexia, thirst, disturbed rest, quick pulse, heat of skin, and other marks of constitutional derangement, exist; the countenance is expressive of much pain and anxiety, and as the disease progresses, emaciation, increasing debility, perspirations, and frequently recurring rigors, are observed. In this condition things may remain for weeks or months, when, in the majority of cases, either from the effects of remedies, or, in rarer cases, from apparently the unaided efforts of nature, the urgent local and constitutional symptoms gradually or abruptly abate, and the patient ultimately recovers, with the joint apparently anchylosed; but in process of time a limited degree of motion sometimes returns, only to a slight amount. Ultimately, however, after a year or two, the patient is able to walk without much lameness. The swelling never completely subsides; the patella is more or less fixed; the hollows on either side of the ligamentum patellæ are permanently filled up, not from effusion into the joint, but from thickening of the exterior tissues.

Owing to the limb resting for such a length of time in the extended posture, troublesome sores and excoriations are apt to form on the heel and nates; for months after the subsidence of the acute symptoms the patient is

unable to rest on the limb, and its premature use will surely reproduce the original symptoms. Occasionally, during the acute stages of the disease, a degree of tenderness and some fulness is felt in the course of the femoral vessels as high as the groin, where an enlarged gland can be sometimes felt; showing that, in some instances, the venous and absorbent vessels participate in the existing inflammatory action. Such is the progress and termination of the disease, in its milder and more favorable forms; its leading features being well summed up by Mr. Hawkins, in the following extract from one of his clinical lectures on the subject, viz.:—"1st. Very acute tenderness of the skin, resembling, in this respect, hysterical affection of the knee. 2ndly. Great aggravation of pain on moving the limb, or the bed on which they lie. 3rdly. A pale, white, glossy appearance of the skin. 4thly. The swelling is peculiar, differing from that of simple synovial inflammation; for, while the swelling in the latter extends around the joint alone, in this disease it extends around the lower part of the thigh, and the upper part of the leg; it pits on pressure, and you can feel a sensation of crackling beneath the finger." Mr. Hawkins believes that the disease commences in the periosteum covering the lower end of the femur; and that after involving the areolar membrane external to the capsule, it passes thence to the synovial membrane and cartilages of the joint. At all events, it is probable that the inflammation originates in the areolar and fibrous tissues external to the joint. If, as occasionally happens, the disease is supposed to be rheumatic gout, or common synovial rheumatism, and is treated by colchium, and afterwards by hydriodate of potash, the patient will suffer for months from the violence of the pain, and the attending irritative fever, escaping ultimately with a joint more or less completely anchylosed, or the cartilages and synovial membrane of the joint become ulcerated and disorganized; and matter forms in and around the joint, running down the patient's strength, and either ending fatally, or rendering amputation of the limb necessary for the preservation of life. In the vast majority of cases, however, there is no tendency to the formation of matter within or around the joint; the cartilages are absorbed; and false anchylosis, without any accompanying suppuration, is established. If the disease be early recognized, rapid mercurialization of the system, and local depletion, will quickly restore the joint to its normal condition; even at a more advanced period, the same line of treatment, will in the vast majority of cases, arrest further mischief, and relieve in a very marked and satisfactory manner the local and constitutional symptoms, which begin abruptly to disappear as soon as the mercurial action is established. The painful condition of the limb pending the mercurialization of the system is often remarkably relieved by local sedative applications, amongst the most potent of which is a lotion of cyanide of potassium dissolved in water (four grains to the ounce), applied tepid. In general, warm applications are preferred to cold; and French wadding, or carded wool, around the joint and limb, with a pad under the heel, will be grateful and useful. Generally speaking, counter-irritation, by blisters or otherwise, does more harm than good, even on the decline of the disease. When the acute symptoms have wholly subsided, and the patient is able to rest a little on the limb, the Buxton waters, and a judicious course of shampooing, tend powerfully to restore a certain degree of motion to the joint. During convalescence, there are sometimes returns of severe pain, which are best relieved by the cyanide of potassium lotion, or by belladonna—two drachms of the extract in an ounce of water, smeared over the seat of pain.

In the acute stage, the stomach is so much disordered, that opium, in any shape, does more harm than good, when taken internally to relieve pain or produce sleep; but I have found Battley's sedative liquor to be, when a sedative is urgently required, the most preferable form, far more so than any of the preparations of morphia. During convalescence profuse perspirations are common; here the liquor cinchonæ, and eight grains of tannin, every night, will often check this exhausting symptom. A flannel bandage, using very moderate pressure, from the toes upwards, is useful; but bandaging should not be attempted until *all* pain and tenderness cease. When the disease

rapidly disorganizes the cartilages and other textures of the joint, or when a similar result follows a protracted attack, the case assumes the features of ordinary acute or sub-acute ulceration of the cartilages of the joint, and requires a similar treatment.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 337.

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## 42.—EXCISION OF THE ELBOW-JOINT FOR COMPOUND FRACTURES.

By Dr. George H. Porter, Surgeon to the Meath Hospital.

[Although for years, excision of the elbow-joint has been an established procedure in cases of diseased bone, compound fracture of the joint has been quite sufficient until of late to condemn the limb to amputation. The following case will prove interesting to the practical surgeon, as illustrating, in a marked manner, the benefit arising from this operation in a case of compound fracture of the joint.]

George Thompson, aged 35, by occupation a house-painter, and of very intemperate habits, was admitted into the Meath Hospital on the 7th of January, 1860. The history of the accident was briefly as follows:—

He was standing on a ladder about ten feet in height, and engaged in painting a sign-board, a drunken man passing at the time staggered against it, and upset the patient; he came to the ground on his left elbow, which received a compound fracture. When I visited him half an hour after, I found him suffering great pain, and very much collapsed; the joint on examination presented considerable swelling, was unnaturally movable in all directions, and gave the sensation as if the part were filled with a number of small pebbles. There was a transverse wound one inch in length above the joint, and towards the outside, through this, I distinctly felt the broken humerus, and on introducing a probe, considered that it passed directly into the cavity of the articulation. In consultation with my colleagues, it was decided to enlarge the wound, examine the state of the parts, and if the joint was safe, merely to remove any loose portions of bone, but if otherwise, to excise the articulation, and try to save the limb. Accordingly, having had the patient put thoroughly under the influence of chloroform, I made an incision five inches in length along the centre of the posterior aspect of the elbow, extending an equal distance above and below the articulation; this brought into view the transverse fracture of the humerus, just above the condyles; and on closer investigation I discovered a second fracture passing between the condyles into the joint. I then sawed off with a narrow-bladed saw the broken extremity of the humerus, and having made another incision an inch and a half in length from the middle of the former one towards the external condyle, severing the attachment of the triceps muscle from the olecranon. I seized the internal condyle in a strong forceps, and carefully dissected it out, all the time keeping the knife close to the bone; in like manner I removed the external condyle. My assistant now forcibly flexed the fore-arm on the arm; I then cleared the soft parts from the olecranon process of the ulna and head of the radius, and rapidly sawed them off. It is curious that throughout the entire operation I did not see the ulnar nerve; scarcely any blood was lost, two small arteries only requiring ligatures. The edges of the flaps were now brought evenly together, and retained by four points of silver-wire suture; the dressing consisted in applying wet lint to the wound, and bandaging the limb on a carefully padded rectangular splint; he was then placed in bed, felt quite cheerful after the operation, and expressed his gratitude for leaving him his arm. Ordered twenty drops of Battley's sedative liquor of opium.

March 12. Wound quite healed; the splint taken off daily for some hours, and the arm merely supported in a strong sling; by giving him a little assistance, he now had the power of flexing the forearm on the arm in a great measure, and when the limb was in the extended position, was able to pull *towards him* comparatively heavy objects. Nothing worthy of recording oc-

curred from this date until he left the hospital on the 16th of April, at which period his arm was every day becoming stronger. He returned to let me see him at the end of June, when I found him possessing a very useful limb; he had the power of flexion to a great extent, and told me he was able to use the arm on many occasions in his wonted employment.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 314.

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## 43.—ON THE TREATMENT OF CURVATURE OF THE SPINE.

By Holmes Coote, Esq., Assistant-Surgeon to St. Bartholomew's and to the Royal Orthopædic Hospital.

One of Werner's chief indications as to treatment is so important, and agrees so entirely with the views which I wish to inculcate, that I beg you to commit it to memory. It is that you must bring back the elongated ligaments to their normal length; and he also rightly adds that these ends are not to be obtained by exercises, but by steady mechanical pressure.

I have not time to speak here of arthritic or rheumatic curvatures, but proceed to the important question of treatment.

When a patient whom you imagine to be the subject of lateral curvature of the spine seeks your advice, be careful to examine the spinal column in its entire length. There may be ulceration in the lumbar region, accompanied by consecutive curvature in the dorsal region. This rule even applies to doubtful cases of wry-neck. Last year I was consulted by the friends of a young lady who was suffering from wry-neck and projection of the right shoulder. There was some talk of the division of the tendon of the sterno-mastoid muscle, a proceeding by no means free from danger. On examination, however, I found a considerable amount of ulcerative disease of the bodies of the lumbar vertebræ, and angular curvature of the spine in that region. The projection of the shoulder was consequent on this alteration of form; the wry-neck was due to irritation of the spinal cord. I corrected the deformity of the neck, which was a subject of serious annoyance, as well as I could by apparatus, but declined to interfere by surgical operation. The patient went to the seaside with proper instructions; but the accuracy of the diagnosis was shown by the fact that since the period when I last saw her she has suffered from lumbar abscess.

Having ascertained that the patient is free from any such complication, proceed in the next place to ascertain that there is no physical defect, such as inequality in the length of the lower extremities—that there is no rhachitic deviations from the proper form; inquire minutely into the patient's habits; note whether there be short sight or any disparity between the two eyes. Having arrived at the conclusion that the deformity is due to general debility, you proceed to give directions by which the elongated ligaments may be allowed to contract, the shortened ones become elongated, and the bones be pushed back to their proper relations. In very many instances this may be accomplished if the treatment be commenced early; but what hope of cure can be reasonably entertained when the vertebræ have become altered in form?

The mistake commonly made is that in incipient cases the muscles may by proper exercises rectify the impending deformity. Will muscular action cure a knock-knee or flat-foot? Why should the effect be different in one articulation as contrasted with another? What benefit can accrue from the wearing of a weight on the top of the head? The spine is already too weak to sustain the natural burden imposed on it.

You may meet with many cases in which it is said that localized movements, muscular exercises, gymnastics, and calisthenics have effected a cure; but upon minute inquiry you will generally find that, combined with these measures, which as accessories are both useful and healthful, there is usually abstinence from some habit detrimental to upright growth. Girls are kept often many hours a day cultivating an accomplishment which they give up when married, such as drawing or harp-playing. The shoulder

grows out slightly. Abstinence from the injurious occupation, healthful exercises, a few months' growth, and the slight increase in *embonpoint* may render the spinal deviation almost imperceptible. Such measures cannot remove it, but may cause it to remain stationary. What that person's future may be will depend on circumstances. Suppose that she becomes a mother, and has to nurse a family of heavy children, the evil returns, and that at a period in life when instrumental support is impossible. I have a lady under my care, aged 42, in whom this deformity has rapidly increased during the last ten years of her life. Health and spirits are quite gone. She has lost all moral courage, and while suffering severely from the sinking of the side, malformation of the chest, and visceral displacement, has not the fortitude to bear the least inconvenience which every instrument must in some measure cause. Those changes in the heart and lungs which I have already described are now taking place—namely, dilatation of the right cavities and pulmonary emphysema, and if she were called upon to exert herself for a living, she would be in danger of falling down dead.

There is but one way of correcting spinal curvature—namely, by mechanical support and direct pressure. That pressure must be unremitting, commenced early, and maintained at the proper standard for many months, perhaps for years. The inconveniences attendant on such treatment are doubtless considerable; but what have we to put in its place? Will the general health bear confinement in the prone position on a couch? And as regards localized movements, they may pull upon and even stretch contracted ligaments, but they cannot guard against their recontraction. But in nearly every case there is a stage in which a moderate amount of support will effect all that is necessary. When the spine first begins to yield, the curve being periodical as it were, and coming on only in certain postures of the body or after fatigue, the greatest possible amount of benefit may be obtained by the wearing of stays provided with lateral supports. These side-crutches, introduced so as to present no unsightliness nor demand alteration in the dress, serve to maintain the spine in the upright position. The stays, called "French stays," fix in front, but can be tightened to any degree by strings which pass from behind forwards and then round the waist. Instructions must be given that the patient should rest upon the sofa whenever the back begins to ache; and with this mechanical treatment you may combine the administration of tonics, proper out-door exercise, and well-regulated amount of calisthenics.

The more decided curvatures, however, are to be treated only by powerful apparatus, weighty and strong; if it were not so, it could exert no active influence on the spine, which would press the instrument itself back, instead of yielding before it. It is constructed on the same principle as Tavernier's belt, but is incomparably superior, the pressure being exerted by pads attached to steel rods moved with cog-wheels. The object which you have in view is to raise the shoulder which is depressed; to push as far as possible into place, by gentle yet unremitting force, the displaced bones by acting on them through the ribs, and to maintain them in their right position until the tendency to curvature has passed away. During all this time the patient may take the usual exercise and follow her accustomed avocations. But let me give this advice: Do not undertake the management of a confirmed case unless it is agreed that your attendance is to be very frequent, and extended over twelve months at least; and, moreover, do not allow the patient or the friends to take charge of the instrument. If these rules be neglected, disappointment will follow, and the blame attached to want of success will rest upon you.

The apparatus, now well known, and called by the name of the gentleman who designed it—Mr. Tamplin—may be worn by infants as well as adults. Apparatus somewhat similar have been designed by Messrs. Adams and Brodhurst.—*Lancet, March* 2, 1861, *p.* 209.

## 44.—INSTRUMENT FOR DISLOCATIONS.

Dr. WATSON exhibited an instrument, which he owed to the kindness of Dr. Keith, of Aberdeen. The instrument was invented some years ago by a student of the Aberdeen Infirmary, who following up a suggestion of Dr. Keith's, that a great desideratum in reducing dislocations was some means by which the extension force could be suddenly slacked off so as to admit of the muscles asserting their influence to rectify the displacement. This instrument most completely fulfilled the indication of "holding fast as long as was desired, and of letting go at once when required." It consisted of an ellipse of steel, which could be attached to the hook of one of the pullies of the extending tackle, by a hole in an arm projecting from and continuous with one side of the ellipse. The ellipse itself was thus composed of two pieces hinged together at one end, so that the other extremity could open or close like a pair of "*shears*" or compasses. Near the open end of the movable arm of the ellipse was a hook, which, when the two arms were in contact, caught upon a pin projecting from the side of the other arm, thus binding them firmly together; so that, unless the hook was displaced from its hold upon the pin, the ellipse of steel remained complete, and the ring, which might be placed within its curve and hooked on to the "laque," or extending straps, could not escape from its embrace. In order still further to keep all firm, and prevent any lateral displacement of the hook, a side-spring pressed it flat against the arm upon which the pin was fixed. So arranged, the instrument could hold fast against any force which was not sufficient to bend the hook or break the pin. When, however, it was desired to slip the extension force, this could be instantly effected by a small lever hinged close by the pin. Its long arm stood at right angles to the rest of the instrument so long as the extension force was applied, and its short arm, of a wedge form, worked beneath the spring and elevated it, while it pushed against the hook and disengaged it when the long arm was depressed. Thus the extending force could at once be let slip from the limb without the risk of entanglement or of a moment's delay. Dr. Watson had been fortunate enough to see Dr. Keith employ the instrument in a case of dislocation of the hip-joint. From observation, he could testify that, whether actually useful in effecting the reduction of a dislocation, by enabling the surgeon to let go the extension force suddenly, it certainly appeared to be extremely convenient as a means for *instantly* slacking away the tackle when the dislocation had been reduced.—*Edinb. Med. Journal, March*, 1861, *p.* 839.

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45.—*Osseous Anchylosis of the Elbow in a Faulty Position.* (Under the care of Mr. Henry Lee.)—We have several times drawn attention to the good results which ensue in the treatment of anchylosis of the elbow and other joints by the forcible rupture of the uniting medium, whatever that may be, provided it is not bony. Flexion of the joint is not to be thought of when osseous union has taken place. The question then arises, what is to be done in a case of anchylosed elbow in a faulty position? An example in point was recently admitted into King's College Hospital, under the care of Mr. Henry Lee, in the person of a delicate-looking girl, about seventeen years of age, who had long been a sufferer from possibly strumous disease of her right elbow, which in the course of time had become anchylosed in a straight position. There was no motion in the joint, the various bones seeming to be fused into a solid mass. Mr. Lee determined to put into practice a plan, the merits or demerits of which experience has yet to decide upon—namely, to saw through the lower end of the humerus above the joint, bend the arm, and obtain anchylosis in an improved position. Accordingly, on the 6th of October, chloroform was administered, an incision made at the back of the joint, and the lower end of the humerus exposed and sawn across by means of a circular-saw and concave blade, the latter being placed under the bone. By rotating the saw with the handle, the bone was quickly

cut through. The arm was now flexed after closure of the wound, and the patient removed to the ward.

In the event of anchylosis again occurring by the aid of callus, it seemed extremely questionable whether the girl would be in a better condition than she was previously. A false joint, it appears to us, should have been aimed at, and this result has actually happened, a useful arm being gained by the very measure which was intended to give her a stiff arm, only in a better position than it was before.—*Lancet, March* 30, 1861, *p.* 314.

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## ORGANS OF CIRCULATION.

### 46.—ON A NEW MODE OF DELIGATING THE FEMORAL ARTERY.

By George H. Porter, Esq., Surgeon to the Meath Hospital.

[The father of the author many years ago showed, that when, after ligature of the femoral artery for aneurism, gangrene occurs, it is usually occasioned by the vein also becoming obstructed by the size of the tumour.]

But he regarded a wound or injury of the vein a source of great and often unavoidable danger in the operation as usually performed, and held the principle that no proceeding could be considered as safe and eligible, when such risk was incurred. In the second number of this journal, for May, 1846, this part of the subject has been extensively discussed, and, therefore, it is needless to dwell farther on it here; but it may be observed that, if it be true that in the best hands, and with the greatest caution, not only may the vein be wounded, but that sometimes it is impossible to avoid it, and if the enclosure of a portion of that vessel within the noose of the ligature be inevitably fatal, surely the operation exposed to such contingencies cannot take a very high place amongst the resources of surgery. Actuated by these considerations, he proposed that the usual place selected for securing the femoral artery should be abandoned, and another chosen, which should at least be free from the last-named formidable objection.

After the artery has passed under Poupart's ligament, it lies quite superficial for nearly an inch of its course, accompanied by its vein, which is to its internal side, and just as superficial, and on a level with it. Any incision, therefore, by which this latter vessel could be endangered, must so far expose it to view as to render its avoidance easy, and any operation performed on this portion of the artery must be free from the objection above stated. Indeed, it is difficult to conceive how the vein could be injured, unless by the most absurd and culpable recklessness. But in this part of its course it gives off numerous branches, and the vicinity of any one, even of the smallest of these, would interfere with the formation of an internal coagulum, and with the subsequent process that ought to lead to the obliteration of the vessel. True, it does give off these branches immediately while passing under Poupart's ligament, or shortly afterwards; it gives off the circumflex ilii and epigastric, two large and important branches, quite sufficient to mar any operation, if this objection were really effective; but these branches would be above, or at the cardiac side of the ligature, and experience proves that secondary hemorrhage does not take place in such situation. Moreover, there seems no reason for supposing that these branches would be more injurious in the case of a ligature placed below them, than would be the profunda in the old operation, which was always left above the cord in order to preserve the circulation of the limb. Lastly, the common carotid artery had been tied within a quarter of an inch of the bifurcation at the arteria innominata and the subclavian at the edge of the scalenus muscle; and, assuredly, if the presence of a large branch at or near the cardiac side of a ligature would prevent the obliteration of the *vessel*, such operations as these could scarcely have proved successful. But *the femoral* artery gives off, lower down, certain small and irregular branches

—the external or superficial pudic, for instance—some of which must be below, or at the distal side of the ligature, the very situation where hemorrhage occurs, and where the presence of one ever so small may thus prove destructive. It may be so, but it has never been proved. On the contrary, where secondary bleeding has happened, the cause has always been in the ulceration of the artery at the ligature, and this, whether there was a collateral branch in the vicinity or not. There has been no lymph, no adhesive inflammation, no attempt at occlusion, although the space of an inch might intervene between the cord and the nearest branch, whereas in the many operations that have been successfully performed it seems impossible that all must have been so fortunately circumstanced as that not one should have had even a small or trivial collateral twig in the vicinity of the ligature at its distal side.

Supported by these considerations, it was suggested that the artery might be deligated safely and successfully at a point about half an inch below Poupart's ligament, where it lies superficial, where neither vein nor nerve could be exposed to danger, and where the collateral branches would be at a sufficient distance not to interfere with the subsequent process of obliteration, if such was really effective; and it was also proposed that the operation should be performed by a single incision parallel to Poupart's ligament, and of course transverse to the vessel, such incision giving the least disturbance to the artery in separating it from its adjacent connexions, and affording equal if not greater facilities for passing the cord around it, and placing the wound in the most favourable position for healing subsequently. All that was required to insure success was, that the patient should be prepared for the operation for a few days by confinement to bed, a moderate diet, and gentle aperient medicine; that he should be kept during the whole progress of the cure in a pure and wholesome atmosphere; and that, until the perfect cicatrization of the wound, he should observe the most absolute and perfect quietude. The importance of this latter condition will be made very apparent hereafter. At the time, however, these suggestions met with small encouragement. Few surgeons agree in the pathology of the arterial system, and fewer still could be found willing to leave the beaten track, and adventure their own reputation and their patient's life on an operation wanting the sanction of experience. In the meantime, a case of popliteal aneurism was cured in the Richmond Hospital by mediate compression; another occurred in Jervis-street Hospital, and soon this method of treatment came to be ratified and confirmed by a course of success so unvarying and continued, that most surgeons began to regard the operation of securing the femoral artery as little more than a matter of bygone history, never more to be resorted to in the treatment of popliteal aneurism.

But in medicine there is no proposition universally true,—no mode of treatment applicable to every possible case. Doubtless the operation of deligating the femoral artery at the usual place has totally fallen into abeyance, and probably has not been twice performed in Dublin during the last fifteen years, whilst the treatment by compression has been generally so successful, and has so entirely won the confidence of the profession, that very good and sufficient reasons would be required of any practitioner proposing to deviate from it. Such cases, nevertheless, must occasionally occur; sometimes the vessel may be so circumstanced that compression cannot be satisfactorily applied or maintained; sometimes the pain is so great (for it cannot be denied that the process is occasionally very painful) that the patient will choose any risk of operation rather than endure it; and sometimes we meet with persons so wayward and ungovernable, that no temporizing method can possibly be carried out with them. I have seen two cases in which, after compression had been tried, and failed, amputation was necessarily had recourse to, and in one of which the operation proved fatal. To meet such exceptional emergencies, the operation of taking up the femoral artery must be resorted to, and the sole question for discussion is, whether, in such eventuality, the operation I am about to describe is likely to be attended with less hazard than the old one, as performed in

Scarpa's space. Perhaps the best description I can give will be a detail of the first case in which it was performed: I extract it from my father's case-book, and give it in his own words:—

John Neile, aged 40, by occupation a farmer, and of very healthy appearance, was admitted into the Meath Hospital, October 18, 1849, with a large pulsating tumour, occupying the middle and internal part of the right thigh, extending upwards to within a hand's breadth of the groin, and downwards to within the same distance of the knee. Its measurement, both in the longitudinal and transverse directions, as far as could be ascertained, appeared to be about six inches; and the circumference of the affected limb, where the tumour was most prominent, three inches more than that of the other. Pressure on the artery above, arrested the pulsation, and caused the tumour slightly to collapse, but it still remained very soft, and its contents were evidently quite fluid. There could be no doubt of its being an aneurism, and its history showed it to be one of no trifling importance.

On the morning of the 20th, the femoral artery was secured in the following manner:—

An incision, about an inch and three-quarters in length, was made *across* the direction of the artery, at the distance of half an inch below Poupart's ligament, and exactly parallel to it; this first incision very nearly exposed the vessel sufficiently, only a few touches of the knife being required to free it from its connexions, and allow the needle to be passed easily around it. Scarcely a teaspoonful of blood was lost, and he seemed to suffer very little until the moment the ligature was tied, when he complained of a pain shooting down the entire limb to the foot; it was, however, only momentary. On the artery being secured, the wound was closed with a strap or two of adhesive plaster, the limb bandaged with a flannel roller, and the patient replaced in bed; the whole operation having occupied an incredibly short space of time.

[The ligature came away on the nineteenth day, and the patient left the hospital cured about eight weeks after admission.]

After relating another case, the author observes:—

I confess that the point of interest which has induced me to publish these cases, and to which I wish to direct the attention of the profession, has reference to the question of secondary hemorrhage, that object of terror to all operators, and which has really proved such an obstacle to success. All surgeons of the present day believe that the vicinity of a collateral branch (however small) to the ligature, vitiates the process of union by preventing the effusion of agglutinative lymph; and, as far as I know, this is the only objection that can be raised to principle of those operations I have just detailed. Mr. Erichsen, who may well be accepted as the exponent of British Surgery, in his splendid work, "The Science and Art of Surgery" (London, 1853), when treating of tying the femoral artery between the inferior edge of Poupart's ligament and the origin of the profunda, says:—"Of twelve accorded cases, in which this artery has been tied, it would appear that three only succeeded, whilst in the remaining nine instances, secondary hemorrhage occurred, which proved fatal in three, and in six was arrested by the ligature of the external iliac. This operation, I think therefore, ought to be banished from surgery; and in all those cases of aneurism that are situated above the middle of the thigh, and in which sufficient space does not intervene between the giving off of the profunda and the upper part of the sac, for the application of a ligature to the superficial femoral, the external iliac should be tied, unless compression can be employed."

Certainly it requires no little confidence to question a doctrine thus authoritatively advanced and universally received. But I must remind my readers, that up to this moment the operation on the femoral artery in this locality has always been exceptional, never adopted through choice—never resorted to unless when some strong objection existed to the selection of Scarpa's space, which objection may be admitted as adverse, more or less, *to the* success of any operative proceeding. And in the cases alluded to by *Mr. Erichsen*, as far as I have been able to analyze them, this remark is

fully borne out. Again, in the recorded cases of hemorrhage, I do not find any one instance proved to have been occasioned by the vicinity of a collateral branch; such vicinity may have existed, and probably did, but its injurious influence has in no case been demonstrated. As far as I know, destructive consecutive hemorrhage is always accompanied by the presence of unhealthy ulcerative inflammation and the absence of lymph; but I am not aware that these results have been traced to the presence of a collateral branch. It is very true, that when such branch exists, there will be no coagulum of blood within the vessel. Its absence may have originated the idea that the cause which prevented its formation also prevented the deposition of coagulating lymph, but certainly the two results can have no connexion; and that they have not is, I think, fully proved by the cases just related. There can be no question that two large collateral branches lay closely above the ligature, and that a layer of lymph was formed nevertheless. This lymph was so thin that the pulsation of the trunk of the vessel could be felt into the very wound, and so weak that it yielded and broke under the excitement of a violent cough, and permitted the escape of a considerable quantity of blood. This took place in both instances, but the lymph was adhesive and healthy, the bleeding easily controlled by slight pressure, and perfect recoveries the result. So long back as the year 1813, Mr. Travers noticed that "secondary hemorrhage sometimes results from the laceration of the young and tender cicatrix;" but, says he, "this is not to be classed amongst its natural causes." Now, if this can happen at the cardiac side of the ligature, when the young and tender cicatrix has to meet and stem the full stream of blood under the strong power of the heart's action, why may not the same happen at the distal side, or why should a collateral branch have an influence in one situation which it clearly does not possess in the other? The answer probably is, that the cause of the hemorrhage is not in the presence of the collateral branch, but in the kind of inflammation set up in the vessel; it is plastic, where it is to be obliterated; it is erysipelatous and ulcerative, where it bleeds. I am quite aware how little reliance can be placed on experiments on the lower animals for the elucidation of human pathology, and especially with regard to the arterial system; nevertheless, there is one circumstance that seems to bear strongly on this point under consideration. I do not believe it possible to produce secondary hemorrhage in the inferior animal. We may ligature what vessel we please, and where we please, near to or at a distance from a collateral branch, and either above or below it, and the result is always the same, the perfect closure and obliteration of the vessel. But the inflammation in the lower animal is always healthy, always plastic; and, whenever in the human subject it bears the same character, I believe it carries the same result. If this be the case, then, and it is a question of pathology which any inquirer may determine for himself, it will remove the chief, if not the only objection to an operation which is in other respects simple, comparatively safe, and easy of performance, free from pain and bleeding, and in which a wound of the vein is well nigh impossible. It is not pretended that secondary hemorrhage will not occur; to be sure it will, but not in this case more frequently than in any other, not from the cause to which it has been most frequently attributed, and certainly not in a more hopeless and irremediable form than where the artery is more deeply placed, as it is in the old operation.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 302.

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## ALIMENTARY CANAL.

### 47.—ON THE RADICAL CURE OF REDUCIBLE HERNIA.

By James Syme, Esq., Professor of Clinical Surgery in the University of Edinburgh.

[The subject was introduced at a meeting of the Medico-Chirurgical Society of Edinburgh, Professor Syme at the same time showing a patient on whom

he had operated with success. Mr. Syme considers the only objection to Wütztzer's operation to be, the complexity of the apparatus required. The plan now brought forward he considers more simple, and perhaps more effectual than Wütztzer's operation.]

Mr. Syme would first remind the Society that M. Wütztzer's operation consisted in invaginating or pressing up a piece of integument so as to cause it to occupy the inguinal canal, and in retaining it there by means of a needle until adhesions had formed, when the invaginated part acted like the cushion of a truss, and confined the bowel within the abdomen. The apparatus by which this was accomplished was very complicated. When its different parts had been put together, the instrument (smeared with an irritating ointment) was introduced up the external ring; a long needle was passed through it, so as to penetrate the invaginated integument and the parietes of the abdomen, where it was retained by means of a guard; after which, another part of the instrument was employed to compress the parts concerned. Not only was the apparatus very complicated, but it was by no means easy or certain to introduce the instrument fairly within the external ring.

Since this method had been proposed, various modifications of the apparatus had been suggested, but certainly not with the effect of simplifying it.

The plan which Mr. Syme had now to recommend was of the simplest character. Instead of a complicated apparatus for filling up the inguinal canal, the surgeon only required an elongated body of some kind, such as a piece of bougie, a piece of an œsophagus tube, a bit of wood, or even part of a wax or tallow candle; all the preparation required was, that a hole should be drilled through one end of the body chosen. The remaining apparatus consisted of a piece of strong thread and a needle, such as is used in sewing up dead bodies. The mode of application was as follows:—The string was passed through the hole in the end of the body; the needle was threaded with one end of the string, and laid with its concavity resting on the forefinger of the left hand, which was then passed up along the cord within the external ring; the needle was then turned round, so as to bring its point upwards, and passed, with an inclination to the left, through the textures, and brought out on the surface of the abdomen; the other end of the thread was then passed through the parietes in the same way; only that this time the inclination of the needle was to the right; the two threads were then pulled tight enough to draw the body up the canal, and up it must go. The piece of tube, as in M. Wütztzer's operation, was smeared with cantharides ointment, to irritate the skin and favour the formation of adhesions. The two ends of the thread were then tied together; and, to prevent any chance of cutting through the skin too fast, a bit of elastic bougie was placed below them. For three or four days, or a week, a compress was placed over the groin, and retained in position by a bandage; and this was the whole process.

Mr. Syme presented to the Society a patient on whom the above operation had recently been performed. He was a seaman, 21 years of age, and had suffered from inguinal hernia for two years. The aperture was wide, and the tendency to protrusion very strong; it was, therefore, in every way very desirable that a radical cure should be effected. The patient was admitted on the 30th of last January; the operation was performed on the 1st of February; the tube was removed on the 11th; on the 25th the patient was up and walking about the ward and had done so ever since. No truss had been worn till this evening; but, as the patient had some distance to come, Mr. Syme had thought it advisable to use the precaution of having one applied. The members of the society might satisfy themselves that there was now no tendency to protrusion, no impulse on coughing, and that the parts concerned felt firm and indurated. The object of the operation had, in short, been perfectly attained, and the finger could be introduced nearly to its whole length up the invaginated portion which still retained its place. How far the cure in such cases would be permanent, Mr. Syme was not prepared to say; M. Wütztzer must be responsible for the permanency of the result. What Mr. Syme had done was to facilitate the operative procedure, and to attain M. *Wütztzer's* object in the simplest way, and with means which every surgeon

had in his possession. An objection had been made to the operation on the ground that it excited too much irritation; but this very irritation was useful in exciting such an amount of adhesive action as was required.

Mr. Syme added, that the above was not the first case in which he had operated; but that he had done so repeatedly, and in no case with any bad effect.—*Edinb. Med. Journal, April*, 1861, *p.* 865.

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## 48.—ON WUTZER'S OPERATION FOR THE RADICAL CURE OF REDUCIBLE INGUINAL HERNIA.

By T. Spencer Wells, Esq., Surgeon to the Samaritan Hospital.

[Several surgeons who have tried Wutzer's operation lately, complain that though for a time the cure seems complete, yet that after a short period the hernia again descends; in fact that the operation is quite a failure. Mr. Spencer Wells first introduced Wutzer's operation to the notice of the profession in England, and he again desires to reiterate his opinion that in suitable cases it is simple, safe, and perfectly successful. Want of success is due to certain well ascertained causes which may be classed under the following heads.]

I. The performance of the operation in cases for which it is not suitable.

II. Imperfect performance.

III. Want of precaution in after-treatment.

I. In my first paper on this operation, published in the Medico-Chirurgical Transactions of 1854, I expressly limited the class of cases in which it is indicated to "strong, otherwise healthy persons, up to 40 or 45 years of age, who lead a life of active bodily exercise." I said we might depend upon a radical cure in such patients "when the hernia has only acquired a moderate size, has not become adherent, and when the long diameter of the inguinal canal has not been much shortened by the continued pressure of the intestine." I showed that in other cases, where a radical cure could not be depended on, patients whose hernial tumours could not be kept up by any mechanical assistance were so much benefited that the hernia might be afterwards properly kept up by a truss. In a lecture published in the Medical Times and Gazette in January, 1858, and in a paper published in the Dublin Quarterly Journal of Medical Science in May, 1858, I offered some reason for hoping that this limitation of the operation had been too strict. Mr. Jones, of Sussex, had a successful case in a man 63 years of age. I had one in a patient, aged 59; and on the other hand, Mr. Tamplin operated successfully on a child only 2 years old, and Mr. Bickersteth, of Liverpool, on a boy of . These cases, however, did not induce me to perform or recommend the peration on children,—for in these a well-fitted truss may be hoped to lead a radical cure—nor as a rule to recommend it in very old people, other an as an auxiliary to the truss. In the paper in the Dublin Journal, after ving an account of Rothmund's experience at Munich as to relapse, I state t it "confirmed my previous impression, namely,—that in cases where canal is not larger than to admit the finger easily, the radical cure may relied on with almost absolute certainty, and that the probability of relapse reases with the size of the canal and rings. When the rings are very large the canal very short, the chief use of the operation is to make a truss tive." My subsequent experience has completely confirmed the opinion if these rules be adhered to, and the operation be performed only in cases oderately large rings, and moderately long canal—(a ring, for instance, does not admit more than two fingers, and a canal at least a clear inch gth)—success may be attained in a very large proportion of cases. If ng only admit one finger and the canal be at least an inch and a-half think success is almost certain. I am quite aware that not one in ten of hernia that come before the surgeon fulfils these conditions, and restricted application detracts very greatly from the value of Wutperation. But it does not in the least detract from its value in suitaes. The value is restricted, but not more so than its author repre-

sented, and the error is not his, but the error of those who have expected too much from it and have performed it in cases where a radical cure could not reasonably be expected from it.

II. Published reports of cases, and my own observation, convince me that the operation has often been performed imperfectly. In some cases that I have seen, the cylinder had never been within the ring at all. In others, there had not been the necessary adaption of the size of the cylinder to that of the canal. In some, no attempt had been made to produce an equable compression by the cover, which only pressed near the needle puncture, or else along one edge, making a deep indentation, or sometimes causing a slough. More than once I have seen the instrument bound tightly down by a bandage, until so much swelling of the cord and pain were produced that the cylinder had to be withdrawn. In some promising cases, harm has been done by removing the cylinder too early; and in others by leaving it too long. All these are points of practice which have an important influence on the result, and which my papers, before alluded to, explain fully.

III. In the after treatment, the general error appears to me to have been with regard to the truss. Some surgeons look upon the use of the truss for three or four months simply as a mark of failure; others say it is injurious, as it leads to absorption of recent exudation; both, accordingly, neglect it. I believe, on the contrary, with Wutzer and Rothmund, that it is essential to permanent success. I explained in the Dublin Journal that the truss must be very elastic, "with a weak spring, and large, well-stuffed pad. If the pressure be too great, or the pad too small, absorption of the plug may take place, and relapse of the hernia follow. If no truss be worn, the adhesions which are still soft and yielding might give way." I have operated with complete success on three patients who had previously been operated on by other surgeons; one of them had never worn a truss, nor had the least precaution been taken after removal of the instrument. In another case a very strong truss, with a conical pad, which the patient had worn before the operation, was used after it. In the third case it was quite evident that the cylinder never could have been within the canal. But I must refer those interested in the subject to my lecture and former papers for these details.

The attempts to effect a radical cure of hernia in England have recently undergone two of the usual changes in the history of all improvements. When first made known here, Wutzer's operation was received with indifference or distrust. Then, as these feelings were overcome, a period followed of exaggerated expectation or confidence. Disappointment was the natural consequence; and a variety of new methods have since been tried, all more or less dangerous, some of them having actually led to death, and others to very serious illness. Five years ago it was difficult to convince an English surgeon that any operation for the radical cure of hernia was justifiable in any case. Now it seems to be the fashion to experiment on every case of hernia. Either extreme is equally irrational; but the latter is by far the more dangerous. I have seen cases operated on where I am quite sure that had the surgeon himself been the patient, he would have infinitely preferred to trust to a truss; and when there was nothing in the occupation of the patient which would lead to the conclusion that he ought to be treated upon different principles to his surgeon. It is a sound principle of surgery that it is wrong to perform any operation which is at all likely to endanger life merely to save the patient from inconvenience or remote danger. Now, some of the operations recently performed are really dangerous operations; they have actually been followed by death; although the number of cases is not yet very large. But not one of the surgeons who have performed Wutzer's operation has seen any dangerous symptom, still less death, produced by it. It has been perfectly and permanently successful in the hands of many surgeons — and must continue to sustain this character if carefully performed in suitable cases. What now remains to be done is to find some equally safe operation for those cases of wide rings and short canal for which Wutzer's operation is not suitable. This will probably be done by some simplification of Mr. Wood's operation, substituting, as he has recently done, wire for the twine

he used in his earlier cases. I have made trial, 1, of Schuh's method; 2, of simply passing a wire seton through the canal; and, 3, by fixing the invaginated scrotum within the external ring by one of the large pins sold in the shops as the "safety shawl pin," passed through the plug and both pillars of the ring, and fastened over a compress of linen held in its place by the fastening of the pin. When sufficient time has elapsed to enable me to speak of the results with confidence, I hope to be able to bring them before the profession. But I do not feel disposed to imitate those gentlemen who object to *Wutzer's* operation because relapse has occured several months after its performance; and then bring forward cases as *successful* a few weeks only after *other* operations have been performed.

In conclusion I may allude to the curious fact that I have been consulted in cases where the operator believed that this operation had failed, because the cutaneous portion of the scrotal plug had descended to its former position, whereas I have been enabled to gratify both surgeon and patient by the assurance that the cases were completely successful, the fascial portion of the plug completely occluding the canal, and effectually preventing any protrusion.—*Med. Times and Gazette, Jan.* 19, 1861, *p.* 58.

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## 49.—ON THE RADICAL CU E OF HERNIA.

By Dr. James Morton, Surgeon to the Glasgow Royal Infirmary.

[The patient was a labourer, admitted for another complaint. He was found to have a very large scrotal hernia on the right side. It was oblique and reducible. The inguinal canal very much distended, and straighter than natural. He complained of great inconvenience from the hernia, and operation was determined upon. On August 4, a metallic ligature was passed subcutaneously around the sac, at the lower part of the inguinal canal, excluding that part only which lay behind the cord, and fixed to a leaden plate by a nipple. A compress and bandage were then applied, and the patient was directed to keep the recumbent position. A grain of opium was given. The patient was discharged on the 20th August, the intestine still filling the inguinal canal when he coughs, but not passing any further, and only coming so far when he coughs much. Next month he reported that he had not enjoyed so much comfort for years.]

The operation performed in this case was as follows:—The contents of the hernial sac being returned, the patient, in the recumbent position, was put under chloroform; the sac was then grasped by the finger and thumb, and slightly raised; a handled needle, with the eye near its point, and furnished with a wire ligature, was introduced under the skin, and guided round the sac so as to include the greater part of the sac, if not the whole, care being taken not to include the cord; and the end of the needle being brought out as near to the point of its entrance as possible, the ligature being caught and pulled out, the needle is withdrawn.

It is plain that the needle cannot be brought out precisely where it was first inserted; and, in consequence, it is requisite to pass the end or ends of the ligature (for in this case I used a double one) again into the eye of the needle, and pass it under the skin so as to get the ends of the ligatures brought out at the same point, and there they may be tied. To give a fixed point, as well as to prevent the drawing inwards of the ligatures, I used in the above case a very small metal plate with a single nipple, over which the ligature used was fastened. I have said that I used a double ligature; and for this reason chiefly, that, in event of one being rendered unserviceable by the formation of a *kink*, I might use the other without being compelled again to use the needle. Besides, it occurred to me that the additional irritation produced by the second wire would not be injurious, but the reverse, and possibly not more than would be required for agglutination of the distended parts. Care must be taken that the ligature does not include the cord or vessels; and during the operation this can be ascertained by gently

pulling the wire when the included parts are raised, and the cord can be felt below. A few days after its application, the ligature is removed. A little water dressing, or one equally simple, may be employed.

The advantages of this mode of operating are, chiefly, its simplicity, it causing little or no suffering, its effectually closing the hernial sac and causing its opposite sides to adhere, and that comparative freedom from dangerous or disagreeable consequences which is well known to accompany subcutaneous operations, and even other lesions of various parts, when the skin remains sound, or nearly so. It commended itself to me by its apparent safety, and this has been verified by at least one case. In the aged it must be difficult to procure adhesion by pressure; whereas ligature would be certain to do so, and the risk of peritonitis almost infinitesimal.

The objections which can be urged against the more common modes of operating induced me to adopt the above procedure. These objections we may shortly state. There is, first, the employment of a complicated instrument, which is always an objection so far as the operator is concerned; secondly, this instrument causes a considerable amount of very severe suffering, and in some cases gangrene of the parts has resulted; and, thirdly, when using this instrument there is always an invagination of skin and sac, and this is said to be liable to be pulled away from the inguinal ring by its own weight or other connections when the instrument sustaining it is withdrawn. It is right to add, however, that operations with such instruments have been frequently successful; and that, so far as known, scarcely a fatal case has occurred. At least one case is known to me in which the patient was considerably worse after than before the operation; and some statements regarding their success on the continent have been questioned by British authors. The operation which I performed in the case now detailed may not be quite so safe in cases of femoral hernia as I conceive it to be in inguinal cases. In the former, the femoral vein is so closely applied to the hernial sac at its point of exit from the abdomen, that there may be some risk of wounding the vein in attempting to pass a sharp-pointed needle around the hernial sac.

Since the time when I operated on the above case, it has occurred to me that in some cases, especially in such as presented a large inguinal opening, the following plan might be attended with some advantages, viz., to make a valvular opening in the skin, and use one or more staple sutures. To make this more plain, let me state more fully what is meant by it:—An incision may be made through the skin a little under the abdominal ring, or point of exit of the bowel; the skin could then be easily drawn up so far as to enable the surgeon to reach the sac at the ring, when he might pass one or more staple sutures, according to the size of the opening; the free ends of such sutures lying downwards, and coming out by the original incision in the integument; these ends could either be tied close to the sac, or fastened externally to a plate of lead, and removed when they had served the purpose of the operator. Should an opportunity offer, I mean to put this proposal into practice. It does not seem to me much more hazardous than the one which I have already tried; and it is worthy of remark, that if the attempts which have been made towards the radical cure of hernia have established anything it is this, that the peritoneum, one of the most important of the serous membranes, can be perforated, at least in its diverticula or continuations, with almost unvarying impunity.

It will be perceived that the preceding case is not brought forward as a perfect cure, though such it may yet prove to be. The ligature, as thus applied, does not close the ring, but the sac; and the patient will require a truss for a time. The operation, however, seems to be as safe as it is simple (two most important recommendations), and it very materially improves the condition of the patient.—*Edinb. Med. Journal, Dec.* 1860, *p.* 515.

## 50.—SOME SUGGESTIONS FOR AN IMPROVED PRACTICE IN STRANGULATED HERNIA.

By T. Bryant, Esq., Assistant-Surgeon to Guy's Hospital.

[Mr. Bryant shows how few cases of femoral hernia are reduced by the taxis as compared with inguinal, and how frequently a recently strangulated inguinal hernia may be reduced when the patient has been brought completely under the influence of chloroform.]

Having shown the benefit which the use of chloroform has already conferred on patients suffering from strangulated hernia, the author proceeded to consider how far the recognition of the truth should influence our practice, and in what way we can turn the information to account. The author stated, that to his mind, the lesson to be learnt was not a difficult one; for if it be true that we possess in chloroform the most efficient means for producing complete relaxation of the muscular system, and with it, of the parts involved in hernia, and if it be acknowledged that a patient can be more speedily and effectually brought into that relaxed and desirable condition by such a method than by any other which we have been in the habit of employing, there was but one conclusion to which he could arrive, and that is, that we should, in all cases, employ at once the most certain and most efficacious practice, and neither waste time by delay, nor prostrate the powers of our patient by the use of other remedies, when we possess one so incomparably their superior. He believed that by the use of chloroform the ends which we have been in the habit of seeking by other means can be more speedily attained, and with greater certainty; that delay is not experienced, and durable depressing influences are not excited; and that in the majority of cases of strangulated hernia, the administration of chloroform should be primarily selected.

In quite recent hernia he believed that the warm bath and full doses of opium, followed by the gentle application of the taxis, may be expected to succeed, and it was in such, and in such alone, that he advised their use. But he added that such cases in hospital practice were unfortunately rare, and it was too true that four, five, six, seven, and even ten days, were the average periods of strangulation of the cases admitted into Guy's Hospital.

The author stated, that when the patient had been completely brought under the influence of chloroform, the force which is required to reduce the hernia was simply nominal; anything like violence must prove injurious, and would be unwarrantable. The most moderate application of manual pressure was sufficient if success was to be obtained; and if it should fail, no extra force will be more beneficial: if success is to follow its application, moderation will suffice, and if failure, force will not succeed. If failure ensues, herniotomy should be at once performed; no other means are left from which to select. The most simple and efficient means have been employed, and have failed, and an operation alone remains.

Mr. Bryant then dwelt on the evils of prolonged or forcible taxis, and of delay, as caused by the use of other means, and stated that he believed it would be better for all cases of strangulated hernia to be treated at once by the administration of chloroform, followed by the gentle taxis; and on that failing, by herniotomy, than that any risk of delay should be occasioned by the use of remedies, or of injury to the strangulated bowel by the continued and repeated applications of the taxis. He believed that by such a practice the cases of death from strangulated hernia would be but rare, and the cases of maltreatment would be less numerous.—*Lancet, March* 30, 1861, *p.* 316.

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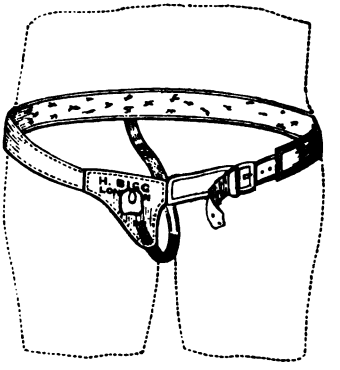
## 51.—NEW TRUSS FOR HERNIA.

[There are many undoubted disadvantages arising from the use of a common steel spring truss. Hence many attempts have been made at producing a

bandage which shall fulfil the required mechanical conditions, without involving the necessity for a metallic band.]

Every surgeon is aware that in reducing a rupture, force has to be employed in an upward, backward, and outward direction; it is therefore evident that any appliance which fulfils the same office as the hand of the surgeon should have these three directions of mechanical force so combined, that their resultant follows the same line as the hernia itself pursues in descending. These desiderata are accomplished by a truss which has been recently invented by Mr. Heather Bigg, a diagram of which will explain its external form.

The truss has been called a "Triple Lever Truss," on account of there being three small levers, having their anterior extremities concealed within a triangular pad, accurately fitted to the inguinal region of the abdomen. These levers are acted upon by turning a little button in the middle of the pad, when the pressure of the truss can be regulated in such a manner as to offer the exact amount of resistance required to support the hernia.



The advantage of this arrangement is highly important both to the surgeon and the patient, as it enables the pressure to be determined with scientific exactitude, and moreover admits of the same truss being at once adjusted either for violent exercise or the slightest exertion. It thus presents the peculiar condition of the same truss being equally applicable for the slightest as well as the most severe cases of hernia.

It should also be stated that, instead of a steel spring surrounding the body, a soft padded band is employed, so that the truss can be worn either night or day, and under no circumstances whatever become in the least degree displaced. The levers not bring rigidly connected are always in a state of equipoise with regard to each other, so that the pressure of the pad remains uninfluenced by any movement of the body.—*Lancet, Dec.* 1, 1860, *p.* 542.

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52.—*Strangulated Hernia: a New Operation.* By Dr. JOHN NIVEN, Edinburgh.—[The writer considers the operation proposed most likely to be useful in the case of small and recent protrusions where there is little chance of adhesions having been formed.]

Every surgeon must have experienced the difficulty of reducing such herniæ by the taxis, and must also be aware of the exceeding ease with which a force acting from within, such as the peristaltic action of the bowels, effects their return. Many years ago I had occasion to perform the post-mortem examination of the body of a female who died from an unreduced femoral hernia of this character, and was astonished to find that the slightest touch of the finger caused the protruded bowel to slip into its place before I had an opportunity of demonstrating it to the bystanders.

The operation I propose is this:—An incision is to be made in a vertical

direction, about an inch or an inch and a half above the neck of the sac, dividing the skin of the abdomen, and gradually diminishing in extent till the peritoneum is reached. The peritoneum is then to be opened to the extent of admitting a blunt hook, or, what is perhaps better, a fine curved pair of forceps, with which the neck of the protruded intestine is to be grasped, and gently drawn upwards, whilst the taxis is applied from the outside to assist. The wound is then to be closed with a harelip pin or a wire suture.

Should this proceeding fail from any cause to effect reduction, it is open to the surgeon either to enlarge the wound, so as to admit the finger and to divide the stricture from the inside, or to perform the usual operation.

The advantages of this operation are the small extent of the incision into the peritoneal cavity, and that in a sound place; and the avoidance of the exposure of the protruded bowel to the fingering required in the usual method. In fact, the danger would not be greater than the operation of paracentesis.

[Dr. Niven forgets that the relaxation of the muscular fibres after death would account for the easy reduction of the gut in the post-mortem—the same would not be the case during life. Moreover, the handling of the gut with forceps is a proceeding not without much risk.—EDS.]—*Lancet, March* 16, 1861, *p.* 276.

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53.—*A Case of Strangulated Oblique Inguinal Hernia treated by inverting the Patient.* By Dr. H. POWER.—[The patient who was 44 years of age had suffered from hernia for twenty years, for which he had always worn a truss. The hernia came down in spite of the truss during a violent exertion, and resisted all his own efforts at reduction.]

On examination a very tense tumour was found in the right inguinal region. It was about equal in size to a guinea-fowl's egg, and was extremely tender to the touch. I could not ascertain whether the hernia was direct or oblique. He complained of nausea, and of pain radiating over the whole abdomen.

I placed him on his back, with the knees drawn up, and for five minutes endeavored to reduce the hernia by steady pressure, but no impression whatever was made upon it. Recollecting the plan which was rediscovered or reintroduced by my friend Mr. Jessop, of Cheltenham, and of which several successful instances are on record, I obtained the assistance of one or two of the students and placed the patient on his head. On again gently compressing the tumour, I had the satisfaction of feeling it quickly recede, and in less than a minute it entirely returned with an audible gurgle.—*Lancet, March* 23, 1861, *p.* 287.

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54.—*On the Removal of Hemorrhoids by the Ecraseur.*—The first surgeon in Paris who drew attention to the occasional occurrence of traumatic stricture of the rectum, as a consequence of the use of the écraseur, was M. Nélaton, of the Hôpital de la Clinique. This practitioner, in a clinical lecture last spring, noticed this drawback to the use of M. Chassaignac's favourite instrument, and said that many of the patients who had been operated on had had reason to regret the exchange of a bearable complaint for a veritable torment. M. Chassaignac, who is not the man to abandon a cause he had once declared a good one, at once set about a modification of his original plan, and now, instead of including the whole mass of the hemorrhoidal tumour within the loop of his instrument, and removing it as if it were a polypoid growth, he contents himself with the excision of the crown of the tumour, trusting to the subsequent inflammation and cicatrical contraction to effect the rest of the process of obliteration requisite for the accomplishment of the cure. This new method, however, does not give the patient the benefit of one of the great advantages attached to the use of the écraseur under ordinary circumstances—namely, the perfect immunity from hemorrhage, as proved by the occurrence at the Hôpital Lariboisière of a death from the

above cause after an operation for the extirpation of piles, although, as very justly observes one of the leading journals here, the Journal of Practical Medicine and Surgery, it has yet to be proved whether in the statistical reports of such operations, either by écraseur or other method, fatal terminations be not occasionally observed also. M. Chassaignac's error has hitherto undoubtedly consisted in too generous an excision of healthy tissue—a proceeding which has been followed by the formation of a large and progressively contracting cicatrix. The alarm has now been pretty widely given in France at least, and operators will for the future be more economical in their dealings with the mucous membrane of the rectum. If M. Chassaignac possesses any imitators in England, it will be well for them to take warning.—*Lancet Correspondent.—Dublin Hospital Gazette, Dec.* 15, 1860, *p.* 375.

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## 55.—ON PROLAPSE OF THE RECTUM.

By Henry Smith, Esq., F.R.C.S.

[There has been some divergence of opinion amongst surgeons, as to whether most frequently the prolapsed part consists of the mucous and muscular coats of the rectum, or of the mucous coat only. Mr. Smith, from a careful examination of the parts, both during life and after death, considers that in the majority of cases the mucous membrane simply is implicated, but that, in a few, the muscular coat likewise is protruded. The treatment suitable to the former will not cure the latter. The health of the child, and especially the power of the muscular system, must be improved by the employment of nutritious diet and powerful tonics.]

The other instances in which we find that there is protrusion of the entire structure of the bowel are seen in those cases of long-standing prolapsus in adults, where the protrusion is of immense size, as large as the fist or a fœtal head, and coming down on the least exertion of the patient. Within the last week, I have been consulted on a case of this description, occurring in the person of an old gentleman, of a weak frame and feeble circulation. The prolapsus had existed for forty years, and it has reached its present enormous size in consequence of neglect of proper surgical treatment. In such cases as this, there is no doubt that at first, the mucous membrane of the bowel was simply protruded, but afterwards the other tissues became involved, and at length the tumour consisted not only of the thickened mucous membrane, but the muscular coat also was extended beyond the sphincter.

In those cases of prolapsus of the rectum of much more frequent occurrence, where the disease is more limited in extent, and where the mucous membrane alone is protruded, there is a considerable difference in the pathological features, and that too of considerable practical importance, especially when viewed in relation to a mode of practice I am in the habit of adopting. In some of these cases it will be found that the mucous membrane is simply extended beyond the sphincter, in but a very slightly altered condition, the whole circumference of the lining membrane of the bowel may be down, or only one or two semicircular folds may be prolapsed; in other instances, however, besides this prolapsed membrane, the protruded part may consist of the muco-cutaneous lining of the sphincter, in a highly congested and thickened condition, forming in fact the greater portion of the disease. This part is protruded first, and may be seen as a dark blue ring around the anus, while situated above it is the proper mucous tissue of the bowel simply relaxed and prolapsed, but otherwise in a normal state.

There is a point of considerable importance, both pathological and practical, in connexion with prolapsus, which has not been sufficiently alluded to by writers, this is the condition of the sphincter ani. In some cases we shall find that this muscle acts in a normal manner, and that the anal aperture is not larger than ordinary, although there may be a considerable prolapsus of the mucous membrane; in other instances the sphincter seems to *have lost* a considerable degree of its contractile power, the aperture is en-

larged and easily distended; in a few cases to such an extent that the whole fingers, when formed into a cone, may be passed into the rectum. In these instances this laxity of the sphincter is the chief cause of misery, for when it exists in a great degree the patient loses, either partially or entirely, control over his rectum, and the fæces escape involuntarily. A remarkable instance of this fell under my care in the person of a patient, aged 70, who had suffered for twenty years with prolapsus, and indeed it was this circumstance which drove him to consult me. When there is a partial loss of the power of the sphincter the patient is continually harassed by calls to the closet night and day, although there may not be any actual involuntary discharge of fæces. Of course this loss of power of the sphincter is the greater misfortune of the two, but in some instances of prolapsus of the mucous membrane when the sphincter is in a healthy condition, the following accident may and does occur, especially when the protruded membrane has on its surface one or more distinct hemorrhodial tumours. the protrusion occurs on one occasion to a larger extent and the patient cannot return it as usual, the most severe symptoms of course rapidly set in, and although this accident is very likely to be followed by a cure in consequence of sloughing of the constricted parts, yet one would be very unwilling to bring about this condition purposely, for death has followed upon the intense amount of inflammation which has occurred. Not long since I was called to an old lady, 70 years of age, to whom this accident happened, and not being in a healthy condition she was reduced to a great amount of suffering, for violent inflammation and sloughing of the protruded membrane had taken place; this latter process was hastened by placing ligatures around the diseased parts, and she made a good recovery.

The treatment which should be adopted in cases of prolapsus of the rectum must differ according to the pathological condition of the part, especially as regards size and the state of the sphincter. It has hitherto been customary among surgeons to use the ligature in most of the cases of prolapsus requiring surgical operation, and of these I am only now talking; and undoubtedly, where the disease has become very extensive, and particularly when associated with distinct hemorrhoidal tumours, the ligature must be used, if there be not any contra-indication to a surgical operation. This is especially the case when, from the large size and the peculiar feel of the tumour, there is every reason to believe that the muscular coat of the bowel is protruded as well, for any operation short of the ligature will be useless in removing the disorder. In some of the cases also alluded to, when the prolapsus is voluminous, and there is a very relaxed state of the sphincter, the ligature alone can be depended upon; but for such instances, which are by no means uncommonly met with in old people of the middle and upper classes, I have lately put in practice an operation which I do not wish to claim as particularly new, for it is a combination of two agencies employed before for similar conditions, but which I particularly wish to bring before your notice. It consists in first applying the strong nitric acid, on one or more occasions, to the mucous membrane; and subsequently when this agent has had some decided effect, to remove with curved scissors narrow strips of skin and mucous membrane from around the verge of the anus at right angles to the orifice. The latter remedy alone was employed both by Hey and Dupuytren, and lately recommended by Syme; but my experience tells me that alone it is not to be depended upon; but if the mucous membrane, which is always in such cases in an extra-vascular and relaxed condition, is first brought into a more healthy state by the contracting and slightly escharotic powers of an agent like nitric acid, the effect of removing the loose folds of skin which are so generally associated with the relaxed state of the sphincter, is very admirable. Two objects, in such instances, are sought by the surgeons, and indeed are absolutely necessary for an efficient remedy—viz., the contraction of the mucous membrane, and the bracing up of the sphincter; these two results are brought about by the combined proceedings mentioned. *I must, however,* not omit to state that it is perfectly useless *to employ the nitric acid in* those instances where the prolapsed mu-

cous membrane has become thickened and indurated, as is very often the case; the agent will produce no good effect; both patient and surgeon will be disappointed. The application of the acid is more especially advisable in those cases where the mucous membrane is granular, very vascular, and readily bleeds; the effect of one application in such an instance is sometimes really astonishing. But there is one caution I wish to impress; and that is, that the surgeon must not be misled into abandonment of further measures because after one application of the nitric acid he finds that the bleeding and prolapsed bowel suddenly disappears. It will sometimes happen that one application will be followed by remarkably good results like these, and that afterwards the symptoms return. It is better, in cases of extensive prolapsus, that the acid should act gradually than suddenly; the effect will be more permanent. It will be necessary to apply the nitric acid when the disease is extensive, on several occasions, perhaps four, six, or eight times; but it is generally attended with so little pain, that the patient does not object to submit to it. If the sphincter ani be not in a weak condition, but acts normally, there may not be any necessity of cutting away the thin slips of muco-cutaneous covering; but if there are any pendulous flaps of integument, these should be excised. These operations, which should be effected by sharp curved scissors, are of course painful; but the application of the freezing mixture of ice and salt will much deaden the pain.

This treatment may be considered as perfectly free from danger, and is admirably adapted for those cases where the patient will either not submit to the ligature, or where there is some contra-indication to this proceeding. For instance, many of the worst cases of prolapsus occur in aged people who are, or who consider themselves, too old to undergo the ligature; others have some symptoms of lurking organic disease about their brain or heart, and it would be highly perilous to use the ligature, but the treatment I advocate may be used with perfect assurance of safety. To illustrate this important fact I will allude to two cases which have lately been under my notice. The first was a gentleman aged 73. He had a bad prolapsus, and when told by me that I could only recommend the ligature with confidence, to destroy his disease, he refused to undergo it. I tried by some applications of nitric acid to remedy it, but he became dissatisfied and consulted a surgeon of great eminence who strongly recommended the ligature and applied it; the patient died three or four days after from an attack of apoplexy. Now there were certain indications about this gentleman which would have prevented a surgeon who knew them and carefully considered them, as I had done, from frequent observation of his case, from performing this operation. The patient was very peculiar in his manner and habits, and thought by his friends to be very "strange," as the term goes, and in addition to this he had almost entirely lost the control over his bladder during the last few months of his life, without the existence of any stricture or disease of the prostate. Now these two facts indicated some lurking mischief about the nervous system, and should have prevented the surgeon from operating. No doubt the stimulus of the operation of the ligature, which is much more severe than is imagined, lit up the lurking mischief in his nervous system and destroyed life. To this the eminent surgeon who performed the operation readily assented when he was questioned by me.

The other instance is that of an old military man, nearly 70, who has had prolapsus with severe pain and bleeding. He was anxious to get some relief; he had a peculiar nervous twitching about his face, and a feebleness of the lower limbs; and on making inquiry of one of his family, I ascertained that he had had something approaching a fit on two occasions. I at once decided against employing the ligature, for this reason, and resorted to the employment of the treatment I have advised with great benefit.

It is not to be supposed from the remarks I have made that I am averse to the ligature in suitable cases, but if it can be dispensed with, and a milder mode of treatment can be successfully adopted, the surgeon is bound to put it in force. For although I believe, when properly performed and in healthy subjects, the operation of ligaturing portions of the mucous membrane of the

rectum is by no means dangerous, still we cannot conscientiously tell a patient there is no risk; independent of the peculiar danger attending the proceeding, such as *pyæmia* or *tetanus*, some serious and annoying accidents are liable to follow this operation, and I shall here draw attention to some of these.

One very peculiar and unlooked-for sequence of this operation for prolapsus worthy of relation occurred in practice not long since. I operated upon a fat old lady who had not much stamina,—the circumstances of the case were such as to demand a speedy and efficient operation; I used the ordinary precautions in the process, and the patient did very well the first day or two. On the third day, however, to the great surprise of her medical attendant and myself, a severe hemorrhage suddenly took place from the part, and had such an effect on the patient that it made me very anxious. I was at a loss to account for this very unusual occurrence, where neither the knife or scissors had been used; but on going to make an examination I found that there had been a rapid slough as large as a shilling formed by the side of the rectum laying bare the muscular coat of the bowel for near an inch in extent, and no doubt one of the inferior hemorrhoidal arteries had been opened up and hence the bleeding which was so profuse and which occurred on a second occasion; but fortunately by keeping up pressure, and by the local application of strong nitric acid, the sloughing process was stopped and the patient made a good recovery. But this might have destroyed the patient had it gone on further. The cause of the sloughing was this:—The patient, who was very fat and heavy, lay a great deal on her back after the operation, and the pressure of the bed induced the sloughing of the part already of necessity irritated by the close proximity to the ligature. We learn from this interesting case the importance of not allowing patients to lie much upon the back after this operation, they should rather be induced to lie on either side.

There is another point connected with the operation of the ligature of the mucous membrane of the rectum which calls for attention, and about which there is considerable divergence of opinion. I refer to the supposed danger of suddenly arresting the great discharges which are so frequently seen attending on these complaints. I believe that, in some instances, the sudden arrest of bleeding, or of a very profuse muco-purulent discharge by the ligature, is liable to be followed by fatal consequences, although many erroneous notions have been promulgated about this. In very robust or full-blooded persons, the sudden stoppage of the discharges may predispose to, or bring on, some internal congestions of the brain or lungs; and therefore, in such, one should be careful about employing the ligature. I have never had good cause, in my own practice, to suppose that any serious result of this kind has occurred, but I will briefly detail one case which, if the circumstances had not been accurately investigated, would have led one to the supposition that the operation produced a disastrous result of this kind.

In March last one of the finest-looking officers in the army, of large frame, in robust, ruddy health, and aged fifty, consulted me for prolapsus and hemorrhoids of a severe nature, which had existed for many years. He was a cavalry officer, and had seen arduous service in various campaigns, having to ride much; and for many years, on such occasions, the hemorrhage was very profuse; but—*and this is important*—there had been scarcely any bleeding for the last two years. I advised the ligature, which operation was also strongly recommended by Mr. Ferguson, who, at my request, was consulted. I performed the operation, which was followed by no untoward symptoms, and in the course of seven or eight days, the ligatures had separated, and I left the patient under the care of his medical attendant, Mr. Coleman, of Kingston. The patient was out in a fortnight, but did not regain his strength so readily as is the case after this operation; and one night, about three weeks afterwards, he suddenly dropped down dead. Now, at first sight, one would be inclined to say that here was the very case to prove the danger of suddenly stopping the discharge from the rectum. And had I not investigated the point, I should certainly have supposed that a sudden congestion of the brain had been caused by the cessation of the dis-

charge; but the fact of there having been scarcely any bleeding during the last two years, militates against the doctrine. Mr. Coleman, who is a very able and shrewd practitioner, considered that it was disease of the heart that destroyed life,—that the man had a feeble heart, and that the low diet to which he had been of necessity reduced, had further enfeebled it, and hence its action had suddenly ceased. It was a most unfortunate thing that no post mortem examination could be obtained.—*Med. Times and Gazette, Nov.* 17, 1860, *p.* 475.

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56.—*Salivary Fistula.* By R. G. H. BUTCHER, Esq., Dublin.—[The following short abstract shows well the mode of dealing with cases of salivary fistula. The case was one of removal of the whole upper jaw and malar bone, for the extirpation of an enormous tumour.]

On the 7th April, I opened a small abscess in the parotid region, or rather in front, over the masseter muscle. From this flowed out pus and saliva in abundance, the latter fluid continuing for some days to drain freely from it. I passed an armed probe with a few threads of silk from the external opening, through the abscess, forwards into the mouth, and tied the seton up; and in a few days, having established a channel for the saliva into the mouth, I pared the edges of the external wound, brought them together, maintained them so by two points of twisted suture, using very fine needles, and in three days the wound was healed and closed permanently, and the saliva continued to pass by the artificial canal into the mouth.—*Dublin Quarterly Journal, Feb.* 1861, *p.* 11.

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57.—*Artificial Teeth.*—Coralline (coloured vulcanized India-rubber) being now extensively employed as the basis of artificial teeth, it requires to be satisfactorily determined whether the subsulphate of mercury used for the colouring is likely in any case to produce prejudicial effects. The material being very hard and insoluble, and not capable of being much softened even in boiling water (the dentists employ steam of 230°), there seems no danger from the vermillion pigment; yet as the extent of surface in a full set of teeth is twelve or fourteen, or more, square inches, and the material, at the temperature of the human body, is acted upon day and night without ceasing by the saliva, and the gums and tissues which come in contact with it are peculiarly susceptible of mercurial influence, it may not be quite certain, without the test of experience, that no evil can arise in the case of a morbidly sensitive invalid. If any such cases had been known to occur, the dental profession would assuredly have changed the cinnabar for some other pigment. But the use of the material is recent, and it would be well to watch the testimony of facts as to whether any of the symptoms of mercury (not amounting to salivation or other marked phenomenon) can be suspected in the mouth or pharynx or alimentary canal.—*Lancet, March* 23, 1861, *p.* 300.

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## ORGANS OF URINE AND GENERATION.

### 58.—LITHOTOMY.—NEW FORM OF STAFF FOR DILATING THE PROSTATIC URETHRA.

Case under the care of JOHN WOOD, Esq., King's College Hospital.

The patient was a boy, aged 9 years. The stone was small, and the contact with the sound was somewhat obscure. In the performance of the operation, Mr. Wood introduced to the notice of the class an instrument devised by him, which combined the uses of the staff and a dilator of the prostate,—a Dilating-staff. In the subjoined wood-cut it is given on a greatly diminished scale. It consists of a staff mounted with a stout handle, and opening at the commencement of the curve into two blades, which pass into the bladder

and act on the prostatic urethra as dilators. The anterior blade (*a*) is immovable, and fixed at the end of the steel tube upon which the handle is placed, and which forms the shaft of the instrument. The posterior blade (*b*) is connected by a rod passing through the before-mentioned tube, with a small lever (*c*) projecting to the right side of the handle, and works (on the application of the thumb of the hand holding the staff) by a double motion. One part of the motion is antero-posterior—by a piston-like movement; and the other lateral—on the axis of the connecting-rod, in such a manner as to lay the blade flat upon the base of the bladder, holding it downwards towards the perineum, and preventing it from yielding before the dilating finger. At the end of the tube whence the anterior blade springs, is a slight bulbous enlargement (*d*) capable of being felt in the perineum when introduced and placed in the bulb of the urethra, just anterior to the deep perineal fascia.

Beyond this the blades open, and for the length of an inch are straight in themselves, but inclined upon the shaft at an angle of about 30°. This part is intended to lie in the membranous urethra. The blades are then curved sharply for about three inches, so as to hook well over the pubes. On the inner surface of the straight part of the anterior blade, where opposed to its fellow, is a deep groove (*e*) an inch long, which is fully exposed by the opening of the blades. Upon this groove the membranous urethra is opened to admit the finger of the operator while it is stretched tightly by the divergence of the blades. The internal or opposed surfaces of the blades open upon each other diagonally or obliquely, and are smooth, even, and flat. Between them, when opened, the dilating finger of the operator slides through the prostate into the bladder. The pin (*f*) prevents the locking of the blades by turning, when open, in the wrong direction.

*The Operation.*—The patient being placed in the usual position, but without being tied, and the staff introduced, the operator first placed the bulb of the staff accurately in the bulb of the urethra, as far down as it would pass easily. The shaft of the instrument was then exactly at right angles to the axis of the body. The assistant then opened the blades by pressing the lever from him with his thumb. The operator next placed the left forefinger upon the projection of the staff at the bulb, and the thumb upon the right tuber ischii—stretching the perineum, and sliding upwards, as far as possible, the superficial part of the bulb of the urethra. Then, with a long narrow lithotomy knife (drawn in the woodcut of a reduced size), with an inch and a-quarter of cutting edge, a lunated incision was made in the perineum, commencing about two lines to the right of the median raphé, just posterior to the bulb, at about one-third of the distance between it and the anus, and carried round the latter, terminating opposite the anterior margin, midway towards the tuber ischii, thus describing the quadrant of a circle, having the anus for its centre. This incision passes through the superficial fascia and central tendon at the anterior margin of the sphincter ani, without dividing the muscle, and avoiding the chief perineal vessels and nerves. The membranous urethra being thus exposed, it was then felt by the left forefinger and opened upon the cleft and groove in the staff a little to the left of the median line. The tense and widened condition of the urethra rendered this proceeding very easy and certain. The finger then entered the cleft between the blades of the staff. A slight touch with the knife upon the ure-

thral edge of the posterior layer of the deep fascia then enabled the finger to pass fairly between the blades into the prostatic urethra, which was slowly dilated by rotation of the finger upon its axis. The prostate gave way easily, the stone being immediately reached and removed with a small scoop. A very small quantity of blood escaped from the wound.

The remarks after the operation were illustrated by a drawing of the natural size, made from a careful dissection of the perineum, and by a diagram showing the relative positions of the pelvic viscera as seen by an antero-posterior vertical section of the trunk. These had been carefully obtained by measurement of the distances and angles in forty dissected subjects, young and old, during the last ten years. Mr. Wood called particular attention to the arrangement of the posterior layer of the deep perineal fascia, its connexion with the recto-vesical, obturator, and anal fasciæ and capsule of the prostate posteriorly, where it separates the urethra and its muscles from the levator ani; and with the anterior layer or triangular ligament and deep layer of superficial fascia below, where it is blended with the central tendon near the anterior margin of the anus. He attaches great surgical importance to this strong fascia, and classified all the perineal operations for stone into—First, Those in which the incisions are made entirely anterior to it, aiming at the membranous urethra; and, second, Those in which the knife is carried behind it into the ischio-rectal fossa, dividing more or less of the levator ani muscle and capsule of the prostate with its large venous plexus. The objections which he entertained towards the latter class of operations are based upon the high rate of mortality resulting from their performance: 1st. From pyæmia, the risk of which he considers to be greatly increased by opening the prostatic venous plexus, connected as it is intimately with the large vesical and hemorrhoidal plexuses; 2nd. From diffuse pelvic cellulitis and consequent peritonitis, which he believed to be mainly owing to section of the pelvic fascia above the levator ani, especially when the muscle is not at the same time divided freely so as to allow the urine a free escape. If the levator ani, or its sheath, be cut at all, it ought to be divided freely. The ureter was in some cases cut off by a free incision, over which accident the surgeon often had little control, with the point of the knife in the bladder so deep from the surface. The accident was rendered the more likely to happen by the spasmodic contraction of the muscles of the trigone approximating the ureters to the urethral orifice, directly in the oblique line of incision into the neck of the bladder, on the contact of the knife with the highly sensitive surface of the trigone. It had happened that, when the bladder was empty at the time of the operation, the point of the knife had passed through its superior wall. 3rd. From hemorrhage. This is more frequent after a free use of the knife in the prostate. In old persons it is often extensive from an enlarged and varicose venous plexus. Sometimes it proceeded from an irregular deep-seated pudic artery, and occasionally from the inferior hemorrhoidal vessels, or from the artery of the bulb.

Of the operations performed in the anterior perineal region, that which had commended itself the most was the median operation as proposed by Allarton. To this method there were some objections, which Mr. Wood considers important. In a boy aged nine years (the age of the patient operated on), he had found the distance of the bulb from the anal bend of the rectum in the median line to be half-an-inch. The mean distance in adults was only an inch and a quarter, from the greater prolongation of the bulb backwards in the median line. The bulb being closely adherent to the triangular ligament by its deep surface, it was impossible to push the whole of it upwards, as some suppose. Consequently, in the median operation, the bulb is invariably more or less cut, to get room and to avoid the rectum. It was thought by many that section of the bulb was of no great consequence; with this opinion he could not agree. Free section of the erectile tissue and venous cells of the bulb for stricture of the urethra had frequently been followed by pyæmia. If avoiding the bulb increased the chances of *the patient* but 1 in 100, he felt strongly that it ought to be done. Without

wishing to over-estimate the dangers of bleeding from this source, he might mention that a very experienced lithotomist had told him that he had had alarming hemorrhage from the bulb in several of his cases. Mr. Wood also thought that stricture might very likely follow an extensive cicatrix in the contractile tissue of the bulb in children.

Another serious objection was the want of room to work the forceps round the bladder, especially if the stone were large and the perineum deep. In one case difficulties of this nature had been followed by the death of the patient. The means adopted in Allarton's method were not powerful enough to overcome an indurated or tough prostate. Hence the invention of dilators and modified gorgets. In the loose tissues of the child, the slipping of the finger from between the staff and the probe would carry it directly between the bladder and rectum, and forcible attempts at entrance into the former would even endanger the entire separation of the membranous urethra from the prostate. The like objection applied to the limited use of the knife in section of the prostate, and attempts to enlarge the opening by dilatation with the finger only upon the staff. If the median incision were necessarily or accidentally continued into the prostate, the ejaculatory ducts were sure to be cut off.

From these dangers Mr. Wood submitted that the operation he had just performed was free. The external incision gave ample room, and was capable of extension in the usual site of the lateral operation if necessary for a very large stone. It avoided the bulb, its artery, the rectum, and larger perineal vessels, and corresponded closely to the plane and lower border of the deep perineal fascia, which he felt it to be so important to avoid cutting. By opening the membranous urethra on one side, he got a greater length between the bulb and prostate in which to insert the point of the dilating finger, and avoided the danger of cutting off the ejaculatory ducts. The advantages of the dilating staff he considered were as follows:—

First. Its extending action upon the membranous urethra renders it easier to make a clean cut into the tube, and to place the point of the finger fairly in the urethral canal. Second. It guides the finger in the prostatic channel with the almost absolute certainty of a double conductor, one on each side. Third. It aids powerfully in the dilatation, acting from above in concert with the action of the finger from below, to which it permits, at the same time, more complete tactile perception of the resisting tissues than in the use of gorgets or dilators. Fourth. It renders the passage of the finger between the pubis and bladder almost impossible, by the pressure of the anterior blade, and diminishes to a minimum the danger of getting between the bladder and rectum. Fifth. It holds the base of the bladder firmly downwards to the rectum and perineum against the dilating force from that direction, which it thus renders more effective, and entirely removes the danger of tearing off the urethra from the bladder. Sixth. It removes the necessity of an extra instrument for getting through the prostate; and lastly, there is that positive certainty of avoiding section and exposure to the urine of the layers of the pelvic fascia, which cannot be obtained by the use of the knife or gorget in the prostate.

These reasons Mr. Wood considered strong enough to justify the introduction of another method of operating in the face of the many methods in vogue.

He concluded his remarks by pointing out the different axes of the urethra and bladder, and their relation to the central tendon, the rectum and the two planes, anterior and posterior, in which he showed the surface of the perineum to lie.

Up to the present date the patient has not had a single bad symptom. The wound is contracted to about an inch, and covered by healthy granulations. There are appearances indicating the occasional passage of the urine by the meatus externus.

[A paper was read by Mr. Wood on the above subject, before the Royal Medical and Chirurgical Society, on Jan. 8th, 1861, after which]

Mr. Fergusson expressed himself pleased with the great beauty of Mr.

Wood's diagrams; but the anatomy which they taught was no novelty. It was well known and taught by anatomists that the bladder in the child was much more out of the pelvis than in the adult; but he had not seen this part so well displayed before. He objected to the instrument invented by Mr. Wood as far too fragile in structure and too uncertain in its probable mode of action, and should prefer the old grooved staff. The prostatic and membranous portions of the urethra were often with difficulty dilated, as proved in the removal of calculi by the forceps; and, except in very young persons, Mr. Wood's instrument would fail to effect dilatation, and even in them its action could not be relied upon. He had no objection to any novelty which might be of service in simplifying or making more easy the operation of lithotomy, but this could not be done by the results of one operation, or by experiments on the dead body. Mr. Fergusson then spoke of the importance of discussing the subject of lithotomy, which of late had not been done; whilst lithotrity had attracted much attention. Of late, there had been a strong feeling in favour of the median operation of lithotomy; but this had been performed chiefly on children, in whom the lateral operation was almost uniformly successful: to show its superiority, it should be tried in the adult. Statistics in large numbers only could decide the question; but he saw no reason at present to prefer the mesial to the lateral operation. *Med. Times and Gazette, Dec.* 22, 1860, *and Lancet, Jan.* 19, 1861, *pp.* 610, 62.

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## 59.—ON SOME OF THE CONSEQUENCES OF STRICTURE AND THEIR TREATMENT.

By F. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.

[*Case* 1.—A waterman, aged 46, was admitted August 17. He had had gonorrhœa many years since, succeeded by permanent stricture. For some years his urine had dribbled from him guttatim without any effort. Two weeks before admission a swelling appeared at the junction of the scrotum and perineum, and an abscess opened spontaneously, through which the urine found its way.]

On admission there was a hard, circumscribed, and tender swelling in the perineum, and a fistulous opening in front of it, through which the urine escaped in drops. As he lay in bed he kept a vessel by his side to catch the water, which was constantly dribbling involuntarily from the penis: in fact, he had entirely lost the power of retaining his water, and was incapable of making any effort to expel it. An attempt to pass a catheter was futile. His general health was feeble, but there was no fever, nor marked constitutional disturbance beyond this. After the lapse of three days, as the symptoms were not ameliorated, he was placed in the position for lithotomy, and a staff being passed down to the seat of stricture, a free incision was made through the back part of the scrotum and perineal swelling upon the staff, a director being employed to guide the knife along the fistulous opening: a small quantity of ill-conditioned pus was evacuated. The urethra having been sufficiently divided beyond the staff, this instrument was removed and the patient returned to bed. A nutritious diet was ordered, and a poultice to the wound.

In the course of two days the urine began to flow freely through the perineal opening, and the patient's general condition was improved; at the same time, the stream being diverted, the water dribbled less from the penis, until it nearly ceased. Some lint was subsequently kept in the perineal opening, which was disposed to granulate too freely near the surface, and some tincture of the sequichloride of iron was given, in ten minim doses, thrice a-day. At the expiration of a month the wound had nearly healed; the incontinence had ceased; and the length of time that the patient could retain his water had gradually increased, and he was able to pass it in a tolerably free stream. There was still a small fistulous opening in the *perineum*. On September 28 he left the hospital.

*Case* 2.—Thomas P., aged 63, a sailor, was admitted on September 21. He has been the subject of stricture for thirty-five years, and had frequently been subjected to catheterism for relief, but not lately. He stated that he had on several occasions suffered from retention of urine; and about eight months since he had an abscess at the back of the scrotum, which burst and allowed the escape of urine. Five days before the perineum began to swell, with severe throbbing pain; the scrotum was also tumid and inflamed. Urine was passed both by the perineal opening and penis, but with straining and pain. There were two or three fistulous openings in the under part of the scrotum, with indurated orifices. Feeble power, and constitutional disturbance indicated by foul tongue, quickened pulse, &c. Urethra impermeable by a small catheter. The perineal section was performed in the same way as in the former case; and some fetid pus was evacuated from the neighbourhood of the urethra. The scrotum was also incised. The stricture being divided, or rather the urethra being cut into behind it, the patient was returned to bed, without any attempt being made to pass an instrument into the bladder. Bark and ammonia with wine and a nutritious diet, were ordered.

The febrile symptoms speedily subsided, and the patient improved daily in health. The wounds became clean, and granulated healthily. For some time the urine found its way principally through the perineal opening; but as the wound closed, it passed more abundantly, and at the same time more freely, by the natural outlet. At the end of a month the water had ceased to escape by the perineum; a very small quantity found its way through the old fistulous openings in the scrotum; but the patient was able to micturate in stream from the urethra, and a good sized catheter could be passed into the bladder with facility. He said that he had not been in so good a condition for many years.

The foregoing cases are examples of a class of common occurrence; but not, therefore, the less interesting and important. Abscesses may occur external to the urethra, and without communicating with it, as in cases where inflammation is propagated, by contiguity of texture, from the urinary passage to the neighbouring areolar tissue. Such collections of matter should be opened early, or they may open spontaneously into the urethra, and thus entail extravasation of urine, and its disastrous consequences. But it is far more frequently the case, that these perineal collections of matter result from mischief which commences within the urethra itself. The rapidity with which such cases run their course varies greatly. Extravasation of urine may occur suddenly, carrying almost immediate destruction to every tissue to which it penetrates; or the consequences of extravasation may be limited, as in case 1, to a small circumscribed spot contiguous to the urethra. The second case illustrates an intermediate condition between the two extremes; there was old disease, but grafted on this was more recent and active mischief, accompanied by constitutional disturbance, sympathetic with the extension of the urine into the scrotum. Sudden extravasation may occur in consequence of the urethra being ruptured from external violence; but such cases belong to a different category altogether, and demand entirely different treatment. When the urine escapes suddenly through a rent in the urethra, consequent on obstruction to its passage, and the incapacity of the canal to resist the force from behind, then this irritating fluid diffuses itself rapidly through the neighbouring areolar texture, which passes rapidly into a state of gangrenous inflammation, and thus perishes. This diffusion of urine is analogous to the emphysematous extravasation of air in wounded lung, but with different results. A case of such urinary gangrene came under my care in private, at the beginning of the year. I refer to it briefly, because I have not lately had a similar hospital case, and it illustrates one or two points of importance which I wish to allude to presently. When I saw this patient the distended scrotum, skin of the penis, and perineum, were infiltrated with urine, and in an irrecoverable condition; and the right groin and contiguous tegumentary covering of the abdomen, half way up the umbilicus, was likewise the seat of urinary infiltration. Free incisions did little more than allow of the more ready separation of extensive sloughs below, but saved the

abdominal skin. The perineum became a gangrenous chasm, the urethra being laid open from the back of the scrotum almost to the public arch, as was proved by passing a sound into this part. Fortunately this patient had a good constitution, and his stomach was kept in good humour, so that he ultimately made a good recovery. The exposed testicles and denuded penis were again enveloped in integument, and the perineal chasm finally closed, leaving, after the lapse of some months, only a small perineal fistula; and even this is now closed, and the urine is passed in an unobstructed stream. No attempt was made to keep a catheter in the bladder during the entire course of the case.

More usually, however, when time is given, nature makes provision to obviate this wholesale destruction from diffused extravasation; and this is apparently accomplished, as in the cases I have narrated, in the following way. A diseased condition of the urethral mucous lining, consequent on gonorrhœa, and entailing stricture, is usually the antecedent state and history. A small point of ulceration in the urethra, possibly at one of the numerous lacunæ, extends outwards into the submucous areolar tissue, and is followed by a minute drop of urine. Inflammatory deposit takes place around this small fistulous opening, which is thus, as it were, walled in, as an ordinary abscess frequently is. But still the irritation of the urine, combined with the *vis a tego*, gradually forces a passage onwards, and the centre of this indurated deposit at length softens down into a circumscribed abscess, to which the urine has access. At this stage the patient may come under our care, suffering from a hard swelling in the perineum, often conical in form with its apex directed forwards, and acutely tender; perhaps attended by complete retention of urine, or at any rate by difficult micturition. There is no fluctuation in this swelling; but on cutting through hard gristly texture, generally of considerable thickness, an abscess, containing it may be only a few drops of matter, is reached, and relief is afforded. If the case run on to a later stage, as in the two instances mentioned, the pus and urine mixed finds a way out for itself; and this may be without extension of the infiltration, as in the first case, or with it, as in the latter. Still, the treatment must be the same, viz., free incision into the seat of abscess. But by this step we reach the urethra, and thus have it in our power to relieve the cause of the existing mischief, as well as the mischief itself. Now, it is to this point that I wish particularly to direct attention; for I think this *desideratum* is best fulfilled by the non-interference plan which I adopted in the cases I have narrated: abstinence, I mean, from further interference after the abscess was freely incised, and the urethra laid open behind the point to which the staff was passed down. I have tested this plan for a long time now, and have no hesitation in recommending it for your adoption as a general rule, in preference to passing a catheter on into the bladder, even if you can accomplish this often difficult task, and leaving it there. Theoretically, the latter course seems to be the most appropriate, and I believe many surgeons regard the introduction of a catheter as an essential part of the treatment. I used to think so; but am now satisfied, and have long been so, that, as a general rule, the presence of an instrument, under these circumstances, is not only superfluous, but positively mischievous. There may, doubtless, be accidental conditions which demand it, such as stricture farther back, which is, however, very rare; or a paralytic and distended state of the bladder; but, even with dribbling incontinence it is not essential, as case I proves. If the urine do not flow readily at once, it very soon finds its way through the artificial opening; and we may then trust safely to nature's modelling power, as we do, for instance, in lithotomy, without supplying a mould for the new material to be modelled upon. This reparative power was well illustrated in the case of extensive sloughing after extravasation to which I have referred. But, beyond this negative recommendation, you save the patient much inconvenience, if not suffering: and I have repeatedly seen the presence of a catheter create so much irritation, that healthy action has been retarded, and even arrested, and its removal thereby imperatively demanded. As the case *advances* towards a cure, then an instrument either bougie or catheter—I pre-

fer the former—may be introduced from time to time, to assist in enlarging the calibre of the urethra; but it need not be left in.

You will not, however, understand me as advocating the treatment of burst urethra from violence in this way. In such cases it should be the surgeon's first duty to introduce an instrument, and to fix it in the bladder. This can generally be accomplished by careful and patient manipulation, and of course without an external incision, unless the mischief resulting from infiltration of urine require it. Indeed, it is the surgeon's business to anticipate such mischief; and to pass a catheter, where he suspects the urethra is ruptured, before he allows the patient to micturate unaided. A case in point occurred under my care a few months since, and I commented upon it in a former lecture—(see Medical Times and Gazette, July 14, 1860). A similar case came under the notice of several of you at an earlier period, in which a railway-guard was struck by the buffer of an engine, and had the ramus of his ischium fractured, and his urethra torn; both of these patients recovered without extravasation, from careful attention to this precaution; and thus, by allowing the urine to drain off through the instrument, so as to prevent the bladder from becoming distended, a ruptured urethra is not necessarily followed by extravasation.

One word in conclusion respecting the cases I have narrated. In both there was mischief of long standing. In the latter the symptoms were more urgent, because the infiltration was extending more rapidly. In neither was was retention of urine complete, had it been so, and if the simple operation performed had not secured relief, it would then have been necessary to pass an instrument into the bladder, if practicable, or to have relieved the distended organ in some other way, as circumstances might dictate. But I believe it will be found that, even with retention, the operation I have described will frequently, if not generally, prove sufficient to give the necessary relief, if not at the time, at any rate soon afterwards. The sesquichloride of iron was prescribed in case 1, to aid the paralysed bladder in recovering its tone.

If it be asked how the division of the urethra through and behind the stricture relieves that diseased contraction permanently, I apprehend the explanation must be found in the fact that rest alone, by diversion of the stream, assists in this desirable result, as demonstrated by the operation of tapping the bladder, for retention, by the rectum. But further, I believe that in the suppurative action that follows, the adventitious tissue which causes the contraction or stricture is softened down and got rid of, and thus a more healthy and natural condition of the submucous tissue remains.—*Med. Times and Gazette, Dec.* 15, 1860, *p.* 575.

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## 60.—ON A NEW AND UNIFORM GAUGE FOR CATHETERS.

By Henry Smith, Esq., Surgeon to the Westminster General Dispensary.

In a paper which I lately read before the Medical Society of London, relating to the treatment of stricture of the urethra, I referred to the importance of carrying on dilatation of the canal slowly and gradually, so that as little disturbance as possible should be produced either in the part operated on, or in the system generally. I also stated that this gradual dilatation would be less liable to be followed by the return of a stricture than if any of the severe methods now often adopted were put in force, but that this kind of dilatation could not be effected in the manner it ought to be, by the instruments generally found in the hands of surgeons, in consequence of the gauges, according to which they were constructed, not being sufficiently minute in their increments, and there being no real uniformity in the scale.

My attention has long been directed to this point, and it has appeared to me most desirable that the difference in size between our catheters should be more minute than they are, and that the increase should be uniform. My friend Mr. Bishop was kind enough to assist me in this matter, and after having carefully examined a gauge according to which catheters are usually made in London, pronounced it to be both "variable and arbitrary" as re-

gards differences in size. It was proposed that in the new gauge the increments should be exactly the one-hundredth part of an inch throughout the entire scale, being very much less than in that examined; but on calculation it was found that if the unit (or No. 1) catheter were to be, as I proposed, much about the same size as that now in use, twenty or more instruments would be required before a size equal to an ordinary No. 12 could be reached. It was then proposed that the increments should be the seventy-fifth part of an inch in the scale throughout. Mr. Matthews, of Portugal street, has made a gauge accordingly, and has constructed for me a new set of catheters, the increase in size being exactly the seventy-fifth part of an inch from No. 1 to No. 15 in each instrument. My No. 15 is about equal in size to the ordinary No. 12, therefore in the set there is an addition of only three numbers. According to this plan, the increase in size in each catheter is not only minute and exact, but it is regular throughout the series, the great desideratum hitherto wanting. Consequently dilatation can be carried on in a mathematically gradual manner,—so slowly, so painlessly, and yet so effectually, that the advantages attending this mode of proceeding must be obvious to all who reflect on the sensitive nature of the urethral canal, and appreciate the difficulty which exists in getting rid of the thickening of a permanent stricture by ordinary dilatation.

All practical surgeons who have had much to do with severe cases of stricture, have found that an instance is not unfrequently met with where a No. 2 or 3 catheter can be introduced; but where no progress can be made beyond one or two sizes; a No. 4 or 5 is arrived at, then the dilatation is stopped; either caustics, cutting, or forcible ramming down of instruments are employed; or it is only after a lapse of some time, and after the painful introduction of the last-sized catheter, on many occasions, that a larger instrument can be put into the bladder. This difficulty is accounted for by the fact that the increments in the old gauges being, as Mr. Bishop says, "variable and arbitrary," and also by a circumstance not hitherto, so far as I am aware of, sufficiently appreciated by surgeons, viz., that in these more severe strictures the urethral canal loses its capacity for dilatation in proportion as it is dilated. It is too often supposed that because a No. 5 or 6 catheter can be passed through a stricture, it must of necessity follow that the diseased canal can be dilated to its normal state as readily as it has been dilated so far. This is a great mistake; and it will in truth be found that the further dilatation will be a most difficult task, in some cases an impossibility. This is owing, in a measure, not only to the incapacity of the canal to dilate, but also in a great degree to the arbitrary and abrupt increase in size in the catheters as ordinarily used, especially in the higher numbers. A great deal of this difficulty will be done away with by using instruments constructed according to the scale proposed in this notice, the increments in which are minute and uniform. I find great advantages in the use of these instruments, and I would strongly recommend all those surgeons who are about to have catheters made, to get them constructed strictly according to the gauge now used by Mr. Matthews.—*Med. Times and Gazette, Feb.* 23, 1861, *p.* 195.

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## 61.—ON THE USE OF POTASSA FUSA IN THE TREATMENT OF STRICTURE OF THE URETHRA.

By Campbell de Morgan, Esq., Surgeon to the Middlesex Hospital.

[The author, in the commencement of his paper, states his belief that this mode of treatment has not received that attention which it merits. The case related is that of a tailor, aged 44, whose lungs contained tubercular deposits.]

About two years ago, he found that he could not at all times retain his urine. It passed from him involuntarily and spasmodically immediately on his feeling a call to micturate. At the same time the stream began to di*minish*; and it continued to do so until about three months ago, when he found

that he could pass urine only in drops. From the first, he had severe and forcing pain in passing his urine. The desire to do so was almost constant; and for some weeks his rest has been completely broken by it. In the course of the twenty-four hours, he voided an average quantity, which was of specific gravity 1015; was slightly acid; and contained no albumen; and no more than the usual amount of mucus. There was no hardness nor tenderness in the perineum. On passing a bougie into the urethra, a stricture was found near the membranous part. Attemps were made to introduce the finest instruments into the stricture, but without success. He did not experience much pain from the contact of the instruments; nor did any bleeding take place. The stricture seemed very hard and resisting.

On the 6th a piece of potassa fusa, of about the size of a large pin's head, was passed down to the stricture by means of a large bougie hollowed at the extremity to receive it. This gave rise to considerable pain, especially when urine passed. This pain, save when he tried to urinate, ceased by the following morning. For nearly forty-eight hours there was no difference in the way in which the urine flowed; it came in drops; but then he began to pass a small stream, ending in drops, and with still some scalding.

On the 10th, the potassa fusa was again applied in the same manner. He had pain as before; but soon afterwards the stream enlarged, and he began to retain his urine for a longer period.

On the 17th, a No. 2 catheter was passed readily into the bladder; and then catheters were introduced one after another till a No. 8 was passed. The urine now came away always in a stream, without any tendency to its passing in drops, though he still had some pain. He now got rest at night, and said that he was in every way more comfortable, except from his cough, which was more harassing.

On the 27th, a middle sized catheter was again passed, but it was obstructed at the upper part of the stricture, and it required to be depressed, in order to guide it through. A small piece of the potassa fusa was again applied; the hollow for its reception being made in the upper part of the bougie, so as to touch the upper margin of the stricture. The pain was the same as before, and there was some scalding for three or four days, after which he had no pain whatever. From the time of this last application no further interference was called for. The stream became free, and from day to day he had less trouble, so that after about ten days he passed a larger stream than he had done for two years, and could hold his urine for many hours; he was indeed scarcely disturbed at night. Every day he seemed to be gaining ground, so far as the urinary organs were concerned, up to the time of his death, which took place on February 1st, the symptoms of phthisis having made rapid progress during the intensely cold weather.

The caustic potash is a safe as well as a rapid agent in removing a stricture. The stricture was extremely small; I could not get a fine instrument into it; and perhaps lengthened and repeated trials would have been necessary before I could have succeeded in doing so. The attempt would probably have caused irritation, and might thus have complicated the case; for, unless a bougie pass the stricture, it is often productive of irritation. Had a bougie been passed, there is no doubt that dilatation might have been easily effected. The stricture was limited to a small segment of the membranous part, and there is no hardness to be felt in the perineum. I was satisfied, however, that not only a more rapid, but a more permanent relief, would be obtained by the application of the caustic, than by any mode of using the bougie alone; while any more decisive measures were entirely uncalled for, and would have been improper.

The patient had all the indications of phthisis; and on that account, again, it was perhaps desirable to free him from his trouble in the urinary organs as promptly and with as little irritation as possible. The case can hardly be regarded as one which shows most strikingly the effects of the treatment; but you have not perhaps seen the caustic applied, and it is worth bringing to your notice in connexion with the general question of the treatment of stricture.

The treatment of stricture by caustics, in one form or another, is of long standing. In this country the nitrate of silver was brought into general notice as a means of destroying stricture by Sir E. Home, who had seen it advantageously used by Mr. Hunter. There is no doubt that many cases of obstinate stricture were cured by it; but its use has become restricted to those in which there is a very irritable state of the membrane; and in such cases the lunar caustic acts as it does when applied to any other irritable surface; it deadens the sensibility. Its escharotic power is not, however, very decided. You have often seen it applied freely even to the conjunctiva, without its destroying the surface of the membrane. When it does act as an escharotic, it gives rise to swelling and inflammation of the surrounding parts. Its use as a destructive agent on the diseased tissues has been generally abandoned.

Mr. Whately first suggested the application of potassa fusa as a means of removing stricture. He only allowed of its use, however, in those cases where a fine bougie could be passed fairly through the stricture. Where this could not be done, he thought that the application of the caustic might set up inflammation, and so, by diminishing still more the aperture, cause a complete retention. The small quantity of the caustic which he used, and the precautions which he thought it desirable to adopt in order to prevent its too free action, made it in his hands a far less effective agent than it has since proved to be.

It is to Mr. Wade that we are indebted for a more just appreciation of the value of this remedy, and I would strongly recommend you to read his views on the subject. He pointed out the true distinction between the action of lunar caustic and that of the caustic alkali. The lunar caustic, he observes, gives rise to a hard slough, and to the effusion of lymph around it; while the potassa fusa effects a softening and dissolution of the thickened tissue, without producing any surrounding hardness. There is little risk, therefore, of causing that blocking up of the stricture which was feared by Mr. Whately. Mr. Wade also uses the caustic in larger quantity, and does not cover it with lard, as was recommended by that gentlemen. He applies it to cases of impermeable stricture, whether those of merely a small cord-like constriction, or where the canal is indurated to a considerable extent; and he expresses his belief that, in cases so treated, there is but small disposition to a return of the disease. The last mentioned point is one of great importance, and if the opinion be correct, would, *cæteris paribus*, render this mode of treatment preferable to most others.

As a general rule, if an instrument can be passed through a stricture, the cure is half effected. The careful gradual use of the bougie will cause the dilatation of the stricture; and, where no untoward symptom is present, such as great irritability or tendency to spasm, this is undoubtedly the best and safest mode of dealing with the complaint. But, in the majority of such cases, the bougie must be regularly used from time to time after the stricture has been dilated; for, when once a stricture has formed, there is usually a great disposition to its return. This is, no doubt, often the result of constitutional causes; the gouty constitution, or chronic dyspepsia, will keep it up. So, too, will local causes—a persistent irritable state of the bladder, or hard horse-exercise. Very frequently, however, the patient cannot give up the time required for this gradual dilatation; and frequently also the stricture will not yield to this plan of treatment. In other cases, again, the urethra seems to resent the contact of the instrument; irritation is set up whenever it is used, or the tendency to spasm is increased after it is withdrawn. In all such cases, the surgeon must resort to other modes of dealing with the stricture. Many plans of treatment have been adopted, suitable to the various complications which may exist, or to the necessities of the patient. Forced catheterism; the dilators of M. Perrève, which have been introduced into this country by Mr. Holt, and which I have used in some cases with great benefit; the dilators of Mr. Wakley; perineal section, or intra-urethral *section*, and other methods,—have been put in practice with more or less *success*; the constitutional condition of the patient being, of course, attend-

ed to. But the disposition to return is perhaps even greater in these cases than in the more simple states of the disease. Does the removal of the stricture by potassa fusa give a better chance of permanent relief? I believe it does; and, irrespective of the cases reported by Mr. Wade, which are entirely satisfactory, my belief is founded partly on theoretical grounds, partly on my experience.

I have often pointed out to you the peculiar appearance of the cicatrix left after the healing of a deep issue made by the caustic potash. Instead of being puckered and hard, as you generally see in the cicatrices which follow the total or partial destruction of the skin by the application of the actual cautery or by most escharotics, it is soft and smooth, and glistening, as large as the portion of the skin originally destroyed, and merging almost insensibly into the sound tissue. My friend, Mr. Hoffman of Margate, some years ago told me of the benefit which may be obtained by destroying the unsightly puckered scars which are left after the suppuration of scrofulous glands of the neck, with this caustic. When the sore produced by the falling of the eschar has healed, you find that you have substituted a smooth even surface for the previous seamed and corded one. In the course of time, the new tissue regains so natural an appearance as to escape notice. This plan I have often put in practice, and with excellent results. The same effect is, I believe, produced in the urethra when the diseased hardened tissue has been thus removed: a smooth surface is left, with little or no disposition to contract; and a more permanent cure is effected than is usually produced by the bougie. The *post mortem* examination allowed us, in the present case, to see the condition of the parts recently acted on by the caustic; and you will observe how completely, so far as they go, the appearances correspond with those which attend the healing of an issue made with the same caustic; and how very slight is the disposition to the effusion of any plastic materials.

The first case on which I tried the caustic, now some years ago, gave me a very favourable idea of the benefit to be derived from this plan of treatment.

A strong-built middle-aged man, who had been a sailor, and afterwards a cobbler, had injured his perineum while serving in the navy. I presume, from his account, that extravasation of blood to some extent had taken place. Stricture began to form, for which he was treated by bougies, but with only temporary relief. The last time he had been under a surgeon's hands, no instrument could be introduced into the bladder. He progressively got worse, notwithstanding any temporary benefit he may have derived from treatment. When he came to the hospital, he was suffering from constant desire to pass his urine, which only dribbled from him. The urethra could be felt in the perineum hard and corded for at least an inch in length; the attempt to pass a catheter brought on severe spasm, and was followed often by rigors. The necessity for speedily relieving this state of things was evident; and I determined to use the caustic potash, rather, I believe, with a view of removing the extreme irritability, than of destroying the stricture. The caustic was applied in the manner recommended by Mr. Wade, and as you saw in the present case. The bougie was kept pressed against the stricture for about thirty seconds; and the operation gave rise to very little pain—less, certainly, than the unarmed bougie had caused. As in the present case, it was found that, in the course of a day or two, he was far more comfortable; he had less difficulty in passing his urine, and less urgency. No unpleasant symptom followed the use of the caustic. I determined, therefore, to repeat the application; and this was done at the end of a week with increased benefit. After two more applications, the urine came away in a good stream; and a large bougie was passed without any difficulty into the bladder. I believe that strictures produced by mechanical violence are amongst the most obstinate of those with which we have to deal; but I have seen this man twice since; the last time was four years after the treatment by the caustic. On both occasions he had some spasmodic irritation, after too free living; but a full-sized bougie could be passed easily and at once into the bladder. I have no doubt that he would have returned, had he again suffered from his complaint.

It is only fair to say that the treatment of stricture by caustic has been condemned by many good surgeons, both British and continental; indeed in France its use is almost unknown. Nélaton, for example, states that cauterisation is now rarely employed, in consequence of the uncertainty of its action; of the swelling of the urethra and retention of urine, which often result from it: and of the deposit of the retractile tissue which is formed afterwards, and which ultimately produces a new impediment. Here I have no doubt he is associating the effects of nitrate of silver with those of caustic potash—effects which, as I have stated, are very opposite. I have certainly seen no such effects, nor any tendency to them; and this is the conclusion at which Mr. Wade, with more extended experience, has arrived. I cannot help thinking, with that gentleman, that the treatment has been condemned rather from want of experience of its effects than from experience of its dangers. For a more complete exposition of these effects, and of the mode of applying the treatment, I must refer you to his works on the subject.—*Brit. Med. Journal, March* 2, 1861, *p.* 217.

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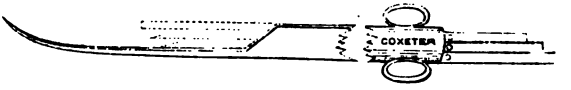
## 62.—AN INSTRUMENT FOR CUTTING, DILATING, AND CAUTERIZING STRICTURE OF THE URETHRA.

It consists of a flat, metallic tube, seven inches long and a third or more of an inch broad, to one end of which there is attached a grooved director of a size that will fill the channel of the stricture. Two or three long steel rods fitting this tube constitute the rest of the apparatus.




When it is used for incision, one of these rods ground for about an inch at the end into a cutting blade, is passed through the tube into the groove of the director, from which it is then pushed transversely and made to press upon and cut the stricture by one or both of the other rods. In cases of very narrow stricture, instead of a director, a cutting blade as small as is consistent with safety may be used, which is covered during the passage of the instrument by the blade of the steel rod in the manner of a pair of scissors.

Hemorrhage, urinary infiltration and pyæmia are consequences of the internal incision of stricture as hitherto performed by instruments which cut as they move forwards or backwards in the canal, making wounds of uncertain depth and extent. The new instrument cuts by pressure alone; or, when it is necessary to add a little sawing motion, this will not endanger the sound and elastic urethra, especially when the knife is not very keen, as it is only as respects the resisting stricture that this motion is conjoined with the degree of pressure required to produce incision.

Solid dilators of transverse or eccentric action have been used by M. Perrève and others, but their numerous defects have prevented their general adoption, notwithstanding the superiority of their principle to that of instruments acting as wedges. The objection to the dilator of fluid pressure is the difficulty of making and using it, although its great advantages should compensate for this difficulty. When the new instrument is used for dilatation, a blunt rod is substituted for the sharpened one; and as the dilatation advances, an inch of it near the point may form half a cylinder of considerable size, lying upon, instead of entering the groove. The advantages of direct or transverse dilatation, whether by a solid or fluid body, were first pointed out, and are fully explained, in Dr. Arnott's works on stricture and stone.

When the object is cauterization, a grooved wire covered with nitrate of *silver or potassa*, is substituted for the cutting or dilating rod.

Although the great majority of strictures can be kept at bay by the common modes of dilatation, there are many hard, irritable, and elastic strictures which will not yield to these; and unless more efficient practices are resorted to, the patient is exposed to irreparable injury from the effect of continued irritation on the neighbouring organs. Unfortunately the more efficient practices hitherto employed are not unaccompanied with danger. Escharotics have fallen into disuse from the difficulty of confining their action; internal incision has the dangers already enumerated; and the perineal section, when restricted to cases in which other treatment is unavailable, has proved a very hazardous mode of procuring relief. The new instrument, by furnishing us with a better, and a very easily-manageable, means of dilating than the wedge, and a perfectly safe means of incision in cases not yielding to dilatation, supplies a great desideratum in this department of surgery. —*Med. Times and Gazette, Feb.* 2, 1861, *p.* 133.

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## 63.—UNSUSPECTED ABSCESS OF THE PROSTATE GLAND, IN A CASE OF GONORRHŒA, WITH FEBRILE SYMPTOMS, FATAL RESULT.

Under the care of Dr. PITMAN, Physician to St. George's Hospital.

[Inflammation of the prostate gland, the result of acute gonorrhœa, sometimes runs rapidly into abscess—this result is usually announced by the occurrence of rigors—sometimes, however, nothing of the kind occurs, and the symptoms are very obscure. In the present case the abscess was not discovered at all during life, the patient sinking from the severe constitutional symptoms to which it gave rise. From the tumefaction caused by the tumour, instrumental assistance was necessary to evacuate the contents of the bladder.]

On referring to such standard works as those of Thompson, Adams, or Hodgson, on the "Enlarged Prostate," or Coulson on "Diseases of the Bladder," we can find no analogous case. The patient did not complain of anything that could have drawn attention to the seat of his disease, although an examination was made as far as seemed to be necessary under the circumstances. At the autopsy the abscess was found to communicate with the urethra, but evidently had not done so till very shortly before death; and the structure of the gland throughout the greater part of its extent was disorganized.

John E———, aged twenty-five, a baker, admitted April 25th, under the care of Dr. Pitman. He had had gonorrhœa for a fortnight, with pain across the loins. Four days before admission this increased in severity and extended down the legs, so that he had to discontinue work. He never had any shivering. On admission, he had much the aspect of a fever patient; the tongue was very dry, though of natural colour, excepting a white line all round the edge; the pulse was soft, 72. He did not complain of any pain, excepting a little in the legs, which was supposed to be rheumatic. There was a slight dry cough. He was unable to pass urine, except with the aid of a catheter. He was ordered to take the ammoniated saline with fifteen grains of chlorate of potass every six hours, and strong beef-tea was given.

May 1st. The surgeon was now requested to examine him; wine was given (six ounces); and as nothing had been elicited by the examination, the former treatment was continued. He was delirious at night, and in a state of great prostration, with involuntary evacuations. The skin was very hot, and still free from eruption or change of hue; the tongue dry and cracked; the pulse jerking and full, but utterly powerless, and with an aneurismal thrill under the finger.

3rd. His only complaint was of thirst. He gradually sank: ceased to swallow in the evening, and died towards morning.

*Post-mortem examination.*—The body was in good condition. There was profuse purulent discharge from the urethra, which had collected about the

glans penis. The lungs were healthy. The heart was large; there was a large fibrinous vegetation on one of the flaps of the aortic valve, and the aorta was slightly atheromatous. Liver and spleen healthy, the latter rather soft. Right kidney healthy; in the left, the pelvis and ureter were rather more vascular than usual. Peritoneum healthy. The mucous membrane of the urethra was examined from its commencement to its termination: it seemed slightly injected in parts, but this appearance was so doubtful that little reliance is to be placed upon it. The muscular fibres of the bladder seemed rather thicker than natural, but its mucous membrane showed no obvious trace of inflammation. Between the bladder and rectum was a large abscess, which at the time of the examination communicated with the floor of the prostatic urethra by two ragged orifices. The tissue of the prostate gland was extensively destroyed and eroded by the abscess, and on squeezing the abscess, pus could be made to exude by the prostatic ducts; but in parts near the abscess the tissue of the prostate seemed healthy. The cellular tissue in the neighbourhood of the abscess was condensed; in other parts of the pelvis it was healthy.—*Lancet, Oct.* 27, 1860, *p.* 408.

## DISEASES OF THE SKIN.

### 64.—ON PARASITIC AFFECTIONS OF THE SKIN.

By Dr. T. McCall Anderson, Fellow of the Faculty of Physicians and Surgeons of Glasgow.

[It is now thought by many that Herpes circinatus, Sycosis, and Tinea tonsurans are only varieties of the same disease, resulting from the difference in locality. In the majority of cases the destruction of the parasite is the *sine quâ non* of the treatment. It is necessary to remove the hairs, as the parasite penetrates into their interior, and is found embedded amongst their longitudinal fibres. This renders them very brittle and liable to break. The most successful mode of extracting the hairs is by means of a pair of broad-pointed forceps.]

If the diseased surface is not very extensive all the hairs can be removed at one sitting. As a general rule as much as three or four square inches of surface covered by hair can be removed daily; so that even were it necessary to remove the whole of the hair from the scalp this may be done in a week or so. But the extent of surface capable of being cleared at one sitting will depend entirely on the patient, those who have very sensitive skins being very intolerant of depilation, in which cases we must be content with slower progress, the depilation extending over a period of three or four weeks according to the degree of intolerance and the amount of hair to be removed. In France, where this treatment first assumed a definite form, great stress is laid on the manner in which the depilation was performed; and, indeed, in St. Louis Hospital, servants are employed (*épileurs*, as they are called) whose whole duty consists in extracting the hair of those patients affected with vegetable parasitic diseases.

Their *modus operandi* is thus described by Bazin:—"Our depilators are seated, and cause the head of the patient to rest upon their knees. With one hand (generally the right) they hold the forceps as one holds a writing pen—the other hand is applied to the part about to be depilated, with the thumb and index finger of which they put the skin on the stretch to keep it steady. They then extract the hairs, pulling them out in the direction of their axis, and only a small number at a time, two, four, six, or at most a small bundle. It is necessary to avoid depilating too quickly or too gently, there being an intermediate point which one can only arrive at after a little practice."

Before commencing the extraction of the hairs it is of course necessary to cut the hair short, say within an inch of the skin, and by means of poultices to remove any crusts which may have formed, and matting the hairs together. Bazin also recommends a layer of the oil of cade, which he says

"destroys, in part, the parasite situated on the surface of the skin, extinguishes the sensibility of the scalp, and facilitates the extraction of the hairs." Did the oil of cade fulfil these conditions, it would certainly be of great value in the treatment by depilation. That it destroys some of the spores no one can doubt; but from what I have seen of this practice, I would be very chary indeed of admitting that it in any way deadens the cutaneous sensibility or facilitates the extraction of the hairs.

A question of great importance is, *When should the depilation be stopped?* There is no doubt that one depilation would be sufficient were all the hairs removed entire, and the parasiticide efficiently applied; but there is great difficulty in removing all the hairs, owing to their friability, and the depilation thus requires frequently to be repeated. Each successive depilation, however, becomes less serious; for we generally notice, after one extraction, that the disease is extinguished at some parts, and reappears only here and there, at those places namely, where the hairs have broken in attempting to remove them. It is only these latter parts, then, which call for a repetition of the operation. After a little experience one can readily distinguish when the disease is eradicated, and when the treatment can be dispensed with. When the hairs shoot out with all the appearance of health, after depilation, their previous friability and twisted appearance being gone, and when the skin assumes its natural appearance, we can then withhold the forceps, and confidently expect a permanent cure. It must, however, be borne in mind that, long after the spores have been destroyed secondary symptomatic eruptions may continue which do not require further depilation, but treatment founded on general principles, and varying according to the nature of the eruption. It requires very little clinical experience to distinguish the symptomatic eruptions from those peculiar to the parasitic affection.

Having now described the method of extracting the hairs, it remains to mention the best parasiticide applications. Of these the bichloride of mercury is one of the best, and the one most generally used; oil of cade and Turbith mineral are much employed in France. Sulphur is also good, although more successful in the destruction of the animal parasites. As for the excipients, water and glycerine are probably the best. Lard is also good, provided that care be taken to get it fresh, and not to keep it too long; for, if rancid lard is applied to the affected parts it favours, in a wonderful manner, the formation of secondary eruptions. The strength of the parasiticide to be used is a point of great importance. Two grains of the bichloride of mercury to an ounce of water is sufficiently strong, and a little alcohol or muriate of ammonia should be used to facilitate the solution of the mineral. Sulphur is best used in the form of ointment, in the proportion of half-a-drachm to a drachm of sulphur to the ounce of lard. Turbith mineral is usually employed in the same proportion, and lard is the general excipient. Oil of cade may be mixed with glycerine or lard in the proportion of half-a-drachm to a drachm in an ounce of the excipient.

Whichever of these is employed, must be rubbed into the skin *during* each depilation, as previously mentioned, and *after* the depilation is completed, and the disease apparently cured, it should be continued for some time. The disease should not be pronounced cured till a considerable time has elapsed after the cessation of the parasiticide, and that for obvious reasons.

After the cure of the parasitic affection, symptomatic, especially impetiginous eruptions may be left, which must be treated according to their nature, and as they are produced by the irritation of the parasite or of the remedial applications, soothing remedies are generally the most useful. But, sometimes, the irritation which the disease produces is the means of calling out some latent diathesis (*e.g.*, an eczema, a psoriasis, or a syphilitic eruption), in which case, general remedies must be employed, in order to correct, if possible, that particular state of system.

Finally, we must, if possible, remove the predisposing causes of the disease. The nature of these causes has, as I have said, not yet been thoroughly investigated, and we must, therefore, at the same time as we attack

the disease locally, attend to the general health, and be guided in this by broad, general principles.

It now remains for me only to say a few words with regard to the particular treatment of each of the varieties of Tinea tonsurans, and also of the other modes of treatment very generally in use in this country.

In cases of *herpes circinatus* (ringworm of the body) the application of a parasiticide is generally sufficient, as the down which usually covers the skin of the body is not impregnated with the spores, except to a very partial extent. In very hairy persons, however, it may be necessary to proceed to depilation, especially if the hairs are manifestly diseased. Herpes circinatus is an affection which not unfrequently gets well of itself, the parasite not having, apparently, the same hold as on the hairy parts of the body. When there are many patches of herpes circinatus on different parts of the body, it is best treated by sulphur or mercurial vapour baths, which act on the whole skin, and thus attack all the patches at once.

*Sycosis* must be treated both by depilation and parasiticides. When the diseased process has run very high and there is a great deal of suppuration and induration, it is, according to some, better to commence the treatment by soothing applications, such as cataplasms of potato-starch combined with purgatives. I am not quite certain, however, whether it is not better to proceed to depilation at once, for the best way of removing the irritation is to remove the cause, and besides, the hairs are much more easily extracted while the inflammation and suppuration are considerable. In the early stages of the disease, depilation causes considerable pain, and, although well performed, some of the hairs usually break, leaving their diseased bulbs in their follicles, so that a second or third depilation, although only a partial one, is generally required. When the disease is treated in the advanced stages after the formation of pustules and indurations, although the inflammatory and suppurative processes have destroyed the parasite almost completely, the depilation is just as necessary. For, although the fungus is all but destroyed, the diseased ends of the hairs remaining in their diseased and abnormally secreting follicles, act as foreign bodies and keep up the inflammation. At this advanced stage of sycosis, the results of depilation are certainly *marvellous*. The hairs are removed with the utmost facility, and one depilation is generally sufficient; but it must be combined with the application of a parasiticide lotion or ointment. It is almost incredible, to those who have not witnessed cures from this treatment, the rapidity with which a chin, twice its ordinary size, and covered with large tubercles, indurations, and crusts, becomes, in the space of two or three weeks, perfectly healthy in appearance, with this exception, that here and there patches of alopecia are left, which are permanent; but this is not the fault of the treatment, but the fault of the patient in neglecting to apply for advice in the earlier stages of the disease. In the treatment of sycosis, many are in the habit of applying strong caustics, a mode of treatment which cannot be too much reprobated; for unless it be carried to such an extent as to destroy the skin and hair-follicles, and thus produce great disfigurement, it is not and cannot be successful. I am glad to think that this mode of treatment is becoming more rare in the present day. Arsenic has always been a very favourite medicine in the treatment of sycosis, and its use, combined with cod-liver oil, is highly extolled by Hunt, who seems to look upon it almost in the light of a specific. I can easily conceive that this remedy, so powerful in many other cutaneous affections, may act beneficially in changing the state of the system favourable to parasitic development, although I have never had occasion to try it; but I can hardly think that it can cure the disease, unless, by changing the composition or qualities of the blood, it causes the death of the parasite. Besides, in examining the illustrative cases cited by Hunt, I am left in doubt as to the nature of the affection. Thus, some are described as labouring under a tubercular eruption of the bearded portions of the face, others as having been troubled with sycosis one year, and in another case the only thing we are told with regard to it is that *the* disease had existed "on and off," for *fourteen years*. And on looking at the results of the treatment I find that one

patient was "nearly well when he last applied," another is "believed to have recovered," and a third "absented himself, *probably* cured"!! Now, however much Mr. Hunt may have convinced himself of the efficacy of arsenic and cod-liver oil in the removal of sycosis, it is necessary to have more convincing proof than he has given us of its beneficial operation. And even supposing that Mr. Hunt's treatment is capable of removing sycosis, it is not nearly so successful, even by his own showing, as the treatment by depilation and parasiticides.

In *Herpes tonsurans* (ringworm of the head) depilation and parasiticides form the best treatment. It is, however, the most intractable of the three varieties of tinea tonsurans, owing to the friability of the hairs, and the difficulty of extracting them entire. Depilation requires, therefore, to be frequently repeated and the cure is thus much retarded. This can, however, be always anticipated with certainty. Caustics have been much used in this affection, as in sycosis, but the remarks made with regard to them when treating of the latter apply equally to Herpes tonsurans. Shaving off the hair from the diseased parts, followed by the application of stimulants and combined with tonics internally, are often made use of, but the results of this treatment are not very satisfactory. Dr. Jenner recommends strongly an ointment composed of twenty grains of ammonio-chloride of mercury, and four drachms of sulphur ointment, which is certainly a powerful parasiticide. He states, however, that the removal of the hairs is *impracticable*, a statement which is far too sweeping, although there can be no doubt that the hairs break very frequently, and that great patience and repeated efforts are required before a satisfactory depilation is attained.

[Speaking of the *Diagnosis*, the author observes:]

This is, in the majority of instances, very simple. In cases where the head is more or less covered with an eruption exhaling the odour of mice, and consisting of bright yellow dry crusts depressed in the centre, through the middle of each of which one or more hairs pass, which have a dull dry appearance, and are more easily extracted than natural, the diagnosis is perfectly simple; and those who have seen the disease once can never mistake it. When it has continued for a length of time, when the crusts have lost their cup-shaped form and their bright yellow colour, and have become entangled in the hairs; when, in fact, we have to do with the variety described as favus squarrosa, it may be—and often is—mistaken for impetigo of the scalp. But in the former there are generally *patches* of alopecia, which are wanting in the latter. In it certainly the hairs often fall out, although only *here and there* and not in *patches* as in favus. The alopecia of favus is *permanent*, that of impetigo generally *temporary*. There is also no alteration of the hairs in the latter; in the former they are dull, dry, discoloured, and easily extracted. Attention to these points generally serves to clear up the diagnosis; but if doubt still exists, it may at once be removed by the microscopic examination of the crusts. There is one point, however, which requires to be borne in mind, namely, that the discovery of some pustules does not prove that the disease is impetigo, as pustules are frequently developed in cases of favus from the irritation of the parasite. And also one should not lay too great stress on the value, in a diagnostic point of view, of the odour exhaled from the eruption; as this symptom is not so pathognomonic as some dermatologists would lead us to suppose.

Psoriasis of the head might in some cases be mistaken for favus when this latter is not well marked. But in the former there is no alteration of the hairs, no alopecia, and no peculiar odour. The crusts, also, are much whiter, are more adherent than in favus, and the characteristic silvery scales of psoriasis are generally to be detected on other parts of the body, and especially on the elbows and knees, for the psoriasis confined to the head is rare. In doubtful cases the microscope at once determines the nature of the affection.

Very often the diagnosis is rendered difficult on account of a propensity of parents to clean carefully, and remove all the crusts from the head before bringing their children for advice. There is then to be seen redness of the

scalp combined with the presence of a few pustules, the results of irritation and which makes the disease resemble impetigo. But if it is a case of favus which we have before us the *deep red, depressed*, distinctly *circumscribed* surface, covered by a *thin shining* epidermis, is quite different from the light-coloured, diffused redness of impetigo. If this is not sufficient, the hairs should be examined, when they will be found to be altered, and the parasite is detected in them with the microscope. If this is not satisfactory, do not give an opinion, or resort to any treatment, but desire the patient to return in a couple of weeks, leaving the head untouched in the *interim*, after which time the disease will have had time to re-develop itself, and its nature is at once discovered. Favus, attacking the hair-follicles of the body, is easily detected, as the crusts invariably assume the cup-shaped form, and the epidermic variety of the disease, and favus of the nails, if not overlooked altogether, are easily diagnosed, provided always that we keep in mind the characters pertaining to each, as given above, and do not forget the fact that they occur, almost invariably, secondarily to the favus of the hair-follicles.

The *Prognosis* which in former times was very serious, owing to the supposed incurable nature of the disease, is now, on the contrary, very favourable; for it has been satisfactorily proved that, by the aid of the treatment about to be recommended, a cure, even of the very worst cases, can invariably be effected. If treated in its early stages, recoveries take place without leaving the slightest deformity, but when the treatment is commenced after the disease has lasted a length of time, the alopecia and the atrophy of the scalp, which it produces, can never be removed. Cases of death from pure favus have always been rare, and should never occur in the present day, and, indeed, it is only after it has lasted a very long time, that it affects the general health to any extent.

*Treatment.*—It can easily be imagined that a disease such as favus, which can be traced back to the remotest ages, and which, till of late, was deemed incurable, should have called forth very varied and opposite principles of treatment. Some considered it to be essentially a constitutional, in fact a scrofulous affection, to be removed by constitutional treatment only; others were of opinion that it was purely a local disease to be removed by topical applications alone; while a third party looked upon it as a disease partly local and partly constitutional, and attacked it both by local and general remedies. The local treatment has, however, at all times and in all countries, played a more important *role* than the constitutional. When no treatment is adopted, it is very rare indeed that the disease disappears, unless after it has destroyed every one of the hair-follicles of the scalp and produced a permanent alopecia. But even then, it may continue to flourish on other parts of the body. When left to itself it generally lasts more or less extensively during the whole life of the person affected.

The effect of an acute intercurrent disease, a typhoid fever for example, on the favus affection is very curious. During its continuance the achorion does not flourish, but fades as plants do, when the soil in which they are planted is not supplied with moisture, or is otherwise inappropriate, and it only assumes its pristine vigour after the disappearance of the fever. Of the many varieties of local treatment adopted, that by means of the *Calotte* is the most celebrated, and is still practised in France to a considerable extent. This consists of a plaster which is prepared in the following way: "About four ounces of rye-flour are mixed with a pint and a half of white vinegar in a pan, this is then put on the fire, and the mixture stirred continually; to it, when it boils, is added half an ounce of the carbonate of copper in powder. The whole is then boiled for an hour, after which about four ounces of black pitch, six of Burgundy pitch, and four of resin are added; when thoroughly mixed and melted, about six ounces of antimonial ethiops are put in, and the mixture is stirred continually, till it has assumed a suitable consistence. This plaster is then spread upon strong cloth. It is employed thus:—The hair is cut as short as possible, the crusts removed by means of cataplasms, and the plaster applied in strips, so as to fit the head accurately. It is left on for a few days, so that the hairs in growing became firmly adherent to

the plaster. The end of each strip is then seized and suddenly and forcibly removed, dragging along with it a number of the hairs. This operation is repeatedly performed, and extends over a period of several months in many cases. The practice has sometimes proved successful; but only so when, by it, the whole of the diseased hairs are removed, but, unfortunately, in many cases, this cannot be accomplished, and in too many others the operation is too painful and cannot be borne, and instances of death from this cause have been recorded. It is adopted, however, by Baumes, of Lyons, and by the Sisters of Charity at Toulouse, in which town, curious to relate, the municipal administration has interdicted the physicians from treating favus, the care of the children so affected being confided exclusively to the Sisters of Charity. This absurdity is carried to a great extent in Paris also, where the St. Louis Hospital counts the Freres Mahon (the quacks) among the medical staff. This was somewhat excusable many years ago, for then the parasitic affections of the head were certainly better treated by them than by others; but now that the treatment proposed by Bazin and adopted by Hardy is proved to be better than that of the Freres Mahon it seems to be highly improper, and a great insult to the distinguished physicians of that hospital, that such charlatans should still be retained in office. For of all kinds of quacks, those are decidedly the worst, who, having discovered a useful remedy for a loathsome disease, prevent the public from profiting by it, retaining it as a secret, for the purpose of advancing their own interests, and filling their own pockets. The process of the Freres Mahon is somewhat the following:—The hair being cut short and the crusts removed by poultices and soap and water, *a marvellous powder* (to use Bazin's expression) is applied and the hairs are then removed by combing, and by pulling out as many as possible with the fingers. Bazin remarks that the epilation by this means is often impossible, in which case they (the Freres Mahon) have no scruple in having recourse to his method of epilation with the forceps. According to the analysis of Chevallier, the depilatory powders of Mahon consist principally of slacked lime and the subcarbonate of potash.

The *best* way of curing favus is *decidedly* that by depilation and the application of parasiticides, as proposed and adopted by Bazin, and followed by Hardy and others. I have already explained so fully this method of treatment, when on the subject of tinea tonsurans, that it is not necessary to enter upon it here, as what we have said with regard to the treatment of the latter applies in every respect to the disease under consideration. The length of time required for a cure will vary of course according to the extent of the diseased surface. When it is very limited, a cure is effected in a few minutes, as in the following cases, cited by Deffis in a pamphlet bearing this eminently French title—"Refutation of the Errors which M. Devergie's Book contains:"—

"March 16, 1854.—Leon Dufour, aged six months. Two cup-shaped favus-crusts on the back part of the head. Cured in a single sitting, on the 16th March, 1854.

"December 9, 1854.—Julie Laporte, aged 10 years. One favus-cup on the summit of the head. Cured in a single sitting on the 9th December, 1854.

"March 30, 1855.—Alfred Hubert, aged 6 years. Four small favus-cups on the scalp. Cured in a single sitting, on the 31st March, 1855."

In these cases the number of crusts is certainly very small, but notwithstanding, there is no treatment but that under consideration which is capable of producing a *radical* cure in nearly so short a space of time. When the disease is very extensive, affecting the whole scalp, the depilation of the whole diseased surface must of course be undertaken and often repeated; and in such cases the duration of the treatment may extend over a period of from six to twelve weeks, in which the most obstinate cases should be *radically* cured; and this is what can never be effected under any other treatment whatsoever. I am well aware that in hospital practice many physicians are content with removing the crusts, and applying stimulating washes, attending to the general health, and dismissing the patients with *clean heads*, and *apparently* cured, after a couple of weeks treatment; but these are not

instances of cure at all, for whenever the local applications are stopped the crusts reappear. No favus can be said to be radically cured unless the head remains perfectly healthy for some months after the cessation of all local treatment. Those, however, who are conversant with the disease, and the treatment by epilation and parasiticides, soon learn when to pronounce a cure; for to an experienced eye the appearance of the skin of the scalp and of the hairs, more especially, is a sufficient index of the eradication or non-eradication of the disease. But on these points it is not necessary to enlarge further, for they have all been discussed previously. The method of removing the favus-crusts before proceeding to epilation is by the use of cataplasms, or by the application of oil, which softens them, and causes them to *swell* and lose their attachments to the skin, after which they may be scraped off with a spatula. This is the method adopted by Hebra. Instead of oil, he sometimes makes use of alcohol, which has exactly a contrary effect on the favus-crusts, causing them to *shrink*, and thus lose their attachments, when they fall out, or are removed with the spatula. With regard to the parasiticide lotions and ointments the remarks made with regard to them under the treatment of tinea tonsurans apply equally to favus. Küchenmeister makes a series of very interesting experiments, in order, as he says, "to test the parasiticidal effects of the most urgently recommended remedies," and he found that *alcoholic solutions* acted most powerfully in this manner:—At his request applications of alcohol were made to the heads of some patients affected with favus by Hebra, and apparently with great success. Notwithstanding this statement of Küchenmeister's, I find in a later publication that Hebra considers the application of the so-called parasiticides as useless, there being, he says, no medicine which is known to act in this way. This opinion is not shared, however, by most dermatologists.

Bennett, who is of opinion that the disease is parasitical, but occurs in scrofulous constitutions, recommends the use of cod-liver oil internally. Locally also, after the removal of the crusts with poultices, he applies cod-liver oil, which, as he says, by excluding the air, causes the death of the parasite. I am told, however, by my friend, Dr. Stewart, of Edinburgh, (formerly Dr. Bennett's clinical assistant), that this treatment is merely palliative, and certainly the cases cited in Dr. Bennett's valuable work on Clinical Medicine (Second Edition pp. 700–794) are not very encouraging.

After what has been said, it will be seen that the local is the only treatment capable of effectually curing favus; but at the same time, in this, as in all local diseases, the general health must be attended to, and any deviations from a natural state corrected when possible. Cleanliness must, above all, be insisted on, combined with the use of good food, and the enjoyment of exercise, pure air, &c. The principles of treatment above sketched out are always efficient in curing even the worst cases of favus; and we hope that, as it comes more extensively into practice, cases of favus described as *incurable* will cease to be recorded, for since the depilatory treatment of Bazin has been instituted, the word *incurable*, when applied to favus, has become quite inapplicable.

The remarks above made apply to favus attacking the hair-follicles. In the epidermic variety depilation is not necessary, the application of a parasiticide lotion, as a solution of the bichloride of mercury (two grains to the ounce of water), being quite sufficient to remove it. When the disease attacks the nails the treatment is also simple. It is necessary to destroy gradually by means of a small file the portion of nail covering the favus matter, and after arriving at it the application of a parasiticide is quite sufficient to destroy the fungous growth.—*Med. Times and Gaz., Feb.* 2, *and March* 9, 1861, *pp.* 112, 247.

## 65.—OBSERVATIONS ON FAVUS.

By Dr. William Pirrie, Jun., late Assistant-Surgeon H. M.'s 71st Highland Light Infantry.

Favus is an affection of the skin usually confined to the scalp, but occasionally found on the trunk, limbs, or face; almost invariably occurring before the seventeenth year, and peculiar to the poorer classes. It is characterised by the presence of more or fewer crusts, which, at first are of a uniform sulphur-yellow colour, have their superior surfaces slightly concave and pierced in the centre by a hair, and their edges slightly depressed beneath the level of the cuticle, and are surrounded by skin which, for some time previous to their appearing, presents a glazed, red, and vascular look, and is more or less covered with desquamated epidermic scales. The crusts, when isolated, have a more or less distinctly round or oval figure, and have in their upper surfaces a depression, in form like that of a lupine seed (porrigo lupinosa); but when numerous and confluent, they acquire, from contact with one another, a hexagonal or honeycomb appearance, and hence the term porrigo favosa. In the progress of development, the edges of the crusts become prominent, and marked by several concentric rings, whilst their upper surfaces gradually lose the concave and assume the convex form. In course of time, there may be observed in the centre of each capsule a whitish spot, which slowly extends into the surrounding deeper yellow portion, and ultimately reaches the edges of the crusts, which gradually sink to the level of the skin, and frequently present numerous cracks or splits. There speedily commences in this central white portion a loss of coherency, or a process of crumbling down into a coarse gritty powder; and when this, in the progress of the disease, has extended to the edges, the capsules lose all definite form, and become broken or split up into numerous pieces, presenting the appearance of one continuous crust irregularly fissured on the surface. The diseased mass crumbles down, and communicates to the fingers, on trituration, a feeling very aptly compared by some to that caused by the crushing of dried putty; becomes a favourite site for vermin and their eggs, and exhales a peculiar odour, which has been likened to that of cats or mice. For some time the disease occasions little uneasiness; but ere long the part affected becomes the seat of a disagreeable itching, which, gradually increasing, at length becomes so intolerable that the patient cannot resist incessantly scratching and tearing at the diseased mass, the consequence of all which is, that in cases of some standing, we usually meet with more or less sanguineous or serous effusion, with secondary pustular eruptions, and at times with violent inflammation, which in a few cases terminates in suppuration, and in other instances in unhealthy ulceration.

Such was the form and such the sequel of phenomena exhibited by all the examples of the disorder which came under the writer's observation; but we have it on the authority of Dr. Bennett that, on some few occasions, the disease, instead of assuming the form already described, becomes disseminated underneath the epidermis, presenting all the outward appearances of pityriasis or chronic eczema, from which affections it is to be diagnosed only by microscopic examination, which bring into view thalli and sporules in various stages of development. With reference to this variety of the disease, the authority already named remarks:—"In one instance I found the sporules smaller than usual, and perfectly globular, instead of oval. In others, I have seen the sporules three or four times larger than those of ordinary favus which included nuclei, multiplying fissiparously. Hence, the so-called parasitic pityriasis of the scalp I believe to be a modification of favus, and consider it a good rule, in all chronic eruptions on the head, to examine the crusts microscopically."

I would here remark that the admission of parasitic pityriasis as a modification of ordinary favus must greatly affect the generally expressed opinion regarding the rarity of the affection.

*Pathology.*—When a recently formed favus crust is carefully examined, it is found to consist of a capsule of epidermic scales, coated in the interior

with a finely granular matter, constituting a soil on which arise multitudes of a peculiar fungus, named achorion Schonleinii by Gruby, in 1841, in honour of its discoverer, Schonlein, of Berlin. From the granular mass sprout numerous cylindrical tubes (thalli), which extend themselves towards the centre of the crust, and, branching dichotomously, contain at their extremities (mycelia) numerous spherical or oval bodies termed sporidia. The thalli frequently contain numerous molecules or granules, and are most numerous near the exterior of the crust; whilst the mycelia and sporules mingled with more or less granular matter, abound at the centre, the whitish colour of which has been ascribed to the aggregation of the sporules. In course of time the thalli and sporules enter the hair follicles, cause atrophy of the hair-bulbs, and consequently the baldness observed in the subjects of favus, and, as I have myself witnessed under the microscope, at times extend into the body of the hair. The sporules are the bodies from which the plant is developed, and, from the observations of Remak and others, it appears that, in the process of elimination, their investing membrane gives off shoots or prolongations, which ultimately become tubes, enclosing at their extremities sporules, which in time are pressed out and become free. Seeing that the peculiar favus matter is found in relation with the derma and the epidermis, filling up the hair-follicle, and at times extending into the body of the hair, it becomes an interesting question to determine what tissue is the real seat of the disease.

On this point the following four opinions have been entertained :—

1st. Some, as Baudelocque, Duncan, Hutchinson, and Willis, consider it as a disease of the hair follicles or hair-bulbs.

2nd. Others, as Bateman, Thomson, and Rayer, give it as their opinion that the disease is at first confined to the reticular tissue of the skin.

3rd. Others, as Murray, Mahon, and Cazenave, state that the disease is located in the sebaceous glands.

4th. Gruby thinks that the plants grow in the epidermic cells, that the true skin is not destroyed, and that the hair-bulbs and sebaceous follicles are only secondarily affected; and Bennett conjectures that the sporules from which the fungi are developed penetrate into the crevices and underneath the portions of half-detached epidermis, and that they do not spring up at first either below or in the thickness of the cuticle.

From the opportunities afforded me of studying the disease, I have formed the impression that the peculiar matter of favus is an exudation on the surface of the derma; that this exudation becomes the seat of peculiar phytaceous growths, which, in the progress of development, penetrate the epidermis and become encysted by its scales; and that the hair-follicles are not the seat of the disease, but only become secondarily affected. That the hair-follicles are not the seat of the disease I have concluded from observing—

1st. That on carefully removing recently-formed favus crusts from the scalp, the only thing to be seen was a slight depression on the derma without any abrasion, which would have been expected were the crust an exudation in the hair-follicle. In the centre of this depression may frequently be observed the open mouth of a hair-follicle, which is more deeply placed in the reticular portion of the corium proper.

2nd. That it is possible to raise from the scalp a crust of short duration, and to draw it over the hair, leaving the latter standing in its follicle.

3rd. That permanent baldness does not invariably follow favus, which would reasonably have been looked for were the hair-follicles the primary seat of such a serious disease. In some instances, the hair, though weak and altered in texture and colour, does appear after the removal of the complaint on the parts which were affected. It has occurred to me that in many cases of favus which have not been left long without treatment, the hair-follicle, from being below the original seat of the exudation, is not obliterated or destroyed, but only impeded or impaired in function, and the hair in consequence temporarily destroyed; but that, on the removal of the disease, *the follicle* again resumes its wonted function, and the hair in time becomes *regenerated*.

4th. That favus on some few occasions occurs in parts where it is impossible to detect any hairs piercing the crusts. Thus in one case under my father's care in the Royal Infirmary, there were several large well-developed crusts over the right shoulder-blade of a little sickly boy about nine years old; and in this instance, not only were the capsules not formed around a central hair, but no hairs could be observed penetrating them at any point.

[That favus can be communicated by inoculation, is placed beyond all doubt by the experiments of Bennett, Remak, and Mahon, and that it does at times spread by contagion is attested by many practitioners. But the question, whether the disease does not often arise without any contagion, is still unanswered. The author has seen five cases, none of which could be traced to contagion, and he believes that favus very often arises without any contagion, and if contagious, is very feebly so. It is probable that favus has a very intimate connexion with the strumous habit, for the following reasons:—]

1. In all the cases there were marked impairment of health; decided depression of the general powers; a thin, weak, and badly-nourished state of body; a sickly white colour; much gastric derangement, more especially acidity, and great irregularity of the bowels. In fact, the general appearance and condition of the sufferers were such as warranted the observers in at once pronouncing them of the scrofulous diathesis.

2. Although the general aspect of all was decidedly strumous, in four there were the following more glaring manifestations of that constitutional taint:—Enlargement of the cervical lymphatic glands, of much older date than the eruption; scrofulous enlargement of the sub-maxillary gland; cicatrices of old scrofulous sores on the arm, and incipient pulmonary tuberculosis.

3. On inquiring more particularly into the histories of the cases, more or less of a scrofulous connexion was always discovered, some one of the patient's family or relatives either being at the time of investigation, or having been, affected with some form or other of scrofulous complaint.

4. All the parties affected had been living, for periods of longer or shorter duration, in circumstances well known powerfully to predispose to the strumous diathesis, such as exposure to cold and damp, hardships, overcrowded dwellings, low and confined localities, imperfect or neglected ventilation, and improper and deficient food. One circumstance particularly worthy of notice in regard to the food of the lower classes in this part of the country is, that oatmeal forms the staple article of diet, and that in the case of very many, it constitutes, in the form of porridge or of cake, the three meals of the day. In the case of others, the afternoon repast is varied by barley broth, either plain, or containing some green vegetables, and, at times, scanty fragments of lean meat; but one feature common to all their diets is the exclusion of the oils, the very substances which form our sheet-anchor in the treatment of scrofulous affections.

5. None of the cases were cured by local applications independently of constitutional treatment.

But as many hundreds of persons placed in the same circumstances suffer from phthisis, or some other form of tubercular disease, and yet never become affected with favus, there must surely be some peculiar and powerful predisponent to the disease. I have thought that this might be arrest or retardation of the cutaneous functions; for one feature common to the histories of all the cases which have come under my notice is, that the sufferers had been living, or more properly, in the case of some, had been allowed to live, in total neglect of cleanliness, almost perfect strangers to ablution, and in a state of perpetual filth. From all these circumstances I am disposed to think that favus is a peculiar and not very usual manifestation of the scrofulous diathesis; that the determination to the skin is owing to perverted nutrition of that tissue from arrest or retardation of its functions caused by long-continued want of cleanliness, or some other irritating agency: that, though traced *in some instances* to contact, still it often arises with-

out contagion; and that the parasitic fungus is a spontaneous growth in the peculiar exudation which constitutes a soil congenial to its development.

*Treatment.*—Not to mention the various measures and appliances recommended for the removal of the hair by those who consider favus to be a disease of the hair-follicles, two distinct methods of treating the complaint may be briefly noticed.

1. Some, who consider that the disease is not associated with any particular diathesis, and whose experience it has been to meet with examples of it in states of moderately good or even rude health, look upon constitutional remedies as altogether unnecessary in ordinary cases, and rely for the removal of the affection entirely on local applications, recommending the adoption of certain substances believed to possess the power of destroying vegetable life, such as mercurial ointment, sulphur ointment, sulphur and ammoniochloride of mercury ointment, creosote, and various other preparations.

2. Others insist on the necessity of administering constitutional remedies conjointly with the employment of local means, and follow and recommend this method of treatment from the belief that the growth of the plants already formed may be destroyed by the adoption of certain local measures, but that the existence of the exudation or soil congenial to the development of the cryptogam is owing to a peculiar vitiated condition of system which must be corrected by appropriate means before a continuance of the disease can be prevented.

The two great indications to be fulfilled by local remedies are—

1st. To remove the crusts from the portion or portions of integument affected.

2nd. To prevent the development—to destroy the life of the fungus.

The first indication is very easily accomplished by the use of warm poultices, or other emollient applications.

The fulfilment of the second indication has been attempted on two different principles, both of which have undoubtedly been effectual in many instances:—

1st. Some have succeeded in checking the growth of the fungus by keeping constantly applied to it certain irritating substances, as creosote, sulphur ointment, sulphur and ammonio-chloride of mercury ointment, sulphurous acid lotions, and other preparations, the beneficial action of which is attributed to their having inherent in them certain properties powerfully and directly injurious to vegetable growth.

2nd. Others have been equally successful in effecting the death of the parasite on the principle of entirely and steadily excluding the air from it, and for this end they use substances which, in themselves, are by no means injurious to vegetable life. Thus Bennett instituted the local application of cod-liver oil with this view; and expresses the belief that common lard or any other oil would be equally efficacious.

As all the cases which have come under my observation here have been distinctly connected with a cachectic or scrofulous habit of body, one important indication of treatment has been the correcting of this condition by the employment of constitutional remedies. For this end, the adoption of all means calculated to maintain a proper condition of the digestive organs and to improve the general strength, the injunction of free ventilation and exercise in the open air, the rigid enforcement of personal cleanliness, and the administration of nourishing diet, tonics, and cod-liver oil, are indispensable. In one word, in the various cases which I have carefully watched under my father's treatment, and in one or two under my own particular care, I have found the energetic adoption of theanti-scrofulous regimen and remedies with all their details, together with the local application of cod-liver oil, recommended by Bennett, attended with such thoroughly satisfactory results, that I would unhesitatingly have recourse to their exclusive employment in any case for the future.

*Summary of Conclusions.*—1. That favus is essentially characterized by the presence of a fungus, which is easily discovered by the microscope.

2. That it is peculiar to the young, and confined to the poor and destitute.

3. That it is most commonly met with on the scalp, but occasionally on other parts of the body.

4. That the hair follicles are only secondarily affected.

5. That it is by no means a rare disease in Scotland, being exceedingly common in Edinburgh, and having been more so for several years past in Aberdeen than in Glasgow.

6. That it is generally considered more common in Ireland than in England.

7. That it is a blood-disorder, and that the fungus is not the sole nor the original cause of the eruption.

8. That many are insusceptible to it; and that it is feebly contagious, and very often arises independently of contagion.

9. That the previous state of health has an important bearing on its outbreak.

10. That it is intimately connected with the strumous diathesis.

11. That want of cleanliness strongly predisposes to it.

12. That, for its removal, general as well as local treatment is necessary. *Lancet, Dec.* 8 *and* 15, 1860, *pp.* 557, 582.

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66.—*Tinea Favosa treated by the Carbonate of Copper.*—The salts of copper are becoming gradually introduced more largely into practice; and the above new application of them has been brought forward by M. Huet, physician to the Penitentiary for Juvenile Delinquents. A great number of children enter that institution affected with tinea favosa, and the good effects obtained in these cases, by the trial of his mode of treatment, has induced M. Huet to recommend it strongly. It consists as follows:—The head is carefully cleaned by means of unctuous cataplasms, and, when the crusts have been completely detached, it is shaved. The following ointment is then applied: Carbonate of copper, 2½ drachms; purified axunge, 15 ounces. After numerous trials, M. Huet considers this ointment as the means which cures porrigo with the greatest rapidity. Sometimes it is necessary to suspend the medicine, and return to the poultices for a certain time, before resuming the use of the carbonate of copper.—*Repertoire de Pharmacie—Bull. Gen. de Therap.*, 1861.—*Edin. Med. Journal, March*, 1861, *p.* 834.

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67.—*Treatment of Prurigo.*—During the year 1860, sixty-eight cases of prurigo were treated in Professor Hebra's clinique. Most of the patients had been under treatment for the same affection on previous occasions. The disease, according to Hebra, occurs only in poor people, who pay little attention to the state of their skin. Any measures which will soften the skin will remove the papules and itchiness. The treatment recommended is warm baths with plenty of soap, and, after each bath, rubbing into the skin an ointment composed of equal parts of cod-liver oil and tar. The itchiness almost invariably ceases after the second or third application.—*Spitals-Zeitung, No.* I., *Jan.* 5, 1861.—*Brit. Med. Journal, Feb.* 23, 1861, *p.* 207.

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## 68.—ON THE TREATMENT OF ECZEMA.

By Professor Hebra.

[The following is extracted from a Clinical Lecture on the subject.]

One has the satisfaction of knowing that we can always cure eczema, however long it may have continued, although in some inveterate cases depending upon a dyscrasis, or upon internal causes, there may be considerable difficulties in the way. As in most other diseases of the skin, the treatment should be purely local, internal measures being limited to the rare cases in which the eczema has been produced by a previously diseased condition of the economy, or when it is combined with some other affection. In the great

bulk of cases internal means, such as mercury, antimony, iodine, purgatives, sarsaparilla, &c., are superfluous and mischievous. Arsenic is the only one of such means which exerts any influence in obstinate cases. Ordinarily, however, it is of no use, and its employment should be limited to the few cases which manifest especial obstinacy. The local treatment even in these also not being at the same time neglected.

Cold water, in its various modes of application, is of great importance, combined with other means, in the treatment of eczema. Employed alone it is far too tedious, and frequently not sufficing in its operation. It only aggravates the malady when applied in *eczema simplex*, arising from excess of secretion, as *e. g.*, in the axilla, between the buttocks, &c. Starch, whether alone, or mixed with oxyde of zinc (starch ʒj, zinc ℥ iij) is, on the other hand, an excellent application when the eczema arises from the friction of two cutaneous surfaces, or from excessive secretion, as in the axilla, under the breasts, the scrotum, buttocks, &c. Oxide of zinc ( ʒj, ℥ ij of lard), sulphate or acetate of zinc, alum ( ʒj to lbj of water), red or white precipitate (6 to 12 grains to ʒ ij of lard), are of good service in acute eczema, or in chronic, when there is but slight infiltration, and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost indispensable *schmierseife*, by reason of its slightly caustic action, becomes the most preferable remedy. Its mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar.

In many chronic cases, with great infiltration, the soap does not suffice, and we must have recourse to a stronger form of potash, viz., caustic potash, 1 drachm dissolved in 2 drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. These are then washed, and cold applications are kept constantly employed. One or two such cauterisations usually suffice, and when more are necessary they should not be repeated oftener than once a week. Carefully applied, the caustic gives rise to no cicatrix, but the practitioner must himself always undertake its application. It is a very painful procedure, and is not often required. Cauterising with strong acids is to be avoided, as it causes great pain and gives rise to scars. The application of the nitrate of silver is of no avail. *Tar* is in its way just as useful a remedy as the soap, the time for its application being when the moisture and itching have ceased, and exfoliation has commenced, *i. e.* when *eczema squamosum* is present. It may be used either alone or mixed with equal parts of cod-liver oil, and should be applied by means of a brush twice a day, carefully avoiding washing the parts or allowing water to come into contact with them. As long as any redness or desquamation continues, the tar must be repeated. Sometimes, when the application of the tar has been premature, moisture and itching are observed at certain spots, and the preliminary treatment has then to be resorted to again. Some individuals cannot bear the tar at all, it giving rise to severe inflammatory action. An ointment of acetate of lead or oxide of zinc should in such cases be substituted. *Cod-liver oil* is a valuable external application, and by its aid alone we are able to cure the eczema when this has not lasted very long and the infiltration is not very considerable. It is also an excellent adjuvatory to the treatment by *schmierseife* and cold applications, as flannels soaked in the oil may be kept bound over the diseased parts during the night. Employed alone, the treatment is very tedious, and

is objectionable on account of the disagreeable smell and befouling the linen which it gives rise to. Taken internally, it does not exert the slightest influence on eczema.—*Weiner Spitals-Zeitung.—Med. Times and Gaz., Feb.* 16, 1861, *p.* 179.

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## 69.—ON ALOPECIA AREATA.

By Dr. T. McCall Anderson, Fellow of the Faculty of Physicians and Surgeons of Glasgow.

[This disease is likewise called Tinea, or Porrigo decalvans; and by Cazenave, Vitiligo capitis. The parasite is the *Microsporon Audouini.*]

Alopecia areata is one of the most interesting, as well as the least understood, of the parasitic affections of the skin. A disease confined to those parts of the cutaneous envelope provided with hairs, it attacks principally the scalp; but the beard, the genital organs, and the hairy parts are also exposed to its ravages. Although not in any way dangerous to life, it is yet a rather alarming disease from the deformities which it may occasion; sometimes continuing to flourish until every particle of hair on the body is destroyed. The phenomena of this curious disease are best studied on the scalp. It consists essentially of the formation of rounded or oval patches of baldness, sometimes solitary, but more generally multiple. These patches commence, according to Willan, by the production of pustules, which are *very ephemeral.* They have, however, never been seen by other observers; and, indeed, it seems doubtful if Willan himself ever saw them.

On examining the hairs of the affected parts in the early stages, they are found to be dull and lustreless, and more easily extracted than healthy ones. The skin from which they proceed is found to be slightly reddened, and it is also the seat of a peculiar swelling having the appearance as if the subcutaneous tissue was œdematous; but it does not pit on pressure, as in true œdema. At this early stage, there is a whitish matter on the diseased skin and hairs, which is nothing else than the parasite which occasions the disease. It is not, however, in nearly such abundance as in Herpes tonsurans, a disease which it resembles in many respects. At the commencement itching is complained of, which is generally slight, so much so, indeed, as often to excite little attention, so that the disease is not observed, and continues its ravages till baldness has been produced. This first stage is of short duration, and is followed by the falling out of all the hairs of the affected parts, which takes place very rapidly, leaving a rounded bald patch. By this time the faint red tinge of the scalp has completely disappeared, being replaced, according to Bazin, by a condition exactly the opposite; that is to say, it has become perfectly white, having apparently lost every particle of its pigment. This white colour certainly contrasts strongly with that of the parts of the scalp provided with hairs; but it seems probable that this white colour of the bald patches is not so much owing to any defect of pigmentary secretion as to the loss of the roots of the hairs. After the hairs have fallen out they are replaced by a number of fine short downy ones, resembling the down on the cheeks of infants. By-and-by even this down disappears, the slight swelling of the subcutaneous tissue subsides, and we now see merely the rounded patches of baldness, the skin of these parts being smooth and white. The disease is often very limited, there being only one small patch on the scalp; but more frequently there are several, and these, at first small, have a great tendency to increase and involve the neighbouring healthy parts by circumferential extension, so that at last a number coalesce, forming one large patch, having a serpentine form; hence the name ophiasis, sometimes applied to this affection. In this way the whole of the scalp may be attacked, and permanent baldness produced. Instances have been met with, as before observed, of the complete removal of the hair from the scalp, eyebrows, beard, and genital organs, so that not a single hair remained.

*Treatment.*—Let us suppose that we have a circular patch of alopecia areata on the scalp: the first thing to be done is to prevent it from extending. This is to be done by causing all the hairs within a couple of lines of

the circumference of the patch to be carefully extracted. For those at the edge are very frequently, to a certain extent, impregnated with the parasite, though not so much as to render the depilation difficult; or, if not actually diseased, they are very apt to become so, as the affection spreads by additions to the circumference of the patches, and they are therefore much better away. The head, also, should be washed daily with black soap, as cleanliness is of great consequence in affections of the head, and the hair should not be kept too long. We must now apply ourselves to the diseased surface. All the downy hairs—if there are any, which is not invariably the case,—are to be removed; and this must be effected very carefully, according to the rules already laid down, for they are exceedingly friable and very apt to break off, leaving the diseased roots in their follicles. This depilation of the downy hairs must be persevered in and continued till the healthy hairs begin to make their appearance. Along with the depilation of the downy hairs, and of the hairs surrounding the diseased patch, a parasiticide lotion or ointment must be applied. One of those already alluded to (under tinea tonsurans) should be used daily, and probably the bichloride lotion, or alcohol, as recommended by Kuchenmeister, is the best. The whole head may also be occasionally washed with one of these solutions, so as to destroy any of the spores which may be entangled in the hairs but have not had time to take root. If the disease is far advanced, and no down is seen on the bald patches, the application of a blister may be made, as is practised by Jenner and Startin, and is sometimes useful, but it must be repeated to be successful. A common fly blister may be employed for this purpose, or the acetum cantharides or collodium vesicans. The acetum cantharides should be applied, on a sponge, to the part, and produces vesication in about ten minutes or a quarter of an hour. The collodium vesicans, a mixture of equal parts of collodium and ether cantharidalis, should be applied with a glass-brush, and two scruples are sufficient to blister a surface the size of the palm of the hand.

The vesicant which I am in the habit of using most frequently is a strong solution of the bichloride of mercury (a drachm of the bichloride dissolved in seven drachms of alcohol). A piece of blotting-paper may be soaked in this solution, and applied to the part affected for five minutes; after which, cotton wad is substituted for it, and vesication is certain. There is no danger of salivation, as some might suppose.

Instead, however, of vesicating the affected parts, we may confine ourselves to the application of the more slowly acting parasiticide lotions or ointments, and to circumferential depilation; but very frequently, although we may thus prevent the disease from extending, we cannot cause the hairs to grow on the bald patch, for, when long left to itself, the parasite destroys the hair-follicles, and permanent alopecia is the result.

It is necessary, at the same time, as the disease is attacked locally, to attend to the general health, to correct any disorder of the system, if such exists, and to endeavour to change that state of system favourable to the growth of the parasite.—*Med. Times and Gazette, March* 23, 1861, *pp.* 298, 300.

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70.—*On Local Applications in Urticaria.*—I have found so much relief and such rapidly successful results from the local use of the liquor plumbi subacetatis in attacks of urticaria, that I wish to direct attention to it. The extreme uneasiness and irritation which this eruption causes in children is well known. Even adults suffer sometimes severely from it, and although from its evanescent character, it is not so frequently brought under medical notice as more persistent yet less distressing affections, still it happens that we are asked sufficiently frequently, what will best relieve the intolerable sensations that it produces. Of course I do not wish in any degree to undervalue the employment of such constitutional treatment by alkalies or aperients, as may seem desirable, or that in relieving the irritation caused by the eruption, our attention should be diverted from these numerous exciting causes of an attack, such as fruit (especially I think hazel nuts and fresh

gooseberries,) fish, shell-fish, &c., and which, by producing gastric derangement, are distinctly connected with the appearance of the rash.

I have found a mixture of liquor plumbi in an undiluted state, mixed with one or two parts of glycerine to be most useful as a local application; the advantages of this mixture are considerable, for if we apply liquor plumbi alone to the skin, although it does speedily relieve the itching that is caused by the eruption, yet we find it is very difficult to wet the surface with it; in fact, it flows off the skin as if the part were oiled, and a repetition of the application becomes necessary; this is greatly obviated by combining glycerine with the liquor plumbi. I have now used this remedy for a long period with success, and I am not without hopes that it will likewise be found of benefit in many other cutaneous diseases; it would be worth trying on a large scale in erysipelas and small-pox, in neither of which affections have I used it as yet sufficiently.—*Dublin Hospital Gazette, Dec.* 1, 1860, *p.* 365.

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## 71.—ON A CASE OF BLOODY SWEAT.

By Dr. Thomas K. Chambers, Physician to St. Mary's Hospital.

[The following is a most singular case—a most rare form of vicarious menstruation. The patient is described as short and stout, twenty-seven years old. She has suffered much from headache and dyspepsia. She has only menstruated twice, and that in consequence of the application of leeches to the uterus; and at or immediately after the last period (three and a half years since) the peculiar symptoms from which she suffered first appeared.]

She feels first a peculiar soreness and tenderness of an isolated spot, which enables her to predict that in the course of a few hours an eruption is going to commence. The first appearance of this is an erythematous blush, sometimes slightly raised above the surrounding surface, but not so much as in erysipelas. After an uncertain time, seldom more than a few hours, there may be detected a scattered crop of fine vesicles, like sudamina, mixed with a fine serous dew, uncovered by any pellicle. This never lasts long enough to form colourless drops, for quickly it becomes blood-stained, and then little points of blood are seen oozing out, sometimes so slowly as to dry and form a scab, sometimes collecting into great thick gouts, and trickling in a ghastly way down her face. The eruption runs through its stages quickest when she is in bed, and especially during sleep. Suspecting from the strangeness of this story that there was some collusion, I had the bed watched, and the nurse saw the blush come, and the blood ooze out in the manner described while the patient was fast asleep. If rubbed, washed with water, or otherwise interfered with, the bleeding is much increased and prolonged; but if left alone to dry in a scab, it stops in a week or ten days, usually, however, to be succeeded, before it is quite recovered, by a similar eruption in another place. Sometimes, at irregular periods, there was an interval of a week or a fortnight; sometimes the cutaneous phenomena were replaced by bleeding from the nose, sometimes by vomiting of blood, but never by hemorrhage from either lungs or bowels. These symptoms continued nine months, and were relieved by anticipating the eruption of blood, with leeches applied to the spot where it was expected. The discharge became serous, then was like little blisters, and finally ceased when her health was re-established by the sea air of Margate.

She continued well till last September, when she was admitted here as a mild case of erysipelas of the face. It really looked exactly like erysipelas; but it puzzled us a good deal when it began to bleed, and has continued to puzzle us ever since, as regards its nomenclature, pathology, and treatment. She was seldom seen free from cutaneous hemorrhage from that period till four weeks ago, but it has varied a good deal in violence. During this attack, the face has not been the only part affected. When she lies down much in the day, that, indeed is almost always the locality where it has appeared; but when she is about, the legs and thighs have exhibited like ap-

pearances; both forearms, too, and once the chest, were attacked. The loss of blood, however, was less than on the brow. Examined under the microscope, the fluid exuding from the skin contained blood-discs in a natural state—blood-discs with roughened edges and shrunken, much granular matter, dark fatty-looking specks, and scales of epidermis. It did not coagulate into rolls. Blood drawn from a prick in the finger looked perfectly natural, and coagulated into rolls, leaving the usual number of pale globules free. She has twice thrown up from the stomach about half a pint of dark brownish purple sanguineous fluid, and I have occasionally seen her pocket-handkerchief stained with blood reported to have come from the nose. She states that she has been hysterical for the last year, and has had occasional paroxysms of crying and low spirits during her residence here, but with that exception there are no visible disorders of the bodily functions. The action of the bowels is free, the urine pale and sufficiently copious, the appetite very small, especially for meat. But in spite of the little she eats, the constant loss of blood by the disease, and a persistence in a very lowering treatment, she has got very fat—pale, it is true, and flabby, but decidedly fat,—and not weaker in muscular power than on admission. In the last month, during which her ailment has been declining, she has been less hysterical, and had somewhat more appetite. She tells me that a similar increase of flesh occurred during her last illness, three years ago. There is no leucorrhœa, and examination per vaginam fails to detect anything abnormal in the parts which can be explored with the finger. She complains of pain, and becomes hysterical during the process. The lungs and heart appear quite healthy.

This appears to me a case of cutaneous hemorrhage, or, in homely Saxon, "bloody sweat," vicarious to suppressed menstruation. Some physicians have fancifully attributed to suppressed menstruation every kind of increased secretion or congestion which accompanies the derangement of uterine function, and the female public are still more disposed to endorse this imaginative pathology. I am far from agreeing with them, and would restrict the explanation to those cases where a flow of blood from a distant part not organically diseased follows (*follows*, not precedes) a defective condition of catamenia, and where this flow of blood is a relief to previous symptoms, and not weakening to the patient. The most frequent forms under which this vicarious menstruation occurs are, hæmatemesis, epitaxis, hæmoptysis, and hemorrhoidal flux, instances of which you may fairly expect to see in the hospital during your pupilage; but it is a chance if any of us see again a case where the discharge occurs through the external skin, and it behoves us therefore to seize the opportunity of taking note of the phenomena. My opinion of its rarity is formed on the difficulty I have experienced in finding records in authors who write on allied subjects. I suppose, since the invention of printing very few examples of so strange a thing can have escaped being put into type; yet Haller, in his exhaustive work, can only cite eighteen from the industrious collectors of physiological curiosities during a century and a half. After him, Van Swieten ("Commentaries on Boerhaave," sec. 1286) quotes a case from Boerhaave's MS. remains, of a young girl who from her twelfth year menstruated through an eruption of pustules in various parts of the surface, which pustules, after the menstruation, entirely disappeared. Pinel, in his "Nosographie Philosophique" in 1802, repeats Haller's collection, but evidently cannot then cap the cases with any later ones at all to our purpose. In Hufeland's "Journal der Practischen Arzneykunde," there is mentioned a woman who menstruated through a scar of an abscess at the navel at her proper periods for two years; and a girl is said to have told a Mr. Gutgesell that her menses always appeared through a red spot over her knee. In the "Archives Générales de Médicine," an. 1829, (t. xix., pp. 212 and 236), are two cases recorded: one of a young lady, who, after ten years' suppression, menstruated for three years through a vesicular eruption in one finger; a second, of a prostitute, in whom the discharge occurred through spots of the size of a five-franc piece, which appeared from *time to time* one after another on the breast, in the axilla, on the back, the

buttocks, and the epigastrium. This is called by the term I have to-day used, "bloody-sweat," and the description accords closely with that of our patient, especially in the eruption being less periodical and more continuous than happens in most vicarious menstruations. The uterus also was healthy, for she became pregnant and bore a child.

In 1836 I find a rather fuller account of a case related in "Schmidt's Jahrbucher," by Heusinger. The woman had diseased ovaries and recto-vesico-vaginal fistulæ, and though sometimes the catamenia appeared at the proper place, they were generally arrested there, and appeared in a variety of parts of the external skin. The most common part was, as with our patient here, the face. She had suffered five years, and been in several hospitals. Heusinger says that she was so hysterical that she would have made a capital subject for mesmerism: whether he means that he suspected her of imposture I cannot say. M. Lheretier in his Traité Complet des Maladies de la Femme (Paris, 1838), cannot add any case from his own knowledge, but he quotes one recorded by M. Gardien, as reported by M. Brulé, of a young girl with suppressed catamenia, who had periodical monthly hemorrhages from vesicles in the legs for six months, from an eruption on the left arm for a year, through a sore on the thumb for six months, by the upper eyelid for two years. This last was preceded by erysipelas, as was also an eruption of the same nature by the navel. She had also hæmatemesis and other vicarious hemorrhage not to the present purpose. Curiously enough, this identical case is again repeated by M. Brierre de Boismont, and said to be quoted from Pinel's Medicine Pratique, but I cannot find it in that author's works at all. It is doubtless our old friend of Lheretier, who also again turns up in Meisner's "Diseases of Women," and there gets made into two persons by having her history quoted separately from each author. The last-named learned and valuable work adds no instances from contemporary observation of menstruation by the healthy skin; and I believe the same may be said of more recent authors. The chatty Meigs and the judicious West do not seem to have seen it.

The literature of the subject is scanty simply because the number of patients is scanty: and I cannot say I have learned anything new from books which this one case had not already taught, though they have given me the satisfaction of agreeing in almost all points of doubt. With their help, and the case before us, I think the following conclusions may be arrived at:—

1. Cutaneous menstruation occurs in robust and healthy-looking, rather than in anæmic persons. It appears, in fact, to be a plethora.

2. It may occur without any detectable disease of the parts of generation; instance this girl, the prostitute mentioned in the Archives, who had a child, and Mr. Gutgesell's case who was going to be married.

3. But may also be caused by disease in those parts.

4. The catamenia are not necessarily entirely diverted from their usual channel.

5. Periodical cutaneous hemorrhage is not necessarily connected with the catamenia at all. Thus Van Swieten quotes from Beneventus the case of a man, who once a month sweated a great quantity of blood from the healthy skin of his right flank.

6. It is not always strictly periodical.

7. *Suppressio mensium* may go on for many years without any tendency to vicarious menstruation. Of this you see frequent instances in cancerous disease of the uterus; and a remarkable one in a female, now under my care in Victoria ward, who never had a womb at all, and in consequence has never menstruated either naturally or vicariously.

8. Cutaneous menstruation usually appears in the shape of a slightly-swelled erythema, painful and tender, on which vesicles form; the vesicles quickly burst, and there is poured forth a sero-sanguinolent exudation, which then becomes bloody, and continues to exude like a bloody sweat for various times, from four days to a fortnight. No scar is left, but the skin is slightly discoloured for some time afterwards in the parts affected.

9. Hence I do not know if we ought to call it a disease of the womb any more than a disease of the skin.

10. Bloodletting is the most efficacious treatment; and it is most efficacious in the form in which it is known to produce its most powerful physiological effect—namely, in small repeated doses, and as close as possible to the seat of action—that is, on the spots affected. The measure of the good effect of the bloodletting is the relief experienced by the patient without loss of strength.

11. No other remedies are as yet known to be of any advantage.—*Lancet*, *March* 2, 1861, *p.* 207.

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## 72.—ON OPENING DEEP ABSCESSES.

By JOHN HILTON, Esq., F.R.S., Surgeon to Guy's Hospital.

[The following extract is from a course of lectures delivered by Mr. Hilton in the Theatre of the Royal College of Surgeons. The question as to what is the best and safest method of opening a deep abscess is one of the greatest importance.]

Persons have died, and many lives have been endangered, by hemorrhage consequent upon opening a deep abscess by the lancet or bistoury; and within the last twelve months I have known two lives placed in great jeopardy by the use of the lancet to open deep abscesses. These circumstances alone will, I hope, be sufficient excuse for my pressing this subject on your indulgent attention; and my own long experience of its value justifies the high opinion I entertain of the method of opening a deep abscess which I am about to recommend to your notice as the safest and best. It is safest, because it is scarcely possible to inflict by it any injury on bloodvessels or nerves. I think it is the best plan too; for if the deep opening into an abscess be lacerated and bruised, it is not at all likely to close by adhesion; and undoubtedly, when we have opened a deep abscess, we are desirous that the deep opening should not close immediately; and in principle, all will admit that if a lacerated opening be made instead of an incised one, that will be one of the circumstances adverse to a quick readhesion. Nay, more: supposing a lacerated wound to be made deep in the thigh, for example, and a bloodvessel divided by being torn asunder, it would be a lacerated wound of the bloodvessel, and therefore not so likely to bleed as an incised wound.

Some surgeons, when speaking of deep abscesses, say, with rash confidence, "Plunge in a knife." It is a grand term—"Plunge a knife into a deep abscess." It is an heroic, not a courageous plunge for the surgeon; for it is without danger to himself, but perhaps it is a fatal stab to the patient. Some more careful surgeons say, "Wait until the abscess comes nearer to the surface, so that it can be opened without danger;" but the patient may die in the meantime. Now the plan I have been in the habit of adopting and recommending is this—for example, in the case of opening a deep abscess in the axilla,—cut with a lancet through the skin and cellular tissue and fascia of the axilla about half or three-quarters of an inch behind the axillary edge of the great pectoral muscle; at that part we can meet with no large bloodvessel. There is only a small branch of one of the external thoracic arteries, which sometimes runs along the edge of the axilla; excluding that, which if wounded can be easily ligatured, we run no other risk, so far as I can see. Then push a grooved probe or grooved director upwards into the swelling in the axilla; and if you will watch the groove in the probe or director as it is being passed up through the comparatively healthy tissues into the axilla, a little stream of opaque serum or pus will show itself. Then take a blunt (not a sharp) instrument, such as a pair of "dressing forceps," and run the closed blades along the groove in the probe or director into the swelling, and by opening the handles you at the same time open the blades situated within the abscess, and so tear open the abscess; and, lastly, by keeping the blades of the forceps open during the withdrawal of the instrument, you leave a lacerated track or canal, communicating with the collec-

tion of pus, which will not readily unite, and which will permit the easy exit of the matter. In this way you may open an abscess deep in the axilla, or in other important parts of the body, without fear of inflicting any injury upon the patient. Having been connected many years with a large hospital, I have necessarily had good opportunities of trying this method. I have not opened a deep abscess during that time in any other way, and I can say, honestly and truly, that it has never failed, and that I have never observed any inconvenience from it.—*Lancet, Nov.* 10, 1860, *p.* 454.

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## 73.—ON THE INFLUENCE OF REST IN THE CURE OF ABSCESSES, SINUSES, AND ULCERS.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

[Much of the difficulty experienced in curing abscesses, and sinuses leading from them, lies in the frequent motion to which many are subject, either by adjacent muscles, or by fascia covering them. For there is probably no fascia in the body which is not influenced by some muscle. Physiologically and surgically considered, fascia is the insertion of muscle.]

*Sub-occipital Abscesses.*—Abscesses under the occipito-frontalis muscle are sometimes very large, and resist surgical treatment for a very long period. Why is this? Let me remind you that the areolar tissue in which this abscess exists is abundant, and that the whole superficies or dome of the abscess is under the influence of the occipito-frontalis muscle. Hence there is no rest to the abscess; it never has a chance of quietude, or of accurate and persistent coaptation of its surfaces, from the disturbance produced by the movements of this muscle. That such abscesses are difficult to heal will be admitted when I mention the particulars of some cases; and I will endeavour to demonstrate to you that in the surgical treatment of such cases the great object should be to keep the occipito-frontalis perfectly quiet. I would say, then, that chronic abscesses or sinuses under the scalp may be cured by keeping the occipito-frontalis quiet and at rest by strapping.

In illustration, I may mention this case to you. Last spring I was requested to see a stout gentleman, aged fifty, who had fallen down the hold of a ship. He had been taken to the London Hospital, where his wound was well dressed. The anterior half of the scalp was injured; it had been turned forwards and downwards, quite over his face, by the accident. This flap was replaced in accurate position, bandaged, and strapped, and the patient was sent home. Not any primary adhesion took place in the wound, and in a few days his surgeon thought it right to take off the dressing, when it was found that the scalp itself was much swollen, and that the whole of it was lifted up or raised from the bones by suboccipital suppuration. The patient had subsequently two severe attacks of hemorrhage, from sloughing and ulceration of the temporal arteries, which required ligatures; and it is worthy of remark that the bones of the cranium were denuded of pericranium to the extent of several inches, and were daily exposed in this condition during nearly a fortnight. Yet the bones did not die, their nutrition being derived from the blood supplied by the arteries of the dura mater. The wound was dressed daily with lint and warm water, and the pus squeezed out from under the whole of the occipito-frontalis; but the abscess would not heal, and the question was, how to get the pus from under the posterior part of the scalp—for there was no outlet for it in that direction—and how to secure rest to the movable dome of the abscess. The hair upon his scalp was shaved off, and long strips of plaster were so arranged across and around the head as to empty the abscess, to keep the occipito-frontalis quiet, and to press it downwards upon the pericranium and bones: this was done simply for the purpose of giving rest to the parts. The rapidity with which the abscess then healed was very remarkably in contrast with the tardy results of previous daily dressing and emptying the abscess. He quickly got well, and without any necrosis of bone.

*Cervical Abscess under the Platysma Myoides.*—Abcesses in the neck, under the platysma myoides, are well worthy of your attention. When these abscesses are opened either by the surgeon or by nature they are sometimes very difficult to heal; and I believe the great difficulty depends upon the fact that they are under the influence of the platysma myoides. About two months ago, my friend, Dr. Daldy, brought to me the son of a clergyman, who was suffering from a large ulceration, the sequel to an abscess, lying deep in the neck. He had had the advantage of good professional attendance and general treatment; but the ulceration not only did not heal, but was slowly extending. We determined to see the effect of rest to the ulcer. Strips of plaster were placed across the neck, and a layer of cotton wool over the plaster and the ulcer, upon the side of the neck; these means, aided by gentle bandaging, kept the platysma myoides quiet. The ulceration had continued a considerable period before I saw the patient, going on from bad to worse; but immediately on this plan being carried out, the surface of the ulceration began to improve in appearance and to diminish in size, and in a very short time was perfectly well; although the child was not, upon the whole, a very healthy subject.

*Deep Cervical Abscess, followed by a Sinus.*—I have here the notes of another case, where a sinus existed in the neck, and was cured by rest. This case was of two years' duration, and was cured in three weeks by rest. I am almost afraid you may be induced to fancy I am using the language of exaggeration in this statement; but really it is not so.

In 1849, Elizabeth H——, aged twenty, living at Lambeth, had had a large abscess extending deeply under the platysma myoides and the sterno-cleido mastoideus. This had continued as a long sinus, discharging freely during many months. She had been an hospital out-patient during two years. Iodine, &c., had been injected into the sinus, and a seton passed through it, and retained within it for some time without any benefit. Strips of adhesive plaster were drawn tightly over and across the muscles covering the sinus, and the head was steadied by a pasteboard splint, cut rudely into a form which could be adapted to the pelvis, along the back of the shoulders to the back of the head, and then laterally on each side, so as to embrace the whole of the head in a circle of pasteboard.* A figure-of-8 bandage was then applied over this apparatus around the head and under the axilla, crossing in front of the chest, so as to fix the head and neck forward and a little downwards firmly upon the shoulders, and to relax the muscles. The sinus ceased to discharge at the end of a fortnight; but she continued to wear the apparatus for a short time—about three or four weeks—longer, as a further security.

This patient was thus cured by rest.

*Small Superficial Cervical Abscess.*—We are often troubled to deal satisfactorily with the results of small superficial abscesses in the neck. Such open abscesses under the skin are mostly said to be scrofulous; but I suspect their persistence depends not unfrequently upon their close relation to the platysma myoides. I believe—and I am speaking here from experience—that by placing a pad of cotton wool, twice as large as the area of the disease, over the part, and by fixing it there so as to keep the platysma myoides and all the surrounding parts at rest, many of these cases that seem to defy almost every kind of treatment may be successfully dealt with. I have had opportunities of observing the success of this treatment in very many such cases.

*Cases of Carbuncle, followed by Sloughing.*—We all know that it is not easy to manage successfully the treatment of a patient who has had a large carbuncle on the back of the neck near the scalp, which by destroying the subcutaneous areolar and fascial structures, leaves large portions of loose overlapping skin, blue, dark-coloured, and congested, showing a very feeble power, and, added to this condition, the trapezii muscles exposed to view.

* This splint was made off-hand with wet, soft, thick pasteboard, covered by linen, pressed upon the surface of the body, so as to become moulded to it, and then allowed to dry firm in its position by the bandaging.

Now I wish to show the therapeutical value of local rest in the treatment of such a case.

Two years ago I saw the wife of a physician whose condition accurately resembled that which I have just delineated. She had been previously attended by a very eminent London surgeon. The case was not proceeding satisfactorily; there was no local evidence of repair; the wound had remained stationary during some time before my visit. On looking at the patient's neck, it appeared to me that there were two additional requisites in the treatment which might help the cure: one was to arrange some simple mechanism which would keep the trapezii muscles quiet; and the other, to support in their proper positions, and to maintain in a state of perfect rest, the loose, feeble flaps of skin. I hoped by such means to facilitate adhesion of the two granulating surfaces. The loose flaps of skin were laid neatly upon the subjacent trapezii, and then a large, thick pad of cotton wool was firmly fixed upon the surface of the flaps of skin and surrounding parts. A bandage was applied around the head, and extended as a figure-of-8 bandage, crossing behind the neck and under the armpits, in order to fix the head, neck, and shoulders, and control the trapezii. In twenty-four hours the healing commenced, and proceeded under the same local treatment, to the most satisfactory and speedy termination.

Not long after that time, I saw a patient with Mr. Wright in the Clapham-road, where precisely the same circumstances occurred, with precisely the same treatment, and precisely the same results. Here the parts were kept at rest partly by a thick pad of cotton wool pressing upon the flaps, and by means of a bandage to keep the head and the trapezii muscles in a state of rest. Cases of the same kind, with the same result from the same treatment, have occurred to me lately in Guy's Hospital.

*Popliteal Abscess and Sinuses.*—Sinuses in the popliteal region, in unhealthy subjects, are very difficult to cure, except by local rest; and here I will take the liberty of reading part of a note which I received from a surgeon in Essex, who had sinuses at the back of his knee-joint:—

"Jan. 9th, 1853.—My sinuses occurred after an abscess in the popliteal space, which left a very irritable, unhealthy ulcer, the size of a walnut; and several long and deep sinuses extending from it, amongst the tendons of the hamstring muscles. Not being able myself to improve this ulcer, I consulted the late Sir A. Cooper, who stated that it was owing to a defective state of my general health, and ordered me steel and quinine, to inject nitric acid lotion, to take exercise, and to wear a high-heeled shoe. At the end of many weeks, the ulcer and sinuses remained as they were. You then saw me, and ordered my leg to be flexed, and placed upon a resting splint upon a wooden leg, and moderate pressure upon the sinuses with soap plaster. I continued this plan, and in two months I was quite well, and have been so ever since, and am in active occupation in my practice."

*Small Facial Abscess.*—In some small abscesses sometimes occurring in the face, cheek, or neck, which have opened by a minute ulceration through the skin, the aperture communicating with the sac of the abscess, I have found the best result from carrying out this principle of rest to the part. I have merely passed a small probe into the aperture in the skin for the purpose of keeping it open whilst collodion was being applied upon the dome of the abscess, so as to press the dome upon the floor of the abscess. In this way pressure and rest are secured, and by coaptation the parts are kept undisturbed; the process of healing then commences, and I think the patient gets well more quickly by this than by any other local method of treatment.

Sinuses, then, are cured by the adoption of local rest; and I think that if surgeons, instead of affixing to every sinus the opprobium of bad general health, would take into consideration the possibility of there being some special local disturbing cause associated with it, such as muscular or other movements, more success would result from a very simple plan of treatment by rest.

[The curative agency of rest in painful ulcer, is well known; it is an exemplification of what Mr. Hilton calls "physiological rest."]

*Painful and Irritable Ulcers.*—An irritable ulcer is to be distinguished from an inflamed ulcer, no doubt by the quantity of lymph which is poured out upon the inflamed ulcer, and the high degree of its temperature. Irritable ulcers, as we know, are exceedingly painful, and sometimes very difficult to cure. As far as I have been able to detect their real essential pathological character, it depends upon the exposure of a nerve on the surface of an ulcer. Of course it is quite clear that every ulcer must have nerves more or less exposed on the surface; but in the case of an irritable ulcer, it seems probable that the sheath of the nerve is destroyed, and that the end of the true nerve-fibre or tubule remains denuded, and so causes the exquisitely sensitive and painful character of the ulcer. Upon this exposure of nerve depends, I believe, the chief feature of what we term an irritable ulcer, and the method of detecting the precise seat of the exposed nerve is very simple. You must understand that my remarks on this subject are founded on an experience in such cases of twelve or fourteen years. Having a very painful uninflamed ulcer before you, you apply the blunt end of a common probe upon the sore, and as you move it about, presently the patient exclaims, "Oh! there you hurt me dreadfully." Then you go a little further on with the same method of examination, and scarcely any pain is felt; but on coming back to the same spot, the patient is again "dreadfully hurt." At that painful spot some small nerve is exposed on the surface of the ulcer. The first time I observed this fact, and placed upon it what I believe to be the right interpretation of it, was in a case of a patient who had a thecal bursa extending from the palm of the hand under the annular ligaments to above the wrist. I made an aperture into the bursa above the wrist large enough to let out all the fluid and solid contents, and the bursal swelling gave me no special trouble, and it was speedily cured; but at the seat of the opening a very irritable painful ulcer remained which I could not cure by any local application. On examining the ulcer with a probe I discovered a point of exquisite tenderness. The normal position of the small palmar branch of the median nerve corresponded with the site of the local sensitiveness and seemed to explain the pain. I then passed a small-pointed bistoury under the track of the nerve above the tender spot, so as to divide the nerve between the spinal marrow and the irritable point of the ulcer; from that time the pain ceased, and the ulcer rapidly healed. It was cured by the "physiological rest" resulting from the division of that little nerve.

*Painful Granulations following Injury.*—Some time after the occurrence of this case, I had a patient in Guy's Hospital suffering greatly from an ulcer at the end of the finger, in which it was supposed there was a piece of broken glass, as the original injury was a cut from breaking a window. She had been under the observation of a surgeon, who had tried repeatedly to get out the supposed piece of glass, and had punished her severely, but unintentionally. She came into the hospital, and I thought the case would give me a good opportunity of making a demonstration of what I had long deemed to be correct, and taught. On placing the broad end of my own finger upon the ulcer, it gave her exquisite pain; the broad surface of my finger, however, was not a sufficiently accurate localizer of the pain. I then employed the rounded end of a probe, and with great care examined the whole surface of the ulcer by pressure, until I came upon a spot that was exquisitely tender, and produced dreadful pain to the patient. With a pair of scissors I cut out the painful granulations. Explaining to Dr. Habershon, the Demonstrator of Anatomy at Guy's Hospital, the views I entertained regarding the cause of the painful granulations, I requested him to examine them by the aid of the microscope, and he found in them, and near the surface, as I had expected, looped filaments of nerves, thus completing the demonstration of the cause of the pain. From the time of my cutting away those sensitive granulations the pain ceased, and the sore began to heal; there was no more trouble or difficulty as regards the treatment of the ulcer; it got well by giving it *"physiological rest."*

*Exquisitely painful Ulcer after Injury.*—About a year and a half ago, I was requested to see a gentleman's coachman, who, on getting off his box seat, slipped his fingers between the lower bar and his seat, and thus had two of his fingers broken off at the second phalanges. One of them went on rapidly towards healing and healed very well. The other remained swollen, irritable, very painful to the touch or on exposure to the air, preventing sleep, and producing great constitutional disturbance. We failed to relieve these symptoms by the local and internal employment of opium. This unhealthy condition could not be from any constitutional defect, because one finger did well; nor could it be from the result of any dissimilarity of the original injury, for they were precisely alike. With the surgeon in attendance, I made a careful examination of the part, and when I placed the end of a probe towards the edge of the ulcer upon the finger, it detected a spot which was exquisitely tender, and the patient screamed out—"Oh, pray, for God's sake, cover it over, I can't stand it." The position of this pain was in the course of one of the lacerated digital nerves. I passed a pointed bistoury under the nerve, about one-fourth of an inch above its exposed portion upon the wound, and so divided the nerve. The pain in the ulcer immediately ceased, and the touch of the probe caused no uneasiness. From that time all the local symptoms rapidly improved, and the case gave no further trouble, being quickly cured by "physiological rest."

These cases prove distinctly that an ulcer may be very much modified in its character from the exposure of a nerve in the wound.

I mentioned this subject to my colleague, Mr. Cock, some time ago, and shortly afterwards he had an opportunity of testing the value of the observation. He recognised the condition of such an ulcer as that I have referred to, and divided the exposed nerve; the patient lost the pain, and the ulcer quickly assumed a healthy character and got well.

*Painful irritable Ulcer of the Leg.*—I have here short notes of two other cases that have occurred lately, under my care, at Guy's Hospital, to which I will, with your permission, now allude.

John J——, aged 27, sailor; admitted on the 9th of November, 1859, suffering from syphilitic sore on frænum and penis, with secondary eruption, and a very painful ulcer on the inner malleolus of his left leg. The syphilis was treated and cured by Plummer's pill, five grains, twice a day; but the painful ulcer remained uninfluenced by the mercury.

This is the history of the ulcer provided by my dresser:—

"When leaping, about four years ago, he sprained his ankle, and an ulcer formed on the inner malleolus of the left leg. It had made frequent efforts at healing, but never cicatrized completely. It now looked irritable, with no inclination to heal, and was very painful, with intense nervous sensibility localized at its upper margin, which was ascertained by examining it with a probe.

"Jan. 24th, 1860.—Mr. Hilton passed a pointed bistoury a little distance above the tender spot, under, and then through, the granulations, thereby severing the filaments of the nerves supplying the morbidly sensitive granulations; and although the patient made much ado about the operation, yet he immediately acknowledged himself relieved by it. The ulcer readily assumed a healing aspect, sensation over the other parts of the ulcer was not more acute than normal, the surface became covered by healthy purulent exudation, cicatrization daily advanced, and the ulcer was closed in a fortnight, and remained so until he left the hospital, on the 8th of March, 1860."

This was a case of irritable ulcer, cured by division of the nerve.

The other case may be put before you in a few words:—

Jan. 11th, 1860.—The painful spot of an old irritable ulcer was examined by a probe; the nerve supplying the tender granulations was divided; marked relief was the immediate consequence. This division of the nerve was done on the 11th January, and on the 16th this is the dresser's report:—

"The ulcer above mentioned is free from pain, and has assumed a healthy character; its edges are throwing new skin over the granulations. From

this time, the painful ulcer required no special attention, and in ten days all was healed."

These are cases that appear to me to display very accurately the therapeutic value of what we may fairly term "physiological rest," to the abnormal sensibility of the surface of the sore. The division of the nerve had its effect upon the neighbourhood *physiologically*, and the ulcer began to heal.

I trust I may have thus briefly succeeded in pointing out the true pathological feature of what is termed a "painful irritable ulcer."—*Lancet, Nov.* 17, 1860, *p.* 477.

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## 74.—ON THE SURGICAL TREATMENT OF SINUSES.

By WALTER JESSOP, Esq., Surgeon to the General Hospital and Dispensary, Cheltenham.

[The patient had on three occasions, suffered from bubo in the right groin, and the whole groin was radiated in every direction with sinuses, some running to a considerable depth.]

On seeing him in December, 1858, eight distinct sinuses could be traced. For nearly six months he had been under treatment; injections, setons, rest and pressure had entirely failed to heal a single sinus. The integument was much indurated and discoloured, and the patient's health greatly impaired. Directing a generous diet, I determined upon a renewal of rest and a careful adaptation of pressure; confining my patient to bed, the better to insure the application of well-regulated bandages. For nearly a month this treatment was scrupulously carried out, but with no good effect. Setons of silk were then tried, saturated in solutions of chloride of zinc, iodine, nitrate of silver, and such like stimulants. They simply increased suppuration. On removing them pressure was again resorted to, but equally without effect, although perseveringly applied for some little time. In one length of the principal sinus, through which it was impossible from its irregular nature to pass the full length of seton, it was necessary to cut down upon the probe and draw out the silk, leaving a further portion of the sinus untouched. To my surprise, this was the only part that had healed, a portion rather more than an inch in length. Arguing from this circumstance that if the continuity of each sinus were destroyed at certain intervals of its course, adhesive inflammation might be more readily induced, I proposed to my patient that he should submit to a trial of the experiment, and to this he willingly consented.

Passing a probe into each sinus a distance of an inch, or an inch and a half, I cut or nicked down on its point with a small lancet-shaped scalpel, so destroying the continuity of the canal at that spot. Withdrawing the probe, I repassed it from the point of incision for another inch, repeating the proceeding, and so on along the track of each sinus. In this way numerous small incisions were made. Pressure was carefully re-applied over the affected parts. Very little disturbance ensued, but the patient complained of more life and soreness in the groin than he had done at any other period of his case.

Pressure was retained for five or six days, and upon examination it was evident that adhesive inflammation had been set up, and that there was a fair probability of a permanent cure. In less than a fortnight the major portion of the sinuses had quite healed. Those which had failed were again treated in a similar manner, and the second operation proved entirely successful.

I saw this patient at intervals for nearly six months after convalescence, and on several occasions carefully examined him. There was no symptom of a sinus remaining, and the thickened and discoloured integument was much improved in appearance and character; the patient's health, also, in every way was re-established.

T. M——, a mate in one of the Australian line of clipper ships, consulted me in February, 1860. Two long irregular sinuses had existed in the left groin for upwards of seven months; and a pustular eruption spread over the scrotum and thigh, evidently occasioned by the irritative nature of the secretion from the sinuses. His case was treated in a similar manner to the

preceding one. The single operation was sufficient, and, under pressure, the whole had healed in twelve days. This patient I have not seen since.

The proceeding referred to in these cases is simple, effectual, and really productive of very little suffering. The sinuses were of so extensive a character, and involved so wide a surface of unhealthy integument, as to preclude all appeal to the vigorous use of the bistoury, or the application of the heated platinum wire—an admirable plan introduced by Mr. Marshall, of University College Hospital, and published in the "Medico-Chirurgical Transactions" for 1851. Pressure and rest, and careful attention to the general health, had been tried, over and over again, without avail; and not the slightest progress was made until the continuity of each fistulous track was destroyed at intervals, and in the manner abovementioned.—*Lancet, March* 9, 1861, *p.* 235.

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75.—*In-growing Toe-nail.*—The treatment is by dropping a few drops of melted tallow, made very hot, upon the diseased part. In the Medical and Surgical Reporter for October 13th, Dr. H. M. Clarkson reports a bad case of in-growing nail at once relieved, and soon cured by one application of this simple remedy.—*Amer. Med. Monthly, Dec.* 1860, *p.* 439.

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## 76.—THE SUBCUTANEOUS APPLICATION OF THE METALLIC LIGATURE TO THE CURE OF VARICOSE VEINS OF THE LEG.

By Dr. R. J. Levis.

Dr. Levis thus describes his operation:—To perform the operation it is important that the veins be distended, so as well to determine their outlines and avoid any risk of wounding them. This may be accomplished by putting a band around the limb above the knee, while the patient is in the erect position; or this may be rendered unnecessary by performing the operation whilst the patient continues standing. If the latter position be chosen, the patient, in order to have the leg at a convenient height, stands on a chair or table which is placed by a wall, against which he steadies himself. The points chosen for ligature are, first, the trunk of the saphena, at the highest point where there is evidence that, owing to the abnormal dilatation, its valvular structure is imperfect; then the largest and most superficial veins, at places where they are most isolated; and, finally, those in the neighbourhood of ulcers or eczematous eruptions of the integument. The only instruments and appliances essential are a long straight needle, some silver or iron wire, adhesive or isinglass plaster, and a roller bandage. The needle should be straight, and two and a half or three inches in length, and differing from the common surgical needle in having a sharp, round point, *which perforates without cutting*. In the absence of a needle properly adapted for carrying the wire ligature, an ordinary fine darning needle will be quite suitable. Experience has proved to me that a straight needle is much more manageable for directing the point accurately than the usual curved one, and the policy of avoiding the cutting or spear point, where, as in the varicose state, even the innumerable venous capillaries are often in a varicose condition, and which, when wounded, pour out blood profusely into the cellular tissue, is obvious. The wire had better be of a fine gauge, as number 30, or finer. Pieces of adhesive, or isinglass plaster, the latter preferred, one or two inches square, are useful. The bandage may be six or eight yards in length, and two and a half inches wide. A wire-twister is a convenience but not an essential to the operation. The patient being in a proper position, the operation is commenced by feeling for the edge of the vein to be ligatured, and entering the needle perpendicularly until a point beneath the under surface of the vein is reached. Then the shaft of the needle is depressed and its point

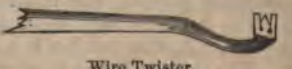
pushed horizontally beneath the vein until it makes its exit through the integument on the opposite side of the vein. The exit is facilitated by pressing on the integument with the fingers of the left hand over the point of the needle. After the needle is withdrawn, leaving a wire beneath the vein, it is *re-entered at the same orifice*, but this time passes *above the vein*, traversing the space between the integument and the vein, and makes its exit at *the point of original entrance*. A slight pulling on the wire draws

Course of the needle.

beneath the skin the loop of wire left on the opposite side of the vein, and all that is seen of the wire is its two ends projecting from the same orifice. Thus the vein is surrounded by a single wire. Proper care will avoid a risk of wounding the vein, but if there should be evidence that this has occurred, the needle ought to be at once withdrawn and another point for the operation selected. The accompanying outlines, to make the matter more explicit, illustrate the course of the needle and wire, and the manner in which the wire encircles the vein.

Wire surrounding the vein before being tightened.

The wires are then pulled sufficiently tight to simply constrict the vein, approximate its sides, and stop the circulation through its calibre. The object, be it understood, is not to induce a rapid ulceration through the coats of the vein. In this, my own practice differs from the directions generally given for the ligature of varicose veins, and on this peculiarity, perhaps, depends its apparently absolute safety. It is true that to insure a perfect closure of a varix it is necessary that it should be actually divided, but this division is best accomplished slowly, and only after the slight pressure on its walls has excited within a plastic exudation which agglutinates them. Exposure of an opened vein to a pyogenic surface is in this way with certainty avoided. The pressure made on the vein by pulling the ends of the wire is secured by twisting them. This may be done by means of the fingers, forceps, or the wire-twister. A simple form of the latter instrument, which I prefer, being easily placed on the wires without threading through holes, as in the form of instrument generally used, is herewith represented.



Wire Twister.

The wire is finally cut off, leaving an inch or more, which is laid flat upon *the skin*; the place of operation is covered with a small piece of adhesive *or isinglass* plaster, and a roller bandage envelopes the limb up to the knee.

If an ulcer exists on the limb, it is to be simply covered, previously to the application of the bandage, with several thicknesses of dry lint. The patient rests horizontally, without disturbance of the dressing, for ten or twelve days. After this time all dressing is removed, and traction is daily made on the wires to accelerate their removal. But little more confinement of the patient is usually requisite. Decided relief from swelling of the varices is experienced, and he is frequently able to walk about with comfort long before the wires are removed. In one of my cases, a labourer had two wires remain in his leg for eight weeks, during the latter part of which time he continued his occupation with but little inconvenience; in another case the ligatures, six in number, were all spontaneously removed on the fourteenth day. On removal of the bandage on the leg, the ulcer, if dependent on the varicose condition, usually seems to be really *dried up*, and cicatrization rapidly follows.—*American Med. Times, Dec.* 15, 1860, *p.* 428.

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## DISEASES OF THE EYE AND EAR.

### 77.—ON IRIDECTOMY.

[The surgeons of the University of Bonn consider that the operation of Iridectomy is equally beneficial not only in acute and chronic glaucoma, but in acute choroiditis, in ulceration of the cornea connected with iritis, and in several other rapidly progressing and destructive affections.]

Professor Weber has, in most of his cases, only resorted to iridectomy after the antiphlogistic remedies, and especially the administration of calomel, even to salivation, had failed; and the success has exceeded his expectations. The process of operation, although simple, nevertheless requires great care. The incision should be an ample one, directed superiorly and internally, beyond the margin of the cornea, and nearly to the level of the iris, so that the ligamentum pectinatum iridis is severed. If there are no adhesions of the iris, it generally prolapses immediately, and there is no difficulty in seizing it. The segment of the iris should be rather large, and Mr. Hancock is quite in error when he says in his paper on division of the ciliary muscle in glaucoma, that all oculists agree that the smaller the quantity of iris removed, the better. On the contrary, small and incomplete excisions of the iris are rarely attended by striking success; and it is, perhaps, to this circumstance that the failures of some English oculists, who have only cut out infinitesimal portions of the iris, are to be ascribed. The segment of the iris should be completely removed up to the ciliary margin of the iris. Cutting of the cornea must be carefully avoided, as, if this be done, it is not possible to remove the iris up to the ciliary margin. Dr. A. von Graefe considers that the removal of the tension of the bulbus is the cause of the curative effect; and it is certainly true that, in the morbid conditions above mentioned, the intra-ocular pressure is so considerable, that the aqueous humour often spirts out with great force when the operation is performed. On the other hand, Mr. Schweigger, one of Von Graefe's pupils, holds that severing of the anterior tendinous insertions of the tensor choroideæ is most instrumental in effecting the cure, while the surgeons of Bonn are of opinion that the effect is to be compared to that of an ample incision into an inflamed tissue, as for instance in panaritium, abscess, &c., when the inflammation generally subsides immediately afterwards. Iridectomy is also advisable as a preparatory operation for linear excision of cataract, as sometimes after the latter has been performed, a considerable tumescence of the fragments of the lens, and pressure on, and irritation of, the iris take place. The operation, which causes only a very slight wound, is scarcely ever followed by bad symptoms, so that the patients are generally able to leave their beds a few days afterwards. The enlargement of the pupil by the coloboma iridis and the disfigurement of the eye, upon which

the opponents to this operation have laid so much stress, are scarcely perceptible if the incision is made superiorly and internally.

With regard to the operations for cataract, I may mention that German oculists have now almost entirely abandoned reclination, as they found that even in successful cases, where the patients could see very well for some time after the operation had been performed, after a few years, blindness recurred; and in such cases the examination by means of the ophthalmoscope always showed that, where the lens had been placed, a choroiditis had set in, which again destroyed the sight. The results which are obtained by the ordinary extraction by a flap formed upwards for the hard cataract of old people, and by the linear incision for the soft cataract of young persons, are so satisfactory that other modes of operation are scarcely necessary. In thirty-five cases of extraction by flap, Professor Weber has only had two unfavourable results; and even these patients completely recovered the sight of the other eye. As to linear incision, the same surgeon strongly recommends the operation to be finished as rapidly as possible, and to avoid bruising the edges of the wound by too diligently endeavouring to remove every small fragment of the cataract, as by such proceeding iritis may be induced.—*Med. Times and Gazette, Feb.* 9, 1861, *p.* 155.

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## 78.—ON A NEW OPERATION FOR IRIDECTOMY.

By Henry Greenway, Esq., Plymouth.

[The operation proposed by Mr. Greenway is entirely novel. It is for producing an artificial pupil in the natural position and of a circular form. The author states that the operation is not suitable for all cases.]

Fig. 1.

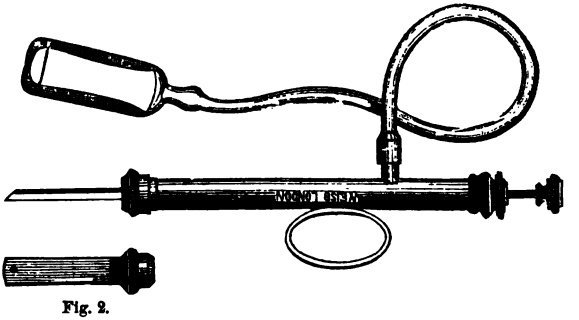

Fig. 2.

Fig. 1.—The instrument complete, showing a side view of the canula.
Fig. 2.—A view of the plane surface of the canula, showing the circular aperture for the admission of iris.

The instrument somewhat resembles a syringe, but, instead of being furnished with an ordinary nozzle, a canula is screwed on to the end of the cylinder, or body, and, there being no piston, the rod, which would otherwise be the piston-rod, is continuous with a blade which lies within the canula. This rod passes through an air-tight fitting at the upper part of the cylinder, and has a spring action. The canula is plano-convex transversely, its bore being one-twelfth of an inch by one-fifth; the plane surface presents near the extremity a circular aperture, about one-eighth of an inch in diameter, for the admission of a portion of the iris; that small portion of the tube beyond this aperture is filled with lead, which serves, not only as a plug to completely obstruct the extremity, but as a point of resistance for the blade. On the convex surface is a small mark which indicates the position of the aperture on the plane surface.

The blade is kept in close contact with the floor or plane surface of the *canula* by means of a spring, which is interposed between it and the roof or

concavity of the canula. Communicating with the upper part of the cylinder is a metal tube, about an inch in length, on which is fixed an India-rubber tube, which is furnished with a mouth-piece at its free end. This may be termed the suction-tube. On the opposite side of the cylinder is fixed a ring to receive the finger of the operator.

It will be evident that if a person placed the mouth-piece between his lips, and caused suction, a current of air would pass in at the circular aperture of the canula, and, that if this aperture were placed in opposition with a thin structure, such as the iris, a small portion of it would be drawn into the canula in the form of a cup. If, now, the blade be pushed forwards by pressing the head of the rod, the enclosed structure, answering to the shape of the aperture, would be excised, immediately the edge of the blade came in contact with the piece of lead which fills the extremity of the canula beyond the verge of the aperture.

The experiments I have made on the eyes of animals, supplied from the slaughter-house, have been most successful. I found that I could, without fail, excise a perfect disc from the iris in its natural state, thereby leaving a corresponding aperture. In some cases, I produced an aperture so near the pupillary margin as to leave only one-twentieth of an inch of iris, and that in an uninjured state, between the natural pupil and the artificial one. I may, therefore, fairly presume that in cases of closed pupil, for which the instrument is intended, the results will be equally satisfactory.

The mode of operating is to pass a lancet-shaped blade through the temporal edge of the cornea, very near its junction with the sclerotica, into the anterior chamber, the incision being made large enough to admit the canula, —the division of one-fifth of the circumference of the cornea would certainly suffice. The operator having placed the mouth-piece of the suction-tube between his lips, should then introduce the canula into the anterior chamber, its plane surface, of course, facing backwards, and, as soon as the aperture of the canula is over the portion of iris required to be excised, suction should be made, and, if the operator is satisfied that the iris is sufficiently drawn in, presure should be made on the head of the rod, to cause the edge of the blade to bear on the lead plug to ensure complete excision of the iris. Before the canula is withdrawn from the eye, the blade should be allowed to recede, by ceasing to press on the rod.

The instrument should be held by placing it between the fore and middle fingers of one hand, the thumb resting on the head of the rod; or, between the thumb and middle finger, the fore-finger being then placed on the head of the rod. The other hand should rest on the patient's cheek, the canula being steadied between the thumb and fore-finger.

The position of the patient during the operation should be upright, the head slightly inclined forwards, so that the iris may have a tendency to gravitate against the instrument. Proper means, of course, would be used to steady the head.

After every operation, the canula should be removed from the cylinder, and thoroughly cleaned and dried; the knife, also, should be cleaned and slightly oiled before it is placed in the canula.

It would be advisable, before operating on the living subject, to procure a bullock's eye, in order to judge of the amount of suction required.

The chief novelty in this Guillotine is, the employment of a vacuum to perform the office of a forceps, and, in combination with the aperture, to determine the shape of the excision. I believe, in all cases where exhaustion of air has been employed in surgery, it has been simply to do the duty of a pump in raising fluid to the surface. Although this instrument was devised for producing artificial pupil, I intend applying it to other purposes, by merely altering the size and shape of the canula and blade according to the requirements of the case. The operation for the removal of the tonsils is one in which this instrument may be used with advantage, as no injury to the surrounding parts could possibly occur. Having ascertained that the aperture of the canula is closed with the surface of the enlarged tonsil, suction should be made until a sufficient amount of its substance was drawn in. Ex-

cision can immediately be produced, even if the patient's mouth were nearly closed. If the operator choose, he may, instead of using the mouth-piece, remove it from the suction-tube, and in its place apply an exhausting syringe, such as is used on a cupping-glass. This may be worked by any bystander under the direction of the surgeon.

In all operations the suction should be maintained during the action of the blade.

In this paper I have not referred to any of the complications which occur in connexion with closure of the pupil; but I do not consider either form of synechia would be any impediment to the use of the instrument.

Messrs. Weiss are the makers of the Guillotine, their name being a sufficient guarantee for its quality and finish.—*Med. Times and Gazette*, *Dec.* 15, 1860, *p.* 578.

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79.—*On Iridectomy in Glaucoma.* By Dr. QUAGLINO.—The following are Dr. Quaglino's conclusions with respect to this operation, the results of abundant clinical observation: 1. Iridectomy may restore or improve vision even in cases in which the glaucoma is chronic, and more or less complete amaurosis has existed for a longer or shorter time, and signs of congestion of the external membranes are absent. 2. It is indicated in chronic glaucoma, when ophthalmoscopic inspection shows that the papilla and its vessels are not in an advanced state of atrophy. The failures of the operation in old glaucoma have depended upon this advanced condition of atrophy of the anterior extremity of the optic nerve and its central vessels. 3. In the majority of cases the neuralgia which accompanies the glaucomatous affection disappears after iridectomy. 4. The phenomena of excavation of the papilla, its greenish colouration, and pulsatile action of the vessels, indicated by authors as characteristic of glaucoma, may be wanting, while those derived from the exiguity of the arteries and veins of the retina, especially in the papillary area, and the shortness of their course, are more constant. 5. This change in the papillary vessels, which in old glaucoma terminates in atrophy, must be regarded as the effect of the pressure of the humours which have become increased in quantity, or as the consequence of the strangulation produced around the optic nerve by the engorged or hypertrophied choroid. 6. Iridectomy, therefore, cures glaucoma by evacuating the humours, taking off compression by diminishing the mass of tissues within the eyeball, and disgorging the choroidal vessels. A proof that this is the case is found in the fact that after the operation the nutrition of the vessels becomes augmented, and the hardness characteristic of the glaucomatous eye ceases. 7. The success of iridectomy, independently of other circumstances, is directly proportionate to the amount of inflammatory action which succeeds to the operation, and to the size of the portion of the iris removed. 8. After the operation, the anterior chamber being for the most part abolished, and the convexity of the ball diminished, sensible advantage is usually derived from the employment of presbyopic glasses of an intermediate number.—*Omodei's Annali.*—*Med. Times and Gazette*, *Feb.* 2, 1861, *p.* 129.

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## 80.—ON THE TREATMENT OF ACUTE GLAUCOMA BY IRIDECTOMY.

[The following case occurred at the Moorfields Ophthalmic Hospital, and was under the care of Mr. Dixon.]

"Mrs. P., aged 46, was admitted December 27th. She was a thin, moderately florid woman, not subject to gout or rheumatism. Twelve days before admission, she had a severe pain in the right eye, which came on suddenly at night. Next morning, she found that she could scarcely see with it; and the pain, still severe, had continued up to the time when Mr. Dixon saw her. Blisters and various applications had been used, but without any benefit. When admitted, she could with the affected eye only just distinguish

light from darkness, and could not count fingers held up between the eye and the light. The sclerotic was dusky, leaden-coloured, and crossed by enlarged veins. The pupil was dilated moderately, and fixed. The fundus, as seen through the pupil, looked dull and yellow. The cornea had lost its brilliancy. Atropine did not produce any effect upon the size of the pupil; whilst in the other eye the pupil readily dilated under its use. The ophthalmoscope was used; but a mere red reflex from the fundus was seen. The woman would not submit to the operation of iridectomy, which was at once proposed, until the 31st, the fourteenth day from the attack, when iridectomy was performed. The piece of iris removed was from the upper part. No chloroform was given. The pain was at once relieved.

"Four days after the operation, she had had no return of pain, and could read No. 10 of Jäger's test-types. There was a minute fistula in the cornea, to which Mr. Dixon applied nitrate of silver. She gradually improved, and was soon able to read small type.

"On Jan. 25th, the left eye became suddenly very painful. The pain was in the globe; it was very intense, and was accompanied by vomiting. She was not seen by Mr. Dixon until the 28th. He then found that the eye was in exactly the same state as the right had been. She could only just count fingers. The pupil was dilated and fixed; and the fundus could not be illuminated by the ophthalmoscope. Iridectomy was at once performed, and with the same amount of success as attended the operation on the other eye. She can now see well with both eyes, and can read pearl type without the aid of glasses.

"Mr. Dixon remarked that, in this case, the disease had been of the exact form to the relief of which iridectomy is best suited; viz., *acute* glaucoma. In the chronic cases, as far as he had seen, little or no benefit followed the operation, but rather an increase of the irritation. It is interesting to note that the attack in the left eye occurred whilst the right eye was going on satisfactorily. The ophthalmoscope had not been used to examine the sound eye, and therefore no blame was traceable to it, as having re-excited the disease in it. The occurrence of glaucoma in the second eye, a few weeks after it has attacked the first is quite in accordance with what is frequently observed; and hence, as both eyes generally suffer, the value of iridectomy is much increased."

[We append the remarks of the Editor of the British Medical Journal on glaucoma and iridectomy, published in the same number of that Journal, and particularly referring to the above case.]

The operation of iridectomy in acute glaucoma has now been before the profession for about six years. It has been largely practiced; and, by those who have avowedly abstained from trying it, energetic attempts have been made to detract from its merits. The great facts, however, remain, that acute glaucoma does not unfrequently occur; that it is irremediable by constitutional treatment; and that the excision of a portion of iris, if performed within a few days of the outbreak, is effectual in restoring vision and putting an end to the disease. To the first two of these propositions all experience bears testimony. Respecting the third, when surgeons of the authority which Mr. Bowman, Mr. Dixon, Mr. Critchett, and Mr. Hulke, must be allowed to possess in such a matter, unite in one strong and unequivocal statement, it is no longer possible to be incredulous. Indeed, the facts are before us, and we may now judge for ourselves. Cases attested by the names of the surgeons we have mentioned are now on record, which, if clinical evidence can be allowed the slightest weight, prove that this operation, whatever may be the true theory of its action, possesses a really marvellous power in controlling the progress of certain morbid changes otherwise destined to result in permanent blindness. If we may trust these facts, Von Gräfe's discovery ranks second only in ophthalmic beneficence to that of the operation for cataract.

Let us ask especial attention to a case recently under Mr. Dixon's care at the Moorfields Hospital (see above). Mr. Dixon's reputation as a sound and most cautious surgeon is well known. He was not one of those who early

adopted this new operation; he even published certain warnings against its too indiscriminate performance. Being, therefore, no partisan, his facts possess especial value. In the case we refer to, the disease was well characterised, and had existed for nearly a fortnight. The woman was so far blind with the affected eye, that she could only see a hand moving before it, and could not count fingers. The surface of her cornea was so uneven that no images were reflected with clearness: and such was the effect of the intra-ocular pressure, that it was impossible to light up the fundus with the ophthalmoscope. Thus the sign of cupping of the optic entrance could not be used, the case being too advanced in its stage. In this condition of things, Mr. Dixon excised a portion of iris. All pain in the globe ceased after the operation. On the next day, the woman could easily see ordinary objects. On the third, the brilliancy of her cornea was restored, and she could read large print. At a week's end, she could read common type with an eye with which previously she could not discern fingers. Subsequently the same disease attacked the other eye. Iridectomy was performed in it also, and with an equally good result.

The case, wonderful as it is, coincides so exactly with fifty others published by different observers, that there is no room for fallacy. It is no longer a matter in which the conscientious surgeon can decline to act. If he operates, his patients will regain their sight; if not, they will go blind. To dissuade a patient with well characterised glaucoma, *in an acute stage*, from having a portion of iris excised, will, if we mistake not, soon become as legitimate a ground for an action for malpraxis as to leave a dislocated hip unreduced.

It must be remembered—and it is for this reason that we wish especially to press the subject upon the attention of all our readers—that acute glaucoma is a condition which will not wait. If the proper time is not seized, it passes by for ever. In this respect, it is more urgent than any form of dislocation, and in fact closely resembles strangulated hernia. There are a few operations which, being usually required at an emergency, it is considered that every surgeon ought to be able to perform at a moment's notice. Amongst these, tracheotomy and the operation for hernia have hitherto been the chief. Operations on the eye have been regarded as a speciality, and as requiring special practice for their successful performance. In iridectomy we have, however, an exception to this. Von Gräfe, in his third memoir on this subject, has very properly insisted that the surgeon in a remote country district ought to feel himself as much bound to be able to perform iridectomy when requisite, as he would to practise tracheotomy off-hand if a patient were in danger of laryngeal occlusion. The operation is in itself not a difficult one, and the delay of a few days may be of the utmost consequence.

There are some interesting facts in the history of the progress of this new operation into general adoption. It suffered temporarily, as many other important novelties have done, through the honesty of its advocates. Having discovered its extreme value in acute cases, Mr. Bowman and Mr. Critchett, who were the first to employ it in this country, felt bound also to give to numerous other cases, otherwise hopeless, the chance which it might possibly afford. It seemed cruel to refuse to do it because only a half-success, or perhaps still less, was all that the most sanguine could hope for. Hence its performance in many cases which from the first were almost hopeless. All these were scrupulously recorded by Dr. Bader in the Ophthalmic Hospital Reports, and supplied materials for attacks upon the operation itself from the indiscriminating and prejudiced. Just so did the operation of trephining in cases of cranial fracture with depression fall for a time into great disfavour. It was performed in all severe cases, and of course a large majority of the patients died, much as they would have died had they been let alone. All the cases were lumped together, and it then appeared that not one in ten of our trephined cases resulted in recovery; *ergo*, trephining was a bad operation, and to be disused. After not a few lives have been sacrificed to this miserable logic, the trephine is now regaining the legitimate confidence *of surgeons*. Had our iridectomists selected their cases for operation, or

had they published only their very successful ones (*i.e.* those done in the acute stage), their triumph would have been much sooner and more loudly hailed. We need not remark, however, that the true interests of science would not have been served nearly so efficiently as they have been by the manly course which was pursued—*Brit. Med. Journal, March* 16, 1861, *pp.* 272, 280.

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## 81.—ON IRIDECTOMY IN GLAUCOMA, WITH A DESCRIPTION OF A NEW METHOD OF OPERATING.

By Thomas Nunneley, Esq., Leeds.

[The following paper by Mr. Nunneley it will be observed recommends nearly the same operative procedure as that by Mr. Hancock, recorded in our last volume (p. 208), although some changes are made in the operation, and the name is changed. We are sorry that Mr. Hancock's valuable paper is scarcely referred to.

Few ophthalmic surgeons really believe that the true position of the operation of iridectomy, as devised by Von Graefe, has yet been determined. Glaucoma depends upon inflammation or congestion of the choroid in the first place, the pressure is secondary. Von Graefe's operation, Mr. N. thinks, is only calculated to remove the effect of the disease; it has no power whatever over the original cause of it, the inflammation.]

Surely, in contending for the correctness of the supposition of the choriod being primarily complicated in glaucoma, which indeed appears most probable in at least many cases, our attention should be earnestly directed to the causes which first induced this condition. As it existed prior to the intra-ocular pressure, which it produces, whatever mischief this causes, or however relieved, it will in all probability continue to exist, even supposing the pressure to be removed, unless means be also found for its cure. This, however, apparently self-evident proposition need not, according to Von Graefe, be regarded. Only remove the pressure, and the eye, if not already absolutely disorganized, is perfectly cured by iridectomy alone. This doctrine appears to be so broadly stated by Von Graefe in the memoirs referred to as to be likely, if generally received, as from the great authority of the writer there is fear of, to be productive of great mischief by leading to a neglect of careful treatment in the earlier stages of these affections, when alone it can be really useful, and of encouraging a reliance upon a reckless resort to a serious operation as all that is requisite in the whole class of these affections, and that at a time when no benefit can possibly ensue, but, as results have proved, where danger of serious mischief supervening is not improbable.

But while endeavouring to recall attention to the necessity of carefully treating every early symptom, both local and general, and attempting by every curative means to prevent intra-ocular pressure by removing that condition of the choroid which gives rise to it, and, even after operation, of still striving to remove the primary cause of the mischief, we must feel grateful to Von Graefe for improving upon the efficacy of former modes of paracentesis by a needle or knife, and showing the greater success of another operation; for though we may not agree with him that iridectomy is the only thing requisite, that it is always free from ill consequences, and always to be practised, there can be no doubt that the larger operation has been of the greatest benefit in preserving vision in many eyes so affected. With these reservations, I do not think the argument which has been advanced against iridectomy—that it is merely a relief of a secondary affection, even according to Von Graefe's own confession—is of any great value; for in how many diseases is not interference directed to the removal of secondary affections, which of themselves may continue after it has subsided, and may be more serious and more permanent than the original disease which induced them? Inflammation of the urethra produces stricture; inflammation of the lachrymal sac, fistula; pressure upon an artery, adhesion of its walls; inflammation of the serous membranes, various effusions into the large cavities; of the mucous

membranes, ulcerations; of the synovial membranes, disease of cartilage, and so on. Hence, while we do not neglect attention to the primary affection the removal of the secondary is most important. That, in many cases, improvement in vision follows iridectomy is, I think, certain. How, then, this effected? In iridectomy, is the removal of a large portion of the iris essential part of the operation? Is the operation successful in spite of mischief inflicted by the removal of a large piece of the iris, owing to something else which is done during this removal? And, if the former, how does this occur? if the latter, in what does this consist? Graefe, those who are convinced by his reasoning, and think his facts incontrovertible, unhesitatingly declare the removal of the iris to be the essence of operation, without which, according to them, it would be a failure, and useless as they say paracentesis is. They attribute permanent success to a diminution in the extent of iris surface, so that thereafter there is a less power of secreting aqueous humour, by which the tension of the whole globe is removed, and the optic papilla preserved in its integrity, or recovers in case pressure has not already too far disorganized it; at the same time the cornea, which had been rendered flatter, recovers its normal curve, and the vitreous humour becomes softer and clearer.

To some, at least, of these suppositions I would venture to demur. I would remark in the first place, that the idea of there being less secretion of vitreous humour by the removal of the inflammation and congestion of the choroid through the evacuation of the aqueous humour and ablation of a portion of the iris, is an assumption and not an ascertained fact; and, secondly, even if this be so, the notion that removing part of the iris proportionally lessens the secretive power of aqueous humour is a mere hypothesis resting upon a very problematical supposition. That the iris is the source whence the aqueous matter is derived is by no means proved, and, to say the least, is very doubtful. I know of no anatomical fact which would prove the iris to be the secretor of this humour; for while all microscopists agree in the presence of epithelial cells upon the posterior surface of the cornea, there is no such agreement of opinion as to their existence upon the iris. While Hassall, Kölliker, and some others, believe that these cells are continued upon the anterior surface of the iris, Bowman decidedly denies it; and though I have in one or two instances seen them, in the great majority of examinations I have altogether failed in finding them. So, then, this surface is certainly not more adapted for the office than is the cornea; and it will surely not be contended by any one that the posterior surface of the iris is more so than the ciliary processes. Hence we can hardly admit as of much value the explanation put forward as to the *modus operandi* of iridectomy.

The necessity for the removal of a portion of the iris, and the statement that the larger the piece removed the greater is the certainty of success, is a question of fact to be determined by observation alone. Upon the question of fact I incline to join issue, and think that the removal of the iris *per se* is an evil to be avoided if possible—one which in recent cases may commonly, and in those of longer standing not unfrequently, be done. The good accompanying its removal does not, in my judgment, result from the loss of the iris itself, but from allowing a greater yielding of the eye-ball—in all probability owing to a greater division of its curve being made when a large portion of the iris is taken away than when none of it is removed, and thus permanently lessening its tension, as well as affording a longer continued drain of the aqueous humour.

Though it has been stated by Von Graefe that the eye becomes presbyopic in glaucoma, owing to the intra-ocular pressure causing a flattening of the normal curvature of the cornea, which iridectomy removes at the same time and by the same means as it does the hollowing of the optic papilla, this change of curvature, as an invariable symptom of glaucoma, and as a necessary consequence of intra-ocular pressure, has been denied by other good observers. So far as my observation extends, it is by no means constant. Indeed, I think I have seen great intra-ocular pressure, with an increase in the curve of the cornea, in some cases where the cornea has been

more yielding than the sclerotic. That the cornea frequently is flattened is certain, and that it then becomes more prominent after operation is also true; not so much, however, I apprehend, from lessened tension within as from diminished resistance in itself. The division of so much of its connexion with the sclerotic (and these tissues are really one, so that whether the incision be made a little more forward or backward is of small importance) has weakened its power. By the operation I propose as a substitute, this increase in the corneal curvature is certainly obtained in a marked degree, more so, I think, than in Von Graefe's iridectomy.

I believe, then, all that iridectomy accomplishes in the cure of acute glaucoma and glaucomatous diseases is in the greater degree and more permanent manner in which it affords relief to intra-ocular pressure than paracentesis, as performed previous to its introduction, did.

Can, then, any simpler means be devised for obtaining this desired result than removing a large portion of the iris? I think it can.

Observing that the eyeball is often distended to the utmost limit which the comparatively unyielding sclerotic and cornea will allow, and that the pain and acutely distressing symptoms in the ball and about the orbit commonly occur in proportion to the rapidity with which the distention takes place, whether the disease be glaucoma, iritis, or choroido-iritis,—and knowing that the most unyielding portion of the globe is the point of junction of the sclerotic, cornea, iris, and ciliary muscle, which may not unfrequently, in very decided cases of hydro-ophthalmia, be observed as a depressed ring between the bulging sclerotic and cornea,—it occurred to me that division of this part would afford the desired relief, and that not improbably the good gained in Van Graefe's operation in reality depends upon the removal of the resistance of this part, and not upon the ablation of the iris. This result, I think, has been proved. I have waited before announcing it until I have had a sufficient number of cases as fairly to admit of a comparative estimate of it with iridectomy. I have now operated upon about fifteen eyes by the method of Von Graefe, and on certainly not less than twenty eyes in the manner I propose to describe. The result has been in favour of the latter operation. It has, so far as I can judge, afforded all the relief that the more serious proceeding has done, and appears to be free from its inconveniences. It is easier performed, produces much less deformity, inflicts much less mischief upon the eye, is followed by as much diminution of the intra-ocular tension, by as great or even greater increase in the corneal curvature, and the relief has been quite as permanent. The space over which my observation has extended is upwards of eighteen months.

The manner in which I have operated is to puncture the sclerotic coat with the point of a sharp, thin knife—a small cataract or very narrow, short bistoury answers very well—not less than one-eighth of an inch behind its junction with the cornea, and carry it on to about the same extent through the cornea, making altogether an incision about one third of an inch long. Care must be taken to pass the knife sufficiently deep to completely divide these textures, and yet not so deep as to touch the lens, which I once did, owing to the patient starting at the moment the incision was made. Care also must be taken not to make the incision too long. A larger incision in the sclerotic, besides unnecessarily wounding important tissues, is useless, and if carried too far towards the centre of the cornea, though allowing this afterwards to yield more, is bad, for it may allow the lens to be displaced into the aqueous chamber; and if the iris should adhere to the whole extent of the corneal section, as it is likely to do, particularly if a portion of its whole breadth has been removed, not only will there be dragging of it, but the section becomes opaque, and hence the field of vision is lessened. In making the section, if the point of the knife has been well kept in, the outer margin of the iris will be divided. Sometimes the iris bulges through the section. I have tried the effect of simply leaving the prolapsed iris in the wound, of cutting it off, and also of pulling out a larger portion, and cutting off a strip through the entire width. In this latter plan the operation more nearly assimilates with Von Graefe's iridectomy, only that the section

through the unyielding tissues is made directly across their junction, instead of into or parallel with it, whereby a greater expansion in it is allowed, and not nearly so much of the iris is removed. If none of the iris be cut off or tied, the pupil usually recovers its circular form; if some be excised, it remains oval and attached to the corneal cicatrix in proportion to the size of the piece removed, but in a much less degree than would *à priori* be anticipated. The degree of deformity is very slight indeed.

The spot where the incision is made, so far as the relief obtained is concerned, can make very little difference; but I have usually selected the centre of the lower corneal curvature as being likely to produce less noticeable alteration in the appearance of the eye; and, when both eyes are operated upon, as interfering less with accuracy of vision than any other. On the whole I think, in severe cases, the removal of some small section of the entire width of the iris advantageous, as affording a longer continuous drain of the aqueous humour than simple incision of the scleroto-corneal junction does. I have in one case only seen much bleeding into the aqueous chamber, and what blood is there effused is soon absorbed. In one case the iris gave out more blood than usual—perhaps twenty drops; but as this at once escaped by the section, it was of no moment, and was easily arrested by cold, wet cloths. In two of the cases in which I performed iridectomy, the quantity of blood effused into the eye was sufficient to fill the aqueous chamber; and in one of them it was a long time before being entirely absorbed. Usually, however, all trace of it is lost within a week, or, at most, ten days. In two or three of the cases severe pain in the side of the head ensued, which an opiate relieved; and in one instance, as I have before said, considerable inflammation; but in all other cases very little inconvenience followed.

In hardly one instance have these operations been performed in really strong, vigorous persons. The majority were females. Though two were large and apparently strong, vigorous young women, both had been in bad health for several months. In all, after the immediate effects of the operation and the more acute symptoms have subsided, I have thought it proper to continue for many weeks, or even months, constitutional treatment. Principally iron, iodine, hydriodate of potassa, with strychnia or quinine, and mercurial purgatives, have been given, by which the constitution has been materially invigorated, and I have no doubt the eyes, in a corresponding degree, rendered more healthy.

I have also performed this operation upon both eyes, in an extremely bad case of conical cornea, where the alteration in the curve of the cornea had been progressing for nearly ten years, until the sight was so bad that the patient had been unable to do even the weaving of plain, coarse woollen cloth, where hardly sight is required, with the happiest result, as the man is now not merely enabled to work at a better description of goods, and thus support himself and family, but possesses a very fair range of vision; indeed this operation, with a little modification, appears remarkably well adapted for the relief of this complaint. The section of the scleroto-corneal junction allows the periphery of the cornea to bulge out, and thus the convexity of the whole surface of the cornea is lessened: and by drawing out not too large a portion of the whole width of the iris, and either cutting it off, or, still better, fixing it at the wound by a fine ligature, adhesion takes place, and a good contractile pupil is made near to the margin of the cornea. The best place for the section to be made in these cases is at the outer and lower margin of the cornea, by which a large field of vision is obtained.

Mr. Hancock has recently written more than one paper in the Lancet on glaucoma and the relief of it by operation, in which he controverts both the theory and the practice of Von Graefe, maintaining both are wrong. Mr. Hancock believes the disease does not depend upon hypersecretion of the vitreous humour owing to disease of the choroid coat, but that it essentially depends upon continuous spasm of the ciliary muscle, which, according to him, induces the hardness in the globe of the eye. Hence the proper treatment, he maintains, is not ablation of a portion of the iris, but division of *the ciliary* muscle by a small incision through the sclerotic coat immediately

over it, and parallel with its fibres, by which he considers the spasm is overcome. He has published cases in which he and other surgeons have operated with success, notwithstanding, as he says, Mr. Hulke has denied the possible success of any other method than Von Graefe's iridectomy. Mr. Hulke, who is strongly impressed with the superior curative power of iridectomy, has, to prove the impossibility of Mr. Hancock's theory being true, referred to the dissection of glaucomatous eyes, in which he states that the ciliary muscle has been found atrophied and completely shrunken —a condition which Mr. Hulke considers incompatible with hyperaction. On the other hand, in rejoinder, Mr. Hancock informs us that his colleague, Mr. Hogg, has dissected glaucomatous eyes, in which the ciliary muscle has been found to be decidedly hypertrophied, and therefore must, they think, have produced the disease in the way he contends for.

I would venture to remark that I think very little value is to be attached to any of these dissections, because, in the first set (those of Mr. Hulke), if, as is almost certain was the case, the glaucomatous condition was of old standing, it is hardly probable the ciliary muscle alone was atrophied, but that its wasted state formed part of a general ocular condition, and as such, proves nothing as to the ciliary muscle alone; while as to the second set, those of Mr. Hogg, we all know that a slight variation in the size and thickness of the ciliary muscle may and does occur in different eyes, without there being any suspicion of diseased hypertrophy. Besides, it must be most difficult to detect any such minute change as mere simple hypertrophy in so small a muscle, if confined to it alone, would occasion; and in an eye really glaucomatous it is most improbable this small ciliary ring would alone be found altered, whatever the alteration may be, and hence, like Mr. Hulke's cases, of no value as to the true condition of the ciliary muscle alone. The dissection of a sufficient number of glaucomatous eyes, under circumstances to be of much value, is hardly likely to happen to any one, as happily few persons die at any rate with the affection in an early stage, and hence the opportunities of seeing the disease uncomplicated are very rare.

It cannot, however, I think, be at all difficult to show that neither the theory nor the practice, as applicable to the theory of Mr. Hancock, can possibly be true, without in any degree disputing his statement of improvement following his operation, which appears most probable.

Supposing the ciliary muscle were really hypertrophied and in a state of tonic spasm, how could it induce hardness of the whole globe of the eye, flattening of the cornea, and much intra-ocular pressure? Muscles unstriped as well as striped act only in proportion to their length, in the direction of their fibres, and towards their most fixed attachment. The fibres of the ciliary muscle, firmly attached to the scleroto-corneal junction, radiate outwards and backwards from this point upon the soft yielding choroid. The fibres are not circular, but straight, and not more than one-seventh of an inch long. They can have no action whatever upon the sclerotic coat, to which they are not attached posteriorly; they therefore cannot render it hard. Whatever effect they may have upon the choroid would rather be to pull it away from the sclerotic, and hence, if of any effect upon the latter, to render it less tense. Though the ciliary muscle be called by some the tensor of the choroid, it can hardly act much in this manner upon this coat, whatever it may upon the ciliary processes. But even were this tensile action proved, and the muscle contracted to its entire length, the choroid coat is too extensive, too lax, and too yielding, to allow of the muscle dragging the whole coat inwards with sufficient force to compress the retina and vitreous humour in any great degree, and it certainly could by no possibility cause any atrophy or cup-shaped depression of the optic-nerve papilla, as all agree does occur in glaucoma; since the cribriform part of the sclerotic coat is *outside* the choroid, and therefore altogether beyond even the theoretical power of the ciliary muscle.

But even were this theory of compression by the ciliary muscle as true as it appears untenable, how could a simple puncture in the direction of its fibres interfere with the action of the entire circle of its fibres? A broad

transverse incision in the direction of the corneal curve, by dividing the fibres, might act powerfully in proportion to the number of fibres divided, but that a simple momentary separation from each other of two or three adjoining parallel fibres, without any division of their structure—for such must be the effect of a fine, sharp, thin knife—could permanently arrest strong spasmodic action in an entire muscle spread over a large circle, it is impossible to conceive.

Mr. Hancock's practice I imagine to be more successful than his theory. It will, I apprehend, be found successful in proportion as the most unyielding part of the outer tissues of the globe are divided; and as there can be no doubt of the correctness of the observed facts of improvement in vision following Mr. Hancock's operation, I think I may claim his cases as strongly in favour of the operation I have suggested, and in support of the explanation I have ventured to give as to the *rationale* of it,—namely, that the improvement following his operation results from a division of the unyielding outer coats of the eyeball, and not, as he supposes, by overcoming spasm in the ciliary muscle, or, as Mr. Hulke thinks, merely by paracentesis, as a simple puncture in any other part of the eye would do, and which has in other hands so uniformly failed in affording more than the most temporary relief.—*Lancet, Jan.* 19 *and* 26, 1861, *pp.* 53, 82.

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## 82.—ON THE FORMATION OF ARTIFICIAL PUPIL BY IRIDDESIS.

By George Critchett, Esq., Surgeon to the Royal London Ophthalmic Hospital.

In a former paper (see *Retrospect*, vol. xxxvii, p. 311, vol. xxxviii, p. 187,) I have described a method of forming an artificial pupil, then recently adopted in several instances, and that I had reason to believe possessed some important advantages over the operations usually had recourse to. It consisted in drawing a portion of the iris through a small wound in the cornea, and tying it with a fine loop of silk; its advantages, as I mentioned in that paper, are, that the iris-fibres are not torn or divided, that the pupil is merely altered in its shape and position, and retains permanently a definite size, a clearly defined boundary and contractility under the influence of light. Again, by this method we can regulate with great exactness the size and position of the pupil, and thus obtain every optical advantage that the circumstances and specialities of each individual case permit.

My present object is to describe some modifications that I have found it convenient to make in performing the operation of iriddesis, and also to describe some cases illustrative of the various morbid conditions for which this operation is suited.

In order to perform this operation with the requisite amount of minuteness and precision, a perfectly quiet state on the part of the patient is important. Except in rare cases this is best accomplished by the use of *chloroform*. The eye having been exposed by means of the wire speculum, and fixed in a convenient position, a broad needle must be entered close to the junction of the cornea and sclerotic, and an opening made *just of sufficient size* to admit the canula forceps. A small loop of fine *floss* silk, moistened, must then be *laid on the eye over the opening in the cornea*. The canula forceps is then introduced through the loop and the opening in the cornea into the anterior chamber. The iris is then seized about midway between the ciliary and pupillary margin, or sometimes, if possible, rather nearer to the ciliary margin, and drawn out *to the requisite extent*. An assistant seizes the two ends of the loop with small broad-ended forceps, and *carefully ties it upon the eye*. so as to include and strangulate the portion of iris that has been drawn out. The ends are then cut off, and the operation is completed.

Usually about the second day all trace of the ligatured portion of the iris is gone, and the bit of silk, if it has not been washed off, may be taken away. *It* occasionally happens that the ligatured portion of iris, with the piece of

silk attached to it, is drawn somewhat within the wound; when this is the case an end of the silk must be seized with forceps and drawn away. The possibility of such an occurrence proves the necessity of leaving the *ends of the knot rather long*. It is very rarely indeed that any irritation of consequence follows this operation; the iris becomes permanently adherent at the spot where it has been tied, so as to fix the pupil in its altered form and position.

This method of operating has been adopted with favourable results in the following groups of cases:—

1st. *Central leucoma*, corresponding in size and position to the natural pupil.

2nd. Where there has been a *penetrating ulcer* of the cornea, with some surrounding opacity, and the pupil is altered in shape, diminished in size, and drawn towards or behind the opacity.

3rd. Cases of *opaque capsule, with adherent pupil*, in which it is desirable to enlarge the pupil slightly in one direction, so as to expose a clear part of the lens.

I have performed the operation in each of these groups of cases on several occasions, and I will select a specimen of each, as it may serve further to illustrate and explain the principles of the operation.

1. In November 1858, a boy, aged 14, was brought to me by his mother at the Royal London Ophthalmic Hospital. He had dense central opacity of the cornea of both eyes, the result of severe strumous ophthalmia about five years previously. He could just manage to guide himself about, but his sight was so imperfect that he was unable to read even large print, and his education in consequence was entirely neglected; the pupils were active, but when contracted were hidden behind the opacity. The largest clear space of cornea was at the lower part of each, I therefore determined to alter the shape of the pupil in that direction, so as to bring it opposite to a clear part of the cornea; this I did in both eyes. The patient was placed under the influence of chloroform, and I drew out and tied a portion of the iris; but in the left eye I drew out rather more than I did in the right. The result of this was that the pupil in the right eye only extended to a point midway between the ciliary and pupillary margins of the iris (*a*), and retained its natural contractility, whereas in the left eye it extended as far as the ciliary attachment (*b*), and had lost its natural contractility. The boy made a rapid recovery without any unfavourable symptom. On the third day I was able to remove the silk, all trace of the prolapsed and ligatured portion of iris was gone, and the pupils were exactly of the size and shape that I had originally made them. Some weeks afterwards, when the eyes had completely recovered, I tested the sight, and found that in the right eye, where the pupil was small and moveable, it was much more perfect than in the left, where the pupil extended as far as the ciliary margin. With the right he could read ordinary print, but he could not read at all with the left. This may be partly due to the condition of the cornea in the right eye; but I am inclined to attribute it in a great measure to the more central position, to the mobility and limited size of this pupil, and, above all, to the circumstance of its not extending to the extreme margin of the cornea, as the pupil did in the left eye.

*a.*

*b.*

I have performed the operation in several similar cases, sometimes trying how small a portion I could draw out and tie. On the last occasion, the piece that I drew out, when included in the ligature, was only about the size of a small pin's head, and the pupil was very slightly altered in shape (*c*). The sight in this case was very perfect. The condition of the artificial pupil is not, however, always a fair test of the operation. It may be everything that can be desired, both as regards size and position, and yet the sight may be very imperfect. This may arise from an altered state of the cornea, the re-

*c.*

sult of the same inflammatory attack that produced the opacity, and by the perfect smoothness of its convex surface having been changed into a series of little flat surfaces, or the retina and other structures may have been compromised by the original inflammatory attack.

The conditions most favourable for a good result are a *clearly defined dense opacity* in the centre of an otherwise transparent and healthy cornea. If a pupil, rather less than the normal size, and retaining its natural boundary and mobility, be brought opposite to the clear part of the cornea very excellent sight is obtained.

The second group of cases, in which "there has been a penetrating ulcer of the cornea, with some surrounding opacity, and the pupil is altered in shape, diminished in size, and drawn towards or behind the opacity," to which it is adherent at one point, presents great variety, both as regards the size and shape of the remaining pupil, the extent of the opacity, and the condition of the transparent part of the cornea; and upon these circumstances must, in a great measure, depend the degree of sight that is obtained; at the same time, I think it must be admitted that iriddesis is peculiarly well adapted for these cases, enabling us to regulate the size and position of the pupil so as to bring it opposite the best part of the cornea, and at the same time to make it with certainty, small and moveable, and with a well-defined and permanent boundary. I have operated upon several of these cases in this manner with satisfactory results, and will briefly detail one that presented some circumstances of peculiar interest.

2. A gentleman came to consult me, from Australia, suffering from severe ophthalmia in both eyes of about two years' duration; in one, vision was entirely and hopelessly destroyed; in the other, there had been a penetrating ulcer that had produced a dense opacity extending over nearly half the cornea, behind which the pupil, of an oval shape, adherent at one part, and diminished about one-third, was drawn. For some months the eye remained so inflamed and irritable, that nothing could be attempted for the improvement of the sight; at last, however, under the influence of a generous diet, change of climate, and soothing topical treatment, the eye regained a healthy condition, the opacity diminished in size, and became more clearly defined and circumscribed, and the pupil extended just to its margin, so that in a subdued light the patient could make out the outline of an object; and, under the influence of atropine, a further slight improvement took place, but not to such an extent as to give any useful vision. Under these circumstances, I proceeded to enlarge the pupil downwards and inwards, so as to bring it opposite to the clear part of the cornea, rather less than its normal size, retaining its defined border and its contractility, and occupying a position midway between the corneal opacity and the margin of the transparent cornea. The operation was performed in the usual way; two days afterwards the bit of silk came away; every trace of the deligated portion of iris was gone, and in a few days the eye had regained its strength. Before this gentleman left England, he could not only see distant objects well, but could read writing and clear print; and I hear that, since his return to Australia, his sight has still further improved.

I think it will be conceded that every possible optical advantage was obtained in this case that the previous condition of the eye permitted, and that any other mode of operating, or any other form of pupil, would have produced less perfect results.

The third group of cases are those in which the pupil is irregular, contracted and opaque, or occupied by an opaque membrane. The object in such cases is to detach the pupil at one point from its adhesion to the opaque membrane, and then to enlarge it in one direction, so as to discover and expose a clear part of the lens and its capsule. This operation has been performed several times at the Royal London Ophthalmic Hospital, and on some occasions with very satisfactory results. It is more difficult to carry *out* successfully the various steps in the operation than in either of the other

groups. One difficulty is the firmness of the adhesion, and another is the softness and rottenness of the previously diseased iris.

3. In a case that came under my care, I adopted the following method of proceeding. The patient was a female, between 40 and 50 years of age, who had formerly suffered from recurrent iritis, which had left a small, irregular, and adherent pupil, and some opacity of the capsule. The eye having been exposed, and fixed with speculum and forceps, and an opening having been made through the cornea, I first introduced Tyrrell's blunt hook, and getting it through the edge of the adherent iris, made gentle traction upon it, so as to detach it from its adhesion. This I succeeded in doing. I then freed the hook from the iris, and drew it out. I then introduced my short canula forceps, and seized the iris about midway between its pupillary and ciliary attachment, and drew it out, and tied it in the usual way. The effect of this method of proceeding was to form a small triangular pupil by the side of the opaque part of the capsule, and opposite to a transparent portion of the capsule and lens. The sight was very much improved in this case.

*f.* *g.*

This operation has been performed several times at the Royal London Ophthalmic Hospital, both by some of my colleagues, and by myself, and sometimes with a very satisfactory result; it is, however, only applicable to those cases in which the opacity of the capsule is limited to that part around which the contracted margin of the pupil is attached, and in which the rest, both of the capsule and of the lens, retains its transparency. It is scarcely possible, prior to the performance of the operation, to determine this point; but in the event of this further complication being revealed by the artificial pupil, it constitutes a good preliminary for further proceedings, and is in itself very safe. I find by referring to the hospital records, that the operation of iriddesis has been performed by my colleagues and myself sixty-six times during the past two years, and in a large majority of cases with very satisfactory results; I have also adopted it four times in private cases with good success.

It seems highly probable that the range of cases to which the operation is applicable, cannot yet be limited, and in proof of this, I have much pleasure in adding a very interesting case furnished me by my friend Mr. Streatfeild, in which the operation has been applied by him in a novel and highly satisfactory manner.

"A farmer, aged about 50 years, came to me at the Moorfields Hospital, on the 15th of September, 1859; he had had cataract of one eye only, for which extraction had been performed about a year previously, with subsequent prolapse of the iris, through the corneal wound, by which not more than, if so much as, one-half of the natural circumference of the pupil was left free (*h*), and this was all above the axis of vision, and partly hidden by the upper lid; nevertheless, he could, with it, distinguish some objects, and by raising the upper lid and using a (2-biconvex) lens he could read small capital letters.

*h.* *i.*

"I performed iriddesis in the downward direction; it was difficult to seize the portion of iris to be tied, as it had lost the support of the lens, its fibres were much stretched, and therefore it receded before the forceps altogether. Having once failed to draw out, by the corneal opening, the portion held by the forceps, I succeeded the next time in securing it with the ligature of floss silk.

"On the 19th (the iris was adherent to the new corneal wound) I found the knot remaining in the cornea deeply, as if drawn into the wound by the tense iris fibres; it had not, however, passed through the cornea, and I readily removed it with forceps.

"The pupil now (*i*) acted perfectly; although, before the operation, it had not seemed to act at all. The pupil was now in the axis of vision; but below it is a minute narrow vertical rent in the iris, probably made in the

operation, when I, the first time, took hold of a portion of it; this, however, appears to be no great disadvantage, for the man voluntarily, without a lens, said he now could see better than he did.

"On the 26th, the pupil remained as at the last date, and acted well; all that portion of iris not involved in the ill-result of the extraction operation *being now of about its natural width.* With a (2-biconvex) lens, he could already, without effort, read with the eye operated on, rather small type. The eye looks altogether as well as can be, and its external appearance is much improved by the operation on the pupil."

Mr. Streatfeild has since added to the above that he has had "a second case of similar displacement of the pupil after 'extraction.' In this instance the cataract operation had involved the iris to an amount as great as in the first case, but the cornea retained a better form. The patient, however, could not (with a lens) see capitals; but, by iriddesis, he could tell the time by a watch, and (with a lens) read the smallest type."

I have been anxious, in the present paper, to point out the easiest and best method of performing the operation, to indicate clearly the various classes of cases to which it is applicable, and to give the results and impressions which a more extended trial has given me. The only objection that I have been able to detect, is an occasional tendency to a drawing in of the loop through the wound into the anterior chamber. If the ends of the silk are left sufficiently long, this may be easily remedied by seizing a loose end and drawing it away three or four days after the operation; but the occurrence itself is exceptional, and may be obviated by making the opening in the cornea very small, and by inclosing the entire piece of prolapsed iris in the silk. The advantages of iriddesis I have already dwelt upon, and may briefly restate:—It is applicable to nearly every variety of case in which an artificial pupil is required; it enables the operator to regulate with accuracy the exact size and position of the pupil he proposes to make; it ensures to it a defined boundary and the natural mobility (provided some portion of the pupil in its natural state existed prior to the performance of the operation); it secures stability to the artificial pupil, so that it will continue of the size and form it had when first made. After a little practice the operation is performed with great ease and certainty, and recovery is almost uniformly rapid and complete. I believe that it possesses important advantages over every method hitherto proposed, and that it enables the operator to ensure every optical advantage that the nature of the cases, and the previous condition of the eyes, permit; and I confidently anticipate its very general adoption.—*Ophth. Hospital Reports, Oct.* 1859, *p.* 145.

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## 83.—PRACTICAL OBSERVATIONS UPON CONGENITAL CATARACT.

By George Critchett, Esq., Surgeon to the Royal London Ophthalmic Hospital.

[The writer considers the subject under two heads, 1st. The disease as found in young infants; 2nd. As in children and young adults. At no period of life are operations so well borne, or so successful as in infancy. Each plan of operating seems nearly equally safe. In cases where the entire lens is opaque, it may be removed at once through a linear opening in the cornea, or it may be broken up and gradually absorbed in the aqueous chamber.]

In December 1860, a little boy, aged 2, with congenital cataract involving the entire lens, was brought up from the country, accompanied by a sister, aged 18, also suffering from partial congenital cataract, the cataracts were dense and only permitted perception of light. Under chloroform I made linear openings at the outer part of the cornea, to the extent of about two lines and a-half, then carefully, but freely opening the anterior capsule, using caution not to wound the posterior capsule, or enter the vitreous chamber, the cataracts were removed gradually by two or three introductions of a flat silver spoon; on opening the capsule, the nucleus escaped into the anterior chamber, and was removed first, then the cortical portion, leaving clear

pupils. Rapid recovery took place with small central pupils, and with excellent vision, and for days the little fellow was kept in a constant state of excitement in gazing for the first time upon surrounding objects. If the case could have been watched for some months, the same results might have been obtained by the old method of solution, and perhaps as a rule a more perfect result is obtained by this method in regard to the mobility and central position of the pupil; but care is required to renew the operation from time to time until all the lenticular matter is absorbed, and to avoid the formation of a tough opaque capsule within the field of vision. In infants, the lens may be very freely broken up and large portions may escape into the anterior chamber, without setting up any serious reaction or inflammation. I have even known the lens escape from its capsule, and become dislocated into the anterior chamber, and gradually undergo absorption, without producing any unfavourable consequences; this state of things would produce serious inflammation in an adult. In those cases in which the opacity is limited to the nucleus of the lens, some modification of the treatment is necessary; these cases differ in the extent to which the nucleus is involved, and in the degree of density in the opacity—usually in infants the opacity is well marked; these cases may be treated upon the same principles whether they are observed in infancy, in childhood, or in adult age.

We have now to consider the subject of treatment in partial congenital cataract, whether it comes before our notice in infancy, in childhood, or in adult age. In infancy, as I have before remarked, the opacity is usually strongly marked and dense, however it may differ in extent, and if it be of sufficient extent to occupy the pupillary area, when the pupil is in its normal state, it will seriously interfere with vision, and require some operative proceeding. There are three methods that may be adopted in such cases:—1st, to remove the cataract by solution; 2ndly, to perform linear extraction; 3rdly, to make an artificial pupil. The first of these methods is so well understood, and has been so long, and so scientifically practised, as to require no comments here.

The other two methods seem to me to merit some observations.

In the performance of linear extraction for partial congenital cataract, it is desirable that the adhesion of the lens to its capsule be loosened, that the transparent portion of the lens be made opaque, and that the general consistence of the entire lens be reduced, and its substance be rendered more diffluent. The principles upon which this operation may be most safely, quickly, and completely performed, have been carefully worked out and practically illustrated by my friend Mr. Bowman. His method is as follows:—The pupil having been completely dilated by means of a strong solution of atropine, the capsule is freely opened with a fine needle, and the lens is thoroughly broken up by repeated small strokes of the needle in every direction, at the same time the utmost care is taken not to wound the posterior capsule, or enter the vitreous chamber. After this operation, the pupil must be kept well dilated with atropine. The effect of this part of the operation is to render the entire body of the lens opaque, to increase its bulk, and to soften it by dilution with aqueous humour, and to separate it from the capsule. It also sometimes causes a bulging of some parts of the broken lens through its capsule into the anterior chamber, which may be followed by more or less rather severe reaction. After a period varying from three to six days, and regulated somewhat by the amount of the reaction, and by the change produced in the substance of the lens, a linear opening is made at the margin of the cornea, and with a small spoon, the lenticular matter is gradually removed, leaving a clear pupil. Atropine must again be freely employed to keep the pupil well dilated, until all irritation has passed off. If these various steps are carefully and skilfully executed, a very good result is almost invariably obtained. The advantages of the proceeding are, the rapidity with which the cataract is removed, and the avoidance of any of the inconveniences attending gradual solution, such as, considerable delay, slow inflammation, opaque and tough capsule, closure of pupil, &c. The risks attending the operation are, severe pain and reaction, threatening

the safety of the eye, and a distorted and adherent pupil. I think it safer to make a tolerably free linear opening, rather than a small one, at the time the extraction is performed. I have certainly seen several cases most successfully treated in this way, and I think that both theoretically and practically the operation seems scarcely capable of any further improvement. I have frequently tested it, and have obtained favourable results, at the same time the impression left on my mind is, that in cases where time is no object, a more perfect result may be obtained, and inflammation may be more completely avoided, by the old method of solution; in this way a small central bright moveable pupil is obtained, securing the utmost amount of vision, that the eye when deprived of its lens is capable of. The third method to which I am anxious to draw attention is by the formation of an artificial pupil. This method is comparatively recent in this country, and its merits can only be determined by further experience; in the meantime it may be useful briefly to sketch the history of the operation up to this time, the cases to which it seems most applicable, the objections to the proceeding, and the advantages it offers as compared with the removal of the lens.

I believe Professor Graefe of Berlin was the first to make an artificial pupil in cases of congenital cataract, sometimes in combination with linear extraction, and sometimes as a single operation. His method was by iridectomy; I have no details of his operations, but I have reason to believe he was satisfied with the results.

Within the last twelve months, Professor Pagenstecher has performed iriddesis in several cases of partial congenital cataract; he considers this method preferable to iridectomy, and has indicated certain points that he thinks important to be carried out in the performance of this operation. Thus he advises that the opening should be made in the sclerotic, just anterior to the ciliary ligament, so that the natural plane of the iris be preserved; also that a considerable portion of the iris be drawn out and tied, so as to draw the entire pupil near to the margin of the cornea, and thus cover as much as possible of the opaque part of the lens with the iris, and leave a well-defined small moveable pupil opposite to the transparent part of the lens. This plan, he contends, gives the best result optically, and is easily accomplished. I have performed this operation in three cases.

The first in November, 1860, Master B., aged 8, was brought to me with symptoms of short and defective sight, both for near and distant objects; cannot face the light; puts himself into peculiar attitudes when reading, and in twilight and candlelight seems nearly blind; he reads No. 14 of Jaeger with difficulty; his sight is not much improved by atropine unless No. 16 convex is employed, and by its aid he reads No. 10, he also sees distant objects better when atropine is used. With the ophthalmoscope partial congenital cataracts could be distinctly seen, rather more than half the centre of the lens was opaque, the margin being quite transparent. Under chloroform I performed iriddesis downwards and inwards, making the opening in the sclerotic and drawing out as much of the iris as I could through the opening, so as to make the pupil small and near the margin, and therefore opposite the transparent part of the lens, and at the same time to cover the remainder of the lens as much as possible; this object I accomplished more completely in one eye than the other, no irritation followed, on the second day the silk tie came away, and within a week the eyes were quite recovered. Sight was very much improved both for near and distant objects; could read No. 4 at 7 inches, or at least spell the words at that distance; and a few weeks ago I heard from his mamma to say that she found that her boy could distinguish distant objects as well as her other children, and that he was able to continue his education in a satisfactory manner.

The next case was a young female, aged 19, who brought her little brother to the Ophthalmic Hospital with congenital cataracts, and whose case I related at the early part of this paper. She was found to be suffering from partial congenital cataracts, in the left eye the opaque lens was partly *detached at* the lower part and sides, and swung backwards and forwards in the *vitreous* chamber. The sight was very defective in this eye, there being lit-

tle more than perception of light, and the ophthalmoscope shewed evidence of changes in the choroid and retina, all of which were probably intra-uterine. The other eye when under the influence of atropine shewed a partial opacity of the lens with a clear margin, which margin seemed broader at the lower part than at the remainder of the circumference; her sight was very imperfect both for near and distant objects, and was not improved by either concave or convex glasses. She could read No. 14 of Jaeger with difficulty at four inches distance, and could perform coarse needle-work, but could never thread her needle. Her sight was somewhat improved by the atropine, but not to any great extent. Under chloroform I made an artificial pupil downwards in the same manner as in the last case, the eye rapidly recovered, and before leaving the hospital the patient could thread an ordinary needle, and read No. 6 of Jaeger at an average distance. The third case was in a little girl, aged 6, who was first brought to me on January 15, 1861. She was very myopic, and intolerant of a strong light. She had been taught to read letters the size of about 18, Jaeger, and could not read 16. Her sight was much improved by atropine. Partial congenital cataracts were distinctly visible, the margin quite clear and appearing to be broadest below. On the 29th of the same month I performed iriddesis downwards in both eyes, and the result was most satisfactory, perhaps rather more so than in either of the two other cases. A fortnight after, this little girl could spell with rapidity the words of No. 4 of Jaeger, and could see distant objects well. The appearance of all these cases, after the operation, was very similar, and under the ophthalmoscope was rather singular. In all these was seen a small active balloon-shaped pupil; about one-third of the greater extremity of the pupil presented the opaque part of the lens, the remainder a perfectly transparent circumference up to the very edge of the lens, beyond which a thin transparent line of illumination was visible; through the extreme boundary of the cornea the fundus was clearly visible.

The advantages of this method, as contrasted with the removal of the cataract in partial congenital cases, seem to be, the extreme simplicity and safety of the operation, the certainty with which the size and position of the artificial pupil can be regulated, and the preservation of the lens, together with the natural focusing power of the eye, rendering unnecessary the use of glasses. In cases also where the opacity is so faint, and the sight so slightly impaired, as scarcely to justify removal of the lens, iriddesis seems to offer a satisfactory compromise. The only objections to the proceeding that suggest themselves to my mind are, the possibility of the margin of the lens becoming subsequently opaque, thus rendering the operation futile; and again, the transparent margin being in some cases so narrow, that the improvement to vision is very slight. In the event of the former, viz., the supervention of opacity of the margin, it would still be competent to remove the cataract under quite as favourable circumstances, as if an iriddesis had not been performed, and according to some high authorities even more so; and as regards the latter, the success of every proceeding must necessarily depend upon the judicious selection of cases. In young people the power of dispensing with cataract glasses seems to me a great advantage, and after weighing every point, I am drawn to the conclusion, that the application of iriddesis to properly selected cases of congenital cataract, is an improvement in ophthalmic surgery.—*Oph. Hospital Reports, April*, 1861, *p.* 137.

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## 84.—ON CONICAL CORNEA, AND ITS TREATMENT BY OPERATION.

By W. Bowman, Esq., F.R.S., Surgeon to King's College Hospital, and to the Royal London Ophthalmic Hospital.

Conical cornea is a thinning and bulging of that structure in its middle region, coming on imperceptibly, and altering or destroying the natural focus of the eye. Up to this time its anatomical conditions have hardly been fully

ascertained, and though many optical and operative correctives have been applied to improve vision, occasionally with some success, there is room for further efforts in the same direction.

*Its Anatomy.*—The following is the only case in which I have had any opportunity of accurate examination. A young lady had had from infancy a slight prominence of one eye, and at 13 it had so increased as to excite alarm. A firm elastic tumour could be felt above the outer canthus, projecting the eye, which saw well. The tumour was punctured, fluid let out, and the eye receded, but in two years the eye had become once more prominent, and had now assumed the state known as conical cornea, remaining otherwise healthy. In three months more the tumour had thrust the globe more forwards, everting the upper lid, and almost destroying sight, apparently by pressing upon and stretching the optic nerve. The conicity of the cornea was very considerable, and as is usual in such cases was accompanied by opacity at the irregular apex of the cone. It was now necessary to remove, if possible, the whole disease. In company with Professor Syme I first excised the globe, then, partly through the space obtained, and partly by an incision below the brow, I succeeded in completely removing a very large sebaceous cyst, with bony plates in its wall, which was adherent to the very bottom of the orbit. The removal of a V-shaped portion of the lid then corrected the ectropion. The recovery was most satisfactory, the young lady now wearing an artificial eye.

Mr. Hulke gave me the following account of an examination I requested him to make of the cornea:—

"*Note on the perfectly fresh cornea.*—The cone is a very prominent one, and is confined to the central region. The apex is nebulous, but elsewhere the cornea is quite transparent. Its surface is polished, and not facetted. The aqueous humour, iris, and lens are normal.

"*Microscopic examination.*—A section was carried through the line of the ora serrata, so as to remove the anterior ring of the sclerotic, with the ciliary processes, the iris and cornea entire; this portion was then pinned out flat on a piece of wood, with the iris downwards, and dried. Various sections were then made, and moistened with water, or acetic acid.

"The central conical nebulous portion was much thinner than the transparent periphery of the cornea, where the curve was natural. This thinning began at the base of the cone, and progressively increased towards the apex, where it reached its maximum. At this point the mean depth of several vertical sections was only one-third of that of the peripheral region. The continuity of the anterior elastic lamina was perfect, but upon the cone this structure was much thinner than elsewhere, and wrinkled; it was underlaid by a stratum of crowded, elongated, club-like nuclei, and beneath these the normal lamellar tissue was replaced by a web of caudate and nuclear fibres amongst the meshes of which clusters of large oval and fusiform cells were packed. The structure of the transparent peripheral region was perfectly normal, and at the base of the cone there was a gradual transition from the healthy to the diseased tissue, the interlamellar corpuscles becoming more plentiful, branched and drawn out into fibres, which in many instances coalesced with those from neighbouring corpuscles. The posterior elastic lamina and the epithelium, both on the front and on the back of the cornea, were unchanged.

"The changes I have described," adds Mr. Hulke, "were confined to the laminated tissue of the cornea and the anterior elastic lamina. The substitution of a web of nuclear fibres and cells for the regular lamination of the cornea, explains the nebulosity of the cone, and the liability to bulge."

The above account is quite conformable with what might have been anticipated from such an examination, and sets at rest doubts which may have existed as to the condition of the bulging tissue. It may remain a question in what degree the cell growth in the opaque part may be regarded as a cause, and how far as an effect, of the stretching.

*Contribution to the general history of conical cornea from my own observations.* (1.) I have observed a very few cases in which it occurred in more

than one member of the same family. The usual age at which it commences is from 15 to 25, but it may begin earlier, and as late as 50. (2.) It is very often, but not always, attended by signs of delicacy of constitution, such as an hereditary tendency to phthisis, over-rapid growth, a weak digestion, feeble circulation. (3.) It very commonly occurs in both eyes, sometimes simultaneously and equally, more often unequally, so that vision is worse in one eye. I have seen several cases in which it has affected only one eye, but in these it has with very few exceptions remained slight. One man of 60 had an extreme degree of it in one eye, none in the other. (4.) It may become stationary at any degree, from such a slight bulge as can be only detected by the ophthalmoscope up to an extreme irregular bulge, and opacity destroying the sight of all but the largest objects. (5.) It never advances to bursting either by extreme stretching or by ulceration or sloughing. (6.) It always proceeds gradually but with variable speed: when come to a stand it sometimes, though rarely, may take a fresh start. (7.) When the tendency exists, it seems to be aggravated by whatever strains the eye; consequently over-use and everything inducive of congestion is to be avoided; local fulness is to be combated by the usual remedies, while the general strength is to be upheld by all suitable means. (8.) In many cases, especially those of a slight and slow progress, there is no mark of weakness or excitement in the circulation of the eye; in others the ciliary vessels are overcharged, often perhaps from imprudent efforts to use the organs when sight is failing. (9.) Soon after the immortal invention of Helmholtz, I found the ophthalmoscope very useful in detecting slight degrees of conical cornea. For this purpose the concave mirror only is to be used without a convex lens. On turning the mirror so as to throw light at different angles, the side of the cone opposite to the light is darkened. Probably every one is now familiar with this test. Professor Donders told me last year that he had himself observed it. It is a very useful one in discriminating the cause of slight defects of vision, somewhat resembling myopia and hitherto deemed anomalous. I have seen many such cases. (10.) As to the physical state of the eye, I have observed that there is no undue tension of the globe appreciable by the touch; in some instances I have even thought the tension rather less than natural. In these examinations I have endeavoured to distinguish between the tension of the vitreous and aqueous regions of the eye, as I conceive it is desirable to do in reference to all the diseases in which the ocular tension comes under consideration. (11.) The pupil is apt to be large, and sometimes the iris inclines to be tremulous.

*Rationale of the Disease.*—It seems certain that the bulging of the cornea is due not to an increase of the normal intra-ocular pressure, but to a diminution of the resistance of the cornea itself. In the natural condition, the cornea is sufficiently strong to maintain its true curvature under the ordinary tension; and even when this tension is augmented, and continues so for a long period, the cornea does not bulge, unless it has previously undergone visible change of structure, and rarely even then. The ciliary or posterior regions of the sclerotica bulge rather than the cornea. I conclude this softening of the cornea to occur only in the bulging part, and not near the margin. At first the softening is not attended with such alteration of the anatomical elements as to produce opacity; but as the bulging increases this alteration does take place; either as a further stage of the same process of softening, or as a consequence of the stretching of the softened part.

It has often seemed to me remarkable that the bulging never goes on to bursting. The reason, probably, is, that as the cornea becomes thinner, the escape of the aqueous humor by exosmose is facilitated; and thus the internal pressure is reduced so as to be no longer in excess of the diminished resisting power of the cornea. A balance is re-established like that of health, only that there is a more than ordinary outflow of the aqueous humor by transudation through the cornea. This accords with my previous observation, as to such eyes being rather unduly soft. I do not pretend to explain why the cornea should thus occasionally tend to become less resisting and tough in its central region. Is it the consequence of some undefined change

in the chemical constitution of the lamellated tissue? I can hardly venture to assign it with any plausibility to a primary alteration in either the anterior or posterior elastic lamina. Certainly the membrane of Descemet is not strong enough to bear any important part in the production of this disease; and as for the anterior membrane, I have many times extensively removed it with even some of the lamellated tissue without observing the slightest after-tendency to conical bulging of the cornea.

*Treatment.*—No reliable testimony exists as to the success of any measures hitherto adopted for the restoration of the cornea to its natural curve, though, possibly, it would be wrong to say that that is not an object to be aimed at in slight and early stages.

*Indications for the treatment by arrest.*—These are two, to check the softening process, and to diminish the intra-ocular pressure. As to the former, in the imperfect state of our knowledge, we can only pursue measures of a tonic kind, which, indeed are very commonly indicated by the general symptoms. When there are signs of congestion it is most important to meet them by the cold douche, by very moderate and cautious local abstraction of blood in certain cases, by abstinence from all that kind of use and exposure of the eyes which excites and reddens them, and from shedding tears.

My friend, Professor v. Graefe, has, within the last three or four years, discovered the important fact that the excision of a portion of iris diminishes the intra-ocular pressure, a fact of pregnant interest in many diseases of the eye, and which he has applied with consummate skill to the treatment of glaucoma, of certain staphylomata, and last autumn to that of conical cornea. While agreeing that when this latter disease has attained a certain degree, and a certain age, all medical treatment is useless, he gives a case in which he practised iridectomy with the double object of favourably modifying the position of the pupil, and of lessening intra-ocular pressure, and he thinks with good results in both respects. (Archiv. fur Ophthal., vol. iv., part ii., p. 271.) The history of operations undertaken in cases of conical cornea is interesting; but up to this time none has come into general use. Mr. Wardrop repeatedly tapped the aqueous chamber. Sir W. Adams removed the lens by solution. Mr. Tyrrell drew the pupil outwards and downwards with the blunt hook, excising the prolapsed portion of the iris. If the iris then remained in the puncture, the pupil must have been balloon-shaped, and probably useful; but if it receded—an equal chance—the pupil would be a large irregular one, partly opposite the margin, and partly behind the central bulge of the cornea, and probably of little value. This operation has been the only one yet attended with any good result. Mr. Tyrrell himself tried it in several instances with advantage; in two with great improvement of sight. His great authority led surgeons to repeat his suggestion, but with variable and conflicting results, and I believe the proceeding has never come into common use with any surgeon. One eye was lost by destructive inflammation. In two the confusion of vision was aggravated. Mr. Walton has several times performed it without the slightest benefit. Mr. Wilde modified it by refraining from the excision of the prolapsed iris, and with marked advantage. Mr. Cooper and others had also variable, and on the whole, unsatisfactory results. (See a good *resumé* in the American edition of Lawrence on the Eye, by Dr. Hays, p. 393.) At present, I believe it may be safely said that operative proceedings in conical cornea have been generally abandoned as hazardous and uncertain. Some patients have been unwilling to submit to an operation when the surgeon could not promise a definite advantage, and the surgeon himself has often preferred the less formidable contrivances devised by the optician which have been frequently found of great use. In fact, at Moorfields, I hardly remember that any operation for the relief of conical cornea had been practised of late years until it occurred to me in 1857 to attempt to modify the pupil by a new method.

It has long been known that an eye thus affected often sees much better *when* either a small hole or a narrow slit in an opaque diaphragm is placed *immediately* before the eye. This is owing to the interception of all but a

narrow pencil or plane of rays, which, passing either laterally or centrally, form a more or less true image, unconfused by admixture with others less accurately converged, entering through neighbouring and more distorted portions of the corneal surface.

Such a contrivance has to be applied with a certain amount of skill, and to be moved as the eye itself moves, and it is seldom useful except for very near vision, because the area of the field is so contracted. To be useful, the aperture must be brought very near the cornea, and on the whole, though often far better than nothing, it is ill-adapted to poor and uninstructed persons. The idea on which I proceeded was so to operate on the iris as to give the pupil a slit-like shape, and to fasten it to the cornea in such a position as to conduce to the formation of the most correct image compatible with the altered curve of the cornea in each particular case.

It seemed likely that if the slit-like aperture could be placed within the eye, immediately behind the cornea, it would be more useful than the artificial perforated diaphragm placed outside. Without confining myself, however, to this idea, I have been endeavouring for the last two years to discover experimentally what modification of the pupil artificially produced was most effectual in improving vision. In no case have I had reason to regret having operated; no case has been made worse; all, with one exception, have been improved, and none has caused anxiety by inflammatory threatenings consequent on the operation.

The first case I shall relate is that of Sarah Collins, aged 26, who for eight years had been delicate, and for six years had suffered from conical cornea, both being equally prominent, and both transparent. With this exception, the eyes were quite natural, she could with an effort read the smallest type for a few moments if held very close, but had been compelled to abandon her employment on account of the impossibility of seeing the objects around her.

On April 30th, 1858, I operated on each eye in a different manner, but with the same object, viz., that of drawing the pupil outwards to a fixed point at the margin of the cornea, as the first step towards forming it into an horizontal slit. On the left eye I excised a minute marginal portion of the cornea by the help of a broad needle (*cornectomy*—the operation of Guepin modified) a small portion of iris was immediately protruded and the pupil became transversely balloon-shaped, the margin of the pupil not being involved in the hernia. On the right eye I performed the operation of *tying the iris*, then just devised by my friend Mr. Critchett, to meet the want we had long felt of a sure method of fixing the iris to a point of puncture in cases of artificial pupil (see Ophthalmic Hospital Reports, vol. i, p. 225, Oct. 1858.) In both eyes the canula forceps was used to seize and draw out the iris to the degree desired, the point seized being about one-third from the pupillary margin, so as to leave the margin still free within the aqueous chamber. The object in each eye was attained, but with so much less irritation from the iriddesis that in all subsequent operations I had recourse to this method, which I regard as one of the most useful improvements made of late years in the operative surgery of the eye, giving as it does precision and certainty, with perfect security under all ordinary circumstances, to the operation for artificial pupil, in the very numerous cases to which it is applicable. The sight being now much improved, but most so in right, I operated inwards, or at an exactly opposite point, in both eyes, on the 21st of May, the immediate result being a transversely oval pupil in each. In the right eye the margin of the pupil was entirely free; in the left, the margin was adherent to the site of the corneal wound. Hardly any irritation ensued. At the end of two days the ligatures had dropped off; the pupils acted promptly, the left being more slit-like than the right. In both eyes vision was immediately much improved, and a gradual further amendment has since gone on, apparently owing to a certain receding of the corneal bulge.

This girl has become able to resume her occupation of a housemaid, and is able even without concave glasses to see the objects in a room with tolerable accuracy; with a glass she can see distant objects still better.

I have operated since on six other patients, on five of them in both eyes. I have operated so as to make the pupil vertical rather than horizontal in all these cases, and in several have experimentally modified the operation in regard to the point of the iris seized by the canula forceps, and so as to test the value of that instrument, compared with the short blunt hook for seizing the pupillary edge. This last I now prefer, as being simpler and less apt to injure the iris, while it affords greater precision in fixing the pupillary edge to the wound. I find, too, no disadvantage in engaging the pupillary edge in the wound, the pupil acting as well afterwards as when the natural pupil is still entirely within the chamber. I have tried to perform iriddesis in the opposite directions on the same eye on the same occasion, but found that the point first tied was apt to be drawn unduly into the chamber while the opposite point was being drawn out, so that I think it better to do the second iriddesis on the same eye a week after the first.

In one of the earlier operations with the canula forceps there was a little bleeding from the stretched iris into the chamber, sufficient to disguise the later steps of the operation, but with the short hook this has never occurred. These operations from their extreme delicacy should be done under chloroform, the influence of which should be sufficiently complete to place the organ in complete repose; one minute is sufficient for each eye when dextrously managed. If sickness ensue it does no harm to the eye. No iritis suffusing the pupil has followed in any case, but of course the usual care has been taken to keep the eye cool and quiet for a few days after the operation. If the thread remains on after three or four days, it may be pulled away with forceps. It is of great importance to the perfection of the operation that the puncturing instrument and the blunt hook should be of equal size, so that a sufficient opening, but not more than sufficient, is made for the free passage of the hook; otherwise, either the hook does not enter easily, or, on the other hand, the knot is prone to slip back into the puncture, or even into the aqueous chamber. The recurved part of the hook should be long enough to hold the pupillary margin, but so short as to admit of the ligature being tightened the moment this margin is outside the puncture. I always leave one end of the silk nearly an eighth of an inch long, so that it may be readily laid hold of afterwards, should it chance to have fallen at all inwards, or be concealed by a coating of lymph. These may seem trifling points, but in my opinion have their importance, and are not unworthy the attention of any one performing this operation for the first time—they pertain to the excessive delicacy of the structures, and of the whole proceeding.

It has been a matter of great interest with me to ascertain, as far as the limited number of cases presenting themselves enabled me, how far the second iriddesis was useful, in other words, whether an elliptical or slit-like pupil gave better sight than one of a balloon-shape; and again, whether the vertical direction was better than the horizontal, or the reverse; also whether other modifications in the direction of the altered pupil were desirable.

Of course, this is a subject which must be worked out in detail by surgeons, and the following remarks are but hints for future experimental enquiries.

The slit-like figure of the pupil suggested itself to me as the most feasible method of much limiting the size of the pupil while changing its situation. If the thing be well considered it will be found difficult to conceive any plan of rendering the pupil *very small* by operation; it is much more easy to *enlarge* it. While the pupillary margin is free, any displacement of the iris is most likely to enlarge the pupil, for I despaired of being able to seize the iris at one side of the pupil and draw and fix it over towards the other side. Any excision of the iris must, of course, enlarge the pupil, and so must any marginal iriddesis; but by a double iriddesis in opposite directions the central region becomes slit-like. By making this slit *horizontal* the light is admitted from each side, as well as through the centre, and the range of the field would probably be expanded, while the precision of the image would be impaired by the inequality of refraction through the central and marginal regions respectively. The appearance of the horizontal pupil was besides

not agreeable. The *vertical* position of the slit offered the prospect of its virtual reduction in size, by the overlapping of its angles by the lids in ordinary vision; and I hoped that the patient would learn to use the lids for this purpose, so as to clarify the image. I cannot say that this expectation has been borne out in any marked degree, but nevertheless the vertical slit is much more sightly than the horizontal, and certainly equally good for vision, so that at present I prefer it.

The improvement of vision from a first iriddesis downwards has been in almost every case decided, the patients being delighted with the result. In some the second iriddesis upwards has not seemed further to increase the precision of view, in others it has certainly done so, and in the present state of the enquiry, I am disposed to continue to practise it in cases of considerable conicity. The improvement, however, consequent on the second operation, is never so marked as that which follows the first, and it can only operate by narrowing that part of the pupil which lies behind the bulge. Its more or less influence in different cases may perhaps depend on varieties in the curvature of the apex of the cone.

*The influence of these operations in lessening the corneal bulge* has been very remarkable. It is not easy to give in any case the exact amount of this result, but that the bulge diminishes speedily, and continues to do so for a considerable time subsequent to the operation, admits of no doubt, and I attribute it to the moderation of the ocular tension. Further experience will show whether I am right, but at present I am for operating quite early in slight cases in a downward direction only, if only to arrest the progress of the conicity; and certainly to obtain this result in almost any degree will make it worth while to perform so safe an operation in a disease otherwise so intractable, and, in its advance, so destructive of all useful sight.—*Ophthalmic Hospital Reports, Oct.* 1859, *p.* 154.

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## 85.—A NEW AND EFFECTUAL CURE FOR ENTROPION AND TRICHIASIS.

By Dr. JOHN WILLIAMS, Surgeon to the Cork Eye Infirmary.

Few diseases of the eye prove more distressing to the patient or more trying to the surgeon than those in which the lashes are inverted or misdirected.

The friction of the inverted ciliæ keeps up constant irritation of the eyeball, and unless the disease be interfered with by operation, the cornea becomes clouded, thinned in its structure, and finally gives way; total blindness or considerable impairment of vision resulting.

With a view to obviate such effects, many operations have been proposed by Crampton, Jæger of Vienna, Jacob, Wilde, and others, which it is not my present intention to refer to; but, unless the bulbs of the lashes be *completely excised*, it will almost invariably be found that any other operation affords but temporary relief, and the surgeon will, after a time, have inverted ciliæ again to contend with.

Cork has gained an unenviable notoriety for inverted lashes and granular lids, and my experience proves that this reputation is unhappily too well deserved; the number of those cases being about sixty per cent.

These scrofulous diseases are almost exclusively confined to the very poorest classes in Cork, among whom scrofula prevails to a great extent, and I may remark in passing, that the non-recognition of granular lids as a *scrofulous* affection—which I believe it to be—and the dependence on local applications alone, to the exclusion of constitutional remedies, in its treatment, have added considerably to the difficulties of cure.

The plan of treatment which I now beg to bring before the notice of the profession is, although simple, perfectly efficacious, and is of great value in those cases in which we would desire to save, if possible, the natural ciliæ, and destroy those only which, by their inversion or misdirection, were pro-

ducing on the eyeball the ill effects before referred to. These cases are :—

1st. Where a whole row of supernumerary lashes lie interior to the natural row of ciliæ.

2ndly. In those cases in which a few only—perhaps a single hair—of the *natural* lashes are inverted; the others maintaining their normal direction.

3rdly. Where a few lashes, either isolated or in tufts, are scattered along the tarsal border.

Another great advantage my plan of treatment affords is the preservation of, in most cases, the meibomian glands; for, as Mackenzie remarks, *lippitudo* follows the operation in which the tarsal border, including ciliæ and meibomian glands, is clipped off.

I will now proceed to describe the treatment I practice.

Having placed a small piece of potassa fusa in a porcelain capsule or bottle, and adding a drop of water to hasten its deliquescence, I dip the point of a fine needle—or a small knife, if the inverted lashes be in tufts—in the *liquid caustic potash*, and then plunge it into the tarsal margin along the course of the hair or tufts of hair, to the depth of the eighth of an inch, at which depth the hair bulbs lie (I would recommend a Desmarres ring-forceps or horn spatula to be placed under the lid for the protection of the globe and better performance of the operation). The pain attendant on the puncture is trifling, and may be instantly removed by applying cold water with a small brush to the border of the lid.

On the second or third day the lashes thus treated are removed with forceps; they are pulled out without any difficulty, and are blackened towards the roots. The bulbs of ciliæ are completely destroyed, never to be reproduced. Unless an entire row of lashes is to be removed, no inflammation follows the operation; even in this case it is of little moment.

For the removal of nævi, fungous growths, small tumours and *warts* in *any* locality, punctures with a needle or small knife dipped in the liquid caustic potash will be found useful, as the needle or knife burns along its subcutaneous track. In the above remarks, I have made no allusion to the amount of deformity left after the loss of the eye-lashes, as I consider it of no moment when the preservation of vision is effected.—*Dublin Hospital Gazette, Jan.* 15, 1861, *p.* 18.

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## 86.—ON RECOVERY OF THE LOWER LACRYMAL PUNCTUM BY A NEW OPERATION.

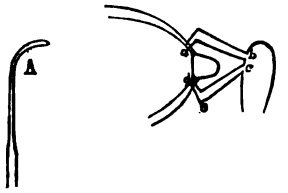
By J. F. Streatfeild, Esq., Editor of the Ophthalmic Hospital Reports.

[The case was one of very old standing tinea tarsi, in which, as usual, the lashes were nearly all destroyed. The lower punctum of the right eye was everted and lost in the thin tense skin of the lid. By appropriate treatment, the condition of parts became much improved, but the tears constantly overflowed the lids, and the first indication became to reinstate the lower tear duct.]

According to the anatomy of the parts I thought I might be able to pass a probe from the upper canaliculus into the lower one, the upper canaliculus was already slit up, and this would facilitate the use of the probe. I did not think that the lower canaliculus was likely to be obstructed or diseased, although the punctum was lost. The course I proposed to take with the probe was all by soft parts, which would readily yield, and if I could keep the probe in the right course throughout, and avoid entangling its point in any fold of mucous membrane, I thought I might be able either to pass the probe's point out at the place of the lower punctum, or at any rate, by feeling with the finger, detect the probe's point, and cut down upon it through the mucous membrane. I did not believe in the valves which have been supposed to exist at the extremities of the canaliculi towards the lacrymal sac, and I knew that exploratory operations (incisions made by the conjunctiva, &c.), for the discovery of the lost passage had not been successful.

On the 22nd, when she came again, with the eyelids of the two eyes improved, but unequally, I took one of Mr. Bowman's canaliculus styles, and bent the end of the thin part into a small curve (A. in the figure), and just the point of the part curved (turned towards me) bent a very little to my right hand. I chose this canaliculus style instead of a probe, because I was sure the thin part of it would be long enough for my purpose, and the thick part I thought would be more convenient to be held in the fingers and more easily directed than a very fine probe such as I should (hoping to be able to pass it out through the lost punctum) have been obliged to select for the attempt.

I took the thick part of my instrument, and holding it downwards close to the patient's cheek in a line with the nose, passed the point of the bent thin part into the upper canaliculus, passed it along it without moving the position of the handle, and pressed it inwards until I was sure the point must be in the sac; then I brought the handle round towards the temple, making the point of the instrument keep, as well as I could judge, in the direction of the outlet of the lower canaliculus by an abrupt turn, according to the anatomical relation of the parts (*a*, *b*, *c*, *d*, in the figure), to reverse the direction of the point (carefully avoiding any force, and, when the point seemed to be caught, moving it about a little), then the handle being brought over the brow of the patient, I gently pressed the point along the lower canaliculus, which with very little difficulty I found I had entered. I could feel the point of the instrument distinctly through the conjunctiva, but could not bring it out through the punctum at first, and thought of cutting down on the point, but by bringing the handle more across the root of the nose between the eyebrows, and manipulating a little with a finger the skin of the lower lid, the point showed at the punctum, and with the pressure of a finger it emerged; then I simply divided the lower canaliculus to some extent, upon the probe, withdrew it, and ended in the regular course, by passing in at the opening I had made, along the remains of the canaliculus, and down the nasal duct, No. 6 (the largest) probe.




A. the instrument. *a*, *b*, *c*, *e*, *d*, the direction it was made to take.

On the 29th, I found the patient's state rapidly improving. The escape of the tears was perfect, the sore edges of the lids were nearly healed, the natural appearance of the skin was restored and the ectropion was consequently almost cured. Her appearance and the state of the parts was, soon after this, as much improved as they could be, for her eye-lashes were at first destroyed.

On November 5th, the lippitudo was less conspicuous, and so she was doing well. This day I practised again with the bent style, the "tour" I made on the 22nd, this I did in order to learn how it should best be done. I was surprised to find that, as she sat in a chair, I could not do it at all, this I could only think was on account of the twitching of the eyelids, which she could not control (the orbicularis and tensor being in constant movement whilst I used the instrument), and it appears to have been so, for when she was put under the influence of chloroform (which on the former occasion she had taken) I, with very little difficulty, passed the bent style from the upper punctum and canaliculus, by the sac, through the lower canaliculus and punctum. In passing the bent style I found it was best to press the convexity of the curve rather upwards in traversing the upper, and downwards in the lower canal, thus more probably keeping the point of the style free from entanglement. The knowledge of when to make the sharp turn to (avoid the nasal duct and) enter the lower canaliculus, I gained by passing the point inwards towards the nose till I felt the bone, then drawing the point a little back, I rotated it as on this point, and directed it first down and outwards, and then more outwards. I never used any force with the point of the instrument, but when the bent style has been in the way here de-

scribed passed into the lower canaliculus, of course a fold of skin of the upper lid rests against the handle of the instrument.

In order to learn that I was able, if in any case in future it might be desirable, I afterwards on this occasion, passed my bent style once in the contrary direction to that I had been practising (also with little difficulty) that is to say from the *lower* canaliculus, by the sac, and out at the upper punctum.—*Ophthalmic Hospital Reports, No.* 12, *p.* 5.

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87.—*Case of Injury to the Eyes.* By F. Le Gros Clark, Esq., Surgeon to St. Thomas's Hospital.—[The following case occurs in a lecture by Mr. Clark, and is interesting from the comparatively trifling ill consequently resulting from so severe an accident.]

J. B., aged 30, an engineer, was admitted into the hospital on September 24, 1860. The patient was engaged in executing some repairs to a steamer, and was using some melted metal—a patent mixed metal, which melted at a temperature of about 80° above the melting point of lead. The accident occurred by the metal splashing, and a quantity of it was thrown up into his eyes. When brought to the hospital, both his eyes were found to be filled with fragments of a metal resembling lead, diffused over, and adhering to, the conjunctiva and cornea, as well as to the eyelids. The eyes were cleansed, as far as practicable, by means of a probe and camel-hair pencil; and, oil being dropped into them, the patient was removed to bed. Some shreds of disorganized membrane were also removed with the fragments of metal. A purge was ordered by the house-surgeon, and six leeches to each temple. Subsequently, several more fragments of metal were removed. On the following day he complained of acute pain, and the vessels of the conjunctiva were injected. On the third day a careful examination of the interior showed the ocular conjunctiva to be clear and free from ulceration, and only slightly injected; but the outside of the lids, which had been burnt by the metal, exhibited patches of ulceration. In the course of the following week he left the hospital well.

I did not see this patient when he was admitted, and was surprised to find so limited an amount of injury, after hearing the history of the case, and ascertaining the temperature of the metal. One of you remarked to me that you had seen workmen plunge their hands, when recently wetted, into molten metal with impunity; and the explanation suggested at the time is that the sudden conversion of the water into steam protects the hand from injury. Such may be the explanation in this and similar cases, where certainly we should anticipate more serious mischief, and the eye escapes almost uninjured. In comparing such an accident as I have described, with the effects of the contact of unslaked lime or mineral acids with the conjunctiva or cornea, the contrast is very striking. In the latter case the destruction is immediate, and the opacity of the cornea permanent and irremediable; a point of considerable importance to be borne in mind in the prognosis you give.—*Med. Times and Gazette, Nov.* 10, 1860, *p.* 488.

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## 88.—ETHER AS A REMEDY FOR DEAFNESS.

The attention of the public, both medical and non-medical, has been of late attracted by a fact which, though in its origin and early phases going back some years, is of interest through the publicity given to it by the official journal of the University of Paris, as well as through the daily press.

To state the question briefly:—About the month of August, 1855, a certain Mademoiselle Cléret, a private governess, inhabiting a populous part of Paris, applied to the Minister of Public Instruction for assistance, basing her application, among other grounds, upon her knowledge of a method of causing the deaf and dumb to hear. This method, the discovery of which was accidental, and of which she had made a successful trial upon some pu-

pils suffering from deafness, after having experienced its efficacy in her own person, consists in the use of sulphuric ether dropped direct into the external auditory canal, at the rate of four, five, six, or eight drops per day. After the application of the remedy for fifteen or twenty days, its use may be suspended some days, and then renewed: it may be continued, if not indefinitely, at least for a very lengthened period.

A commission appointed by the minister, and of which the medical element included M. Lélut as president, M. Béhier as secretary, and the late M. Bérard, was deputed to examine into the state of the children submitted to it by Mademoiselle Cléret. The commission pursued its investigation with the utmost diligence, until upon a sudden the lady was seized with a fearful malady. After having waited, without much hope, an improvement in the mental condition of Mademoiselle Cléret, the commission drew up its report, although the question, necessarily suspended, did not appear susceptible of being brought to a definite conclusion, or to a complete and demonstrative result. It considered, however, that it was its duty to make known such facts as it had witnessed: this it did in the following terms:—

Twenty-nine children have been treated by this lady; all with beneficial results. Two of those brought by her before the commission, and who had been treated by her previously, were completely cured. Seven children have been submitted to the commission previous to any trials being made upon them, and their absolute deafness and dumbness demonstrated by Mademoiselle Cléret; and in all cases, but especially in four, a manifest change has been perceptible after eight or nine months of treatment, and the patients have been able to recognize with great ease various sounds. The reporter to the commission has been careful to add that the most minute precautions were taken to avoid sources of error, and to guard against any illusion that might arise from perception derived through another sense than that of hearing.

The commission wishing to test the means used by Mademoiselle Cléret upon other cases than those exclusively under her own care, deputed one of their number to carry out her plan of treatment upon other patients. Twenty persons were accordingly entrusted to him; most of them were deaf-mute children, but there were also some old men whose hearing was impaired, in some cases upon one side only. In all these a very noticeable effect was produced. It was also found that patients whose sense of hearing had become impaired through typhoid fever were very speedily restored by the same treatment.

In conclusion, the commission state that, with the exception of two or three children, whose deafness and dumbness was attested by authentic certificates, and who now hear well, it has determined nothing but the incomplete results of experiments commenced but not terminated, which manifest improvement, but nothing more definite.—*Gazette des Hôpitaux.—North American Med.-Chir. Review, Nov.* 1860, *p.* 1101.

# MIDWIFERY,

## AND THE DISEASES OF WOMEN, ETC.

### 89.—ON RIGIDITY AND DILATATION OF THE OS UTERI IN LABOUR.

By Dr. CHARLES D. ARNOTT, Gorleston, Great Yarmouth.

[Dr. Arnott gives us the following remarks on this subject. We will first let him explain himself in his own words, and then offer a few comments.]

The following brief outline may serve as an illustration of a case often met with in practice. A patient is in labour with her first child, and has been so many hours. The pains have been regular, have gradually increased in severity, and have now reached such intensity, that the nurse and friends are assured the necessary assistance only is needed to effect speedy delivery. Upon arrival, the medical attendant can but be similarly impressed; the pains recur regularly, are strong, and present all the characters of the genuine expulsive efforts, the last in the train. Digital examination reveals the following conditions:—Vaginal passage dry and tight; os uteri barely within reach, high up posteriorly, a mere slit like a button-hole, scarcely admitting the finger's point; the fœtal head low down anteriorly, pushing before it a large segment of uterine tissue, and under the influence of each pain apparently making ineffectual efforts to force its way through this evident source of obstruction. Under these circumstances, inexpressibly unhappy, the labour, without adequate assistance, must inevitably persist for hours, perhaps even for days.

This is a correct delineation of those cases which are described as depending upon rigidity of the os uteri; but, as I apprehend, improperly so. In the great majority, the os is, in reality, dilatable enough, but some of the chief elements of dilatation are acting disadvantageously, or not at all. The fluid wedge, for instance, is generally absent, the membranes having ruptured early under the influence of the strong uterine contractions, and the fœtal head is expending all its force in a wrong direction, not upon the os uteri, as occurs under normal conditions, but upon that shelf of uterine texture it has infringed upon, and which so long must successfully resist it.

The treatment of these cases the teaching of the schools has hitherto inculcated, I am now fully convinced, is not only inert, but highly injudicious. Bloodletting and antimonial depressants (time-honoured maids-of-all-work) have been loudly extolled as adjuvants of vital dilatability, and almost implicitly relied on. Anodynes (learnedly) to lull excessive irritability, (truthfully) a clumsy excuse to gain time, have also had their share of renown, and these again are now likely to be superseded by a more potent agent, chloroform.

I shall not discuss in full the merits of these plans of treatment. I am, from repeated experience, as fully convinced as I ever can be of any one professional fact, not only that they are of no use whatever, but that they are positively and highly injurious. My opinion is confirmed, that the effectual treatment of these cases is by surgery alone: reduction and dilatation of the abnormally placed os by manipulation, and in the more aggravated and resisting cases, incision of the opposing texture. Much good service may, indeed, thus be rendered, a great amount of maternal suffering saved, and infantile life preserved.

Free lubrication of the parts having been premised, the finger is to be introduced during each pain, and the os solicited downwards and forwards, and

at the same time freely dilated without any unjustifiable force. As with the prostate after the deep incision in lithotomy, so with the os in the great majority of these cases, it will be found to yield readily. Improvement commences forthwith, and all difficulty is soon overcome. In the very rare minority, incision (mere notching with a guarded probe-pointed bistoury) may be requisite in addition to manipulation, and the most intractable cases are speedily made to assume a totally different and more promising aspect.

I am aware the proposal seems harsh and may be denounced by the *laisser aller* school as belonging to that confessedly bad category, "meddlesome" midwifery. I believe, however, it may be fully proved undeserving any such epithet, by the remembrance that there are cases which tax our resources to the highest extent, from the extreme anxiety generally pertaining to them, and the inefficacy of the means usually employed for their relief; and further justified by the great fact of its being merely a close imitation of nature's own *modus operandi* under these special circumstances.

Let us briefly inquire how this difficulty is ordinarily surmounted by nature's efforts when unaided? Almost always, as all observant practitioners have again and again experienced, by spontaneous laceration, sometimes rather extensive, of the resisting uterine tissues; and, in spite of bloodletting, antimonials, and all other such auxiliaries, as they are inaccurately termed, scarcely ever, until this is accomplished, can she complete the process. Instances in which very extensive rupture ensues, are, with this concomitant complication, not uncommon; and many so called cases of occlusion of the os uteri, in which incision is the only alternative, nature having failed in effecting the necessary laceration, are probably often little more than extreme cases of the description we are now considering, in which the os, being more out of reach than usual, escaped detection.

I have employed the practice now recommended for many years, and on many occasions I have satisfied myself of its invariable and great utility: moreover, having never observed one untoward sequence from its employment, I conclude it may be regarded almost entirely free from danger. Let it but have a fair, unprejudiced trial, and I feel assured it will so forcibly commend itself, as to assert its superiority over all other expedients.

[There is much truth in what Dr. Arnott here says, but we think that he has undervalued the power of both ipecacuanha, antimony, and chloroform. In a case of this description we quite agree with Dr. Arnott, that to introduce the finger into the os, and during each pain, or even in the intervals, gently to draw the anterior lip downwards and forwards so as to get the os as soon as we safely can into the *axis of the outlet* of the pelvis, instead of allowing it to remain in the axis of the *brim* of the pelvis, is sound practice; but this is wonderfully accelerated by placing the patient under the influence of chloroform. Instead of waiting for six or eight hours, which formerly we have been obliged to do, we can now get over this dilatation of the os by a little gentle dilatation with the finger, while the patient is very mildly influenced by chloroform, in a remarkably short time. In fact, by following this practice, we have been delighted often to get home at midnight, instead of next morning to breakfast, and this too with great benefit to the patient, and a great saving of misery. We would strongly recommend our friends to chloroform their patients in this way:—Get a small cup, put a little chloroform at the bottom, and let the nurse hold the edge of the cup under the lower lip of the patient, so as to let both mouth and nose be free. You may thus keep the patient gently affected for a long time, and you have no trouble about it. You are busy with the os uteri, and the nurse manages the chloroform, although you watch its effects carefully. Ipecacuanha and antimony will have a similar effect, but these are now banished from practice by the more pleasant and more efficacious adoption of chloroform.]—*Lancet, Nov.* 17, 1860, *p.* 485.

## 90.—ON THE OPINIONS OF WILLIAM HUNTER RESPECTING RETROVERSION OR RETROFLEXION OF THE GRAVID UTERUS.

By Dr. W. Tyler Smith, Physician-Accoucheur to St. Mary's Hospital.

[In an interesting paper on this subject read before the London Obstetrical Society,]

After giving an account of the way in which our knowledge of this displacement of the uterus has been acquired, and the opinions of ancient and modern authors, but particularly of William Hunter upon this subject, Dr. Tyler Smith proceeded to lay down his own views, and especially to dispute the Hunterian doctrine that the chief and exciting cause of complete retroversion is retention of urine and distension of the bladder. He then went on to say: "My own attention became specially directed to the subject of retroversion of the gravid uterus in the following manner:—I attended a lady, a patient of Sir Ranald Martin, who, in the unimpregnated state, suffered from complete retroversion or retroflexion. She left this country, with the uterus retroverted, to join her husband in India. She soon became pregnant, and went the full time. The question suggested itself to me, What was the condition of the uterus in this case, after impregnation occurred? And I resolved to take any opportunities which might occur to me of answering it. I have now seen a considerable number of cases in which the retroverted uterus has become impregnated, and have carefully watched the progress of gestation under these circumstances. The result has been a conviction that the most common cause of retroversion of the gravid uterus is not to be found in the state of the pelvis, or the condition of the bladder, but in the occurrence of impregnation in the retroverted uterus, and in the tendency of the organ thus impregnated to grow and develop itself during the early months of pregnancy, in the retroverted or retroflexed position. When an ovum is deposited in the retroverted uterus, the enlargement of the organ causes a greater sense of weight and pressure in the pelvis than ordinary pregnancy. The os uteri approaches the pubis, and the fundus projects towards the hollow of the sacrum. The fundus is found to enlarge considerably when examined from time to time by the finger. At length, unless the pelvis is of very large size, the bladder and rectum are pressed upon so as to interfere with their functions, and difficult micturition and defecation, especially the former, are the results. Owing to the retention of the gravid uterus within the pelvis, there is little or no increase in the size of the abdomen. There is usually a great amount of pain and discomfort in the lower part of the back, and the sympathetic affections of pregnancy are frequently more severe than usual. Abortion very frequently occurs from the mechanical irritation of the uterus." He had published an outline of these views in the Lancet in 1856.

After making some further general observations to prove the strength of his argument, Dr. Tyler Smith proceeded to say,—"In conclusion, I may observe that it seems to me the great use of the knowledge of the mode in which retroversion of the gravid uterus occurs will be in the prevention of the full retroversion, or strangulation, as I have ventured to term it, of the gravid uterus in the pelvis. As long as retroversion was supposed to take place suddenly and mysteriously, little could be done to avert it; but if, as I believe, the displacement dates from the very beginning of pregnancy, in the great majority of cases, we may do much by position, and attention to the bowels and bladder, to prevent any dangerous symptoms; and, aware of the condition of the uterus beforehand, we shall be more ready to give prompt mechanical assistance when it becomes necessary to pass the hand into the vagina to carry the fundus above the brim.

When retroversion has existed in early pregnancy, but has been relieved spontaneously or otherwise by the ascent of the fundus, labour takes place without any unusual difficulty. We ought, however, in the management of the puerperal state, to endeavour to prevent a return of the uterine dis-

placement. The occurrence of pregnancy is rather favourable than otherwise to the cure of retroversion. In the latter months of pregnancy, the fœtus acts as an intra-uterine pessary; the organ is straightened, and in the return of the uterus to the size of the unimpregnated state by the process of involution, we have a better chance of curing retroversion than under any other circumstances. The abdominal bandage should not be tight enough to force the uterus into the pelvis. The patient should be encouraged to lie on her right or left side, inclining to the prone position, but avoiding recumbency. The bladder should be frequently relieved, and any violent straining during defecation avoided. She should remain in bed or on a couch longer than usual, and before resuming her ordinary duties the condition of the uterus should be ascertained; and if any tendency to a return of retroversion exist, an air-pessary should be worn in the vagina as long as may be necessary to ensure a right position to the uterus.

[We think that most writers on this subject are a little mistaken. It seems not to be sufficiently remembered that in nearly all women who have borne children, the womb has descended more or less from the position which it originally occupied in the unimpregnated state. It originally occupied the *axis of the brim*, *i. e.*, the os points downwards and backwards: as the parts become enlarged by pregnancy, it sometimes occupies the *axis of the outlet*, *i. e.*, the os points more or less downwards and forwards, in the first two or three months of pregnancy. But where is the fundus situated in this last position? Generally on the rectum more or less, in fact it is in a state of retroversion. We think that retroversion of this description is far more common than is generally acknowledged, and is constantly mistaken for the retroversion of Hunter and modern writers. If you introduce the finger and press it upwards at the back of the cervix, you will feel the womb like a tumour. If you introduce the finger of the left hand into the rectum, and the finger of the right hand into the vagina, you may compress the womb between the two fingers; but this is so common that we cannot look upon it as of much consequence, although we think that it is often called retroversion, and is even mistaken for hypertrophy of the posterior portion of the womb. Now, most of what Dr. Tyler Smith says in the above paper may more or less be explained by the views which we have just expressed about this common malposition, and we agree with Dr. Oldham, who in another part of this paper, which we have not abstracted, says, that these cases do not give much trouble if not meddled with too much.]—*Lancet*, *Nov.* 17, 1860, *p.* 489.

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## 91.—ON REDUCTION OF RETROVERSION OF THE UTERUS.

By Dr. Thomas Skinner, Physician to the Northern Dispensary, Liverpool.

[Dr. Skinner states that the following method of reduction is nearly painless, and most effectual in its results. He says:]

Let me premise that if there is any pain or difficulty likely to be experienced, the induction of anæsthesia will greatly facilitate matters; and whether the anal and vaginal orifices are rigid or not, the patient should previously sit over the steam of hot water, or be steamed in bed by the application of flannels, wrung out of boiling water, to the perineum. The preliminary steps having been taken, the bladder and bowels having been emptied, and the bearing-down efforts stopped, the patient should be placed upon her left side, with the pelvis well raised, the shoulders low, and the thighs as much flexed upon the abdomen as possible. If the cervix is bent upon itself, or if it is resting upon the arch of the pubes, it should be gently yet steadily pressed backwards and towards the left acetabulum or the left ileo-pectineal ridge, so that it may correspond as much as possible to one extremity of the right oblique diameter of the brim. Then insinuate the fore and middle fingers of the right hand, well lubricated with oil or lard, into the rectum, directing them towards the left side of the displaced uterus and of the pelvic cavity; exert a well-sustained upward pressure with them

in the direction of the space between the right sacro-iliac synchondrosis and the pectineal eminence, and the tumour will most likely pass slowly from the true pelvis into the abdomen. If the patient is not under the influence of chloroform, the pressure of the taxis should only be exerted during expiration, which ought to be prolonged by the patient. At one time I thought it was necessary to introduce a ball-pessary, or some such support, into the vagina, in order to prevent a recurrence of the retroversion; but I am now quite convinced that if the reduction is complete, and it is done subsequent to the second month, any kind of artificial support is much more likely to do harm than good. The patient should simply be ordered to take a little more than usual rest, to attend to keeping the bowels open with cold enemata once a day or every other day, and to empty the bladder without straining whenever she feels the slightest inclination to do so.

If the above method of applying the taxis were to fail, I would not be disposed to invert the patient by placing her resting on her thighs and knees in the bed, and on her elbows or hands on the floor, as has been frequently attempted with indifferent success by many; the attitude is painfully tiresome to the patient, most repulsive to her feelings of delicacy, and besides there is nothing to be gained by the position, as that which I have described secures all that can be desired—rest, comfort, freedom from unnecessary exposure, relaxation of the abdominal and femoral muscles, and the aid of gravitation.

The following method is so simple and ingenious, so safe, and easy of application, that I shall first allude to it. I mean the inflation of the vagina with air, as practised by Mr. Halpin, of Caven, in 1839, and first made public by him in the Dublin Journal for 1840. Mr. Halpin fixed a recent bladder to the tube of a stomach-pump fitted with an air-tight piston, (there is nothing better suited to the purpose than the caoutchouc barrel syringe of Mr. Higginson of this town) and introduced the bladder empty into the vagina. Mr. Halpin retained it there by making a firm counter-pressure with his hand over the vulvar orifice, whilst his friend Dr. Finlay of Belturbet forced air into the bladder by slow degrees, until the fundus was reduced, and the os uteri was felt in its proper place; thereby affording additional proof of the view I advocate, that the cervix ought to be second in the order of reduction, and that it will right itself if the fundus alone is attended to. Although some of us may be disposed to question the practicability of such a method, it is no less a great fact that Mr. Halpin, assisted by his friend Dr. Finlay, did succeed in reducing with comparatively little pain and with no difficulty, a retroverted gravid uterus at the fourth month, which had previously resisted on various occasions, the use of aperients, hip-baths, leeches, catheterism, and the most approved modes of applying the taxis.

In September 1855, unaware of Mr. Halpin's method by inflation, I adopted the following plan. The case was the same as that in which I used the sponge tents in order to gain the *meatus urinarius*. I inserted pieces of compressed sponge into the vagina and rectum, placing them as much as possible towards the left side of the pelvic cavity, resting as it were upon the tuberosity of the left ischium and the greater and lesser ischiatic ligaments, in order to raise the fundus in the direction of the right sacro-iliac synchondrosis. Tepid water was injected, while fresh supplies of the compressed sponge were added as the fundus ascended, and every assistance was now and again given by a steady pressure with the fore and middle finger of the right hand; within half an hour the os was felt in its natural position. The patient was four months and one week advanced in pregnancy. As already stated, great difficulty was experienced in passing a catheter; the digital taxis, just described, was perseveringly tried in vain; ultimately, the compressed sponge and tepid water, combined with digital pressure, reduced the uterus with ease, and in safety both to the mother and fœtus, as the patient was delivered at the full time of a living infant.

As regards the preparation of the compressed sponge, any quanity of it can be got ready within an hour. Take one or two large common-place sponges; steep them in hot water, wring them, and roll or tie them close and

tight with whip-cord or tape, so as to express as much of the moisture as possible, and place them in an oven to dry. When perfectly dry, unwind the cord, cut the compressed sponge with a sharp knife to the form and size desired, dip the point or edge of insertion into a mixture of equal parts of bees' wax and lard or tallow in a state of fusion, and it is ready for use. By using compressed sponge and water as a mechanical lever, the pressure is equally, slowly, and steadily exerted on every point of the surface exposed to it, and on this and other accounts, it is much superior to passing the whole hand through a previously undilated vaginal or anal orifice.

Since reading the account of Mr. Halpin's excellent method by inflation, another plan has suggested itself to me; namely, to use hydraulic instead of pneumatic pressure, and two bladders, one for the vagina, and another for the rectum, instead of distending the vagina alone. For this purpose, the following arrangement is necessary: A metallic tube, three inches long, dividing into two branches, each about two inches in length, and Y-shaped; to each of the shorter extremities attach a recent sheep's bladder, and to the longer one, a short piece of India-rubber tubing, so that the whole may be attached to one of Higginson's or any other syringe; with a little tepid water, all is complete. (If any one is disposed to try bladders of caoutchouc, instead of ordinary bladders he will be disappointed, because when two India-rubber bladders are filled at once from the same conduit tube, if there is the slightest variation in the thickness or elasticity of the material, the strongest cannot fill until the weaker is full and bursts, which is not the case with bladders obtained from animals.) Hydraulic pressure is every way superior to inflation with air; by the above method, both the rectum and vagina being filled at once, the tightened recto-vaginal septum will in no way interfere with the distention of the entire pelvic cavity, and the vagina will be relieved of a portion of the necessary distention of its walls; besides, inflation opposes one spherical surface containing air, to another containing water, which necessarily causes the uterus to bulge laterally, thereby increasing the impaction more or less; whilst a fluid wedge of water moulds itself to the surface of the uterus and pelvic cavity, and compresses them alike at every point.

Should these methods of applying the taxis fail, I think we are then justified in introducing the whole hand gradually into the vagina as recommended by Naegele, and successfully practised in many of the cases on record. From my own experience of pelvic manipulation, I should be more inclined to favour such a proceeding *per rectum*, because of the mechanical advantages. A cart can be much more easily raised at the extremity of the shaft than at any part of the body; so also with the retroverted gravid uterus, the further we apply our force from the centre of motion, the cervix, the more easy will be our efforts at reduction. Perhaps a combination of both, left to the discretion of the operator, will be found to be the proper course. As to the use of such auxiliaries as venesection, the warm bath, tartar emetic, and the like, they also must be left entirely to the good judgment of those in attendance.

There is a plan lately recommended by an associate, Mr. Greaves of Manchester, to which I will allude; namely, that of exerting pressure on the fundus uteri with two fingers of the left hand *per rectum*, and with the palm of the right hand, making a *counterpressure above the pubes*. Mr. Greaves says, "by this manipulation, persisted in for a few minutes, I succeeded beyond my hopes." (Journal, 1859, p. 270.) From Mr. Greaves's interesting description of the case, it appears that the first stage of labour was nearly completed, that most probably a pain was on when he was engaged in applying the taxis, and that the membranes ruptured just before or about the time of the sudden reduction; that the patient was six months advanced in pregnancy, and that the pelvis "was more than average size." Under these circumstances, I quite agree with Mr. Greaves that, "if the method employed by him did no good, *the pressure of the open hand through the abdominal walls can scarcely do harm*." I would add that, if pressure is at all justifiable, it ought to be used simply as a support, or to press the os and cervix gently from the mesial line, so as to cause them to occupy the anterior extremity

of the right oblique diameter of the brim, where they will obtain greater space and facilitate the reduction of the fundus. As regards this method, the term *counterpressure* should not be used, because I have already shown that counterpressure is contrary to the mechanism in two ways; first, it counteracts the efforts we are making to raise the fundus *per rectum*, and second, it is an attempt to reduce that which ought to be second in the order of reduction, the cervix, before or along with that which alone ought to be first in the same order, the fundus. Of this last mode of reduction, I would further remark, that the first who practised it was the father of the present Dr. Ramsbotham. He made use of it in 1817 in a case about the fifth month of pregnancy. (Practical Observations, vol. ii, p. 441.) Subsequently, in 1844, the same method was practised by Dr. De Billi, of Milan, in a case of his, eight and a half months pregnant. (Edinburgh Monthly Journal, 1845.) And lastly, it has been resuscitated by Mr. Greaves, who, it would appear, was not aware that it had been previously adopted. It is chiefly applicable in cases detected beyond the first half of the period of gestation.

Should a judicious perseverance in the use of all such means prove unsuccessful, we have only one alternative, namely, to produce abortion by tapping the liquor amnii. We may do so in one of two ways; first, by rupturing the membranes through the os uteri, by the finger, the use of a stylet or suchwise, if possible; and secondly, by the operation of *paracentesis uteri*, as first recommended by Hunter, and practised so very successfully by Mr. Baynham of Birmingham; only I would give a decided preference to the operation being performed *per vaginam* than *per rectum*, as a recto-vaginal fistula would be less likely to result, and the placenta would be less in danger of being wounded, which happened in two places in Mr. Baynham's case, which was operated on *per rectum*.

In conclusion, I would observe, that even in the simplest case, whatever method is adopted, there is danger to be apprehended, and less speed to be obtained through haste, whilst the exercise of gentleness, patience, and perseverance, affords us the only safe prospect of real and permanent success. I will only add, that in every case the retroversion ought on no account to be considered reduced until the os uteri is felt to occupy its normal position in the pelvis.

[We think that Dr. Skinner has not given due credit in this paper to the *position* which a patient ought to be placed in when about to have her retroverted womb restored to its proper place. Bear in mind that in these cases the fundus uteri is downwards and backwards, and ought to be upwards and forwards. What, then, is the best position in which to accomplish this restitution of position? Take a pelvis and examine the axis, hold a pear by its stalk and let it drop downwards and backwards into the hollow of the sacrum in the upright position, do the same when the pelvis is placed in the usual obstetrical position. Now try to rectify the pear by pushing it up with the finger or with an instrument and you will have great difficulty, but reverse the position of the pelvis, place it on the table *inverted* and resting on its pubes and promontory of the sacrum, and the pear will drop into its place almost by the force of gravitation. So in these cases we ought to place the woman with the pelvis on the edge of the bed, the legs across the bed in the prone position, with the head and face towards the floor. In this position gravitation will most materially assist any manipulation with the finger either in the vagina or rectum. Moreover, the position can be maintained for a considerable period, and facilitates rather than interferes with the operations of the finger in the rectum, and does not much hinder the same in the vagina. Dr. Skinner's advice about the hand in the rectum is objectionable, although we believe the hand *could* be introduced into this canal if all other methods failed.]—*Brit. Med. Journal*, *Dec.* 8, 1860, *p.* 949.

## 92.—ON THE INDUCTION OF PREMATURE LABOUR.

By Henry James, Esq., Surgeon and Accoucheur to the City of London Lying-in Hospital.

The important position which the induction of premature labour holds amongst obstetric operations, from the great interest which attaches to it as a means of saving the life of the child as well as of alleviating the suffering of the mother, has induced me to communicate to the profession the method which I have for many years adopted for its induction, with almost uniform success. And here I may add that I have learnt, since the time when I first, in 1848, used the operation to which I refer, that Dr. Stultz also successfully adopted a similar plan at an earlier date—namely, in 1834.

When about to perform the operation, the patient is placed in the usual obstetric position, and the forefinger of the left hand is then passed up to the os uteri, and, as soon as possible, between its lips into the uterus; the neck is now to be slightly pulled down, and the finger, still within, is to be passed round it as far as possible. But the passage of the finger through the os uteri is seldom accomplished upon the first attempt; upon the second trial, however, the vagina will generally be found to be more lubricated, and the os also to be situated lower down; and frequently it will be necessary to repeat the manipulation upon the following days until the third or a fourth time; and great assistance may also be obtained by the patient bearing down, or by pressure being made with the right hand of the operator upon the fundus of the uterus, because in cases requiring such aid the uterus is kept high up by the unusual prominence of the promontory of the sacrum. Upon each of the above occasions some pains will usually ensue, and ultimately the sensations of labour will come on; when an elastic tube, or a male catheter (No. 10), free at one end and attached at the other to an elastic bottle, is to be passed up through the os and between the walls of the uterus and the membranes to the extent of from four to six inches. In doing this, considerable care is required so that rupture of the membranes may be avoided; and if any resistance is felt to the passage of the tube it should be withdrawn and passed in another direction. Mention is made of this more especially, because I once experienced such an obstruction, and upon pushing the catheter on, a stream of blood came through it; and I inferred that, without doubt, the catheter had been passed under the placenta. The catheter or tube having been in this manner inserted, pressure is to be made upon the elastic bottle connected with it, which has been previously filled with eight ounces of cold water, and in this way a sensation of cold is conveyed to the child, which generally causes it to plunge about; afterwards pains follow, and the water which has been injected is generally returned in gushes. So soon as the os is dilating and the membranes are at all protruding, they should be ruptured, and the birth will speedily take place. The patient should be desired to move about freely during the period, and a brisk purgative may also be given. I have made use of the secale as an auxiliary, but have been disappointed with the result.—*Lancet, March* 2, 1861, *p.* 210.

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93.—*Annular Laceration of the Cervix Uteri.* By Dr. T. Herbert Barker.—The term "annular" was chosen to distinguish that form of laceration during labour by which a complete ring of uterine structure, consisting of the cervix and os uteri, is separated from the remainder of that organ, the child having passed through the laceration, and not through the os uteri. The cause of this kind of laceration is, the protracted pressure of the head against a circle of the uterine parietes in a contracted pelvis,—perhaps, also, conjointly with a sharp linea ileo-pectinea. In the case related, the author was called, on the fourth morning of labour, to a primiparous patient, 42 years old, when he found the os uteri was not dilated larger than a florin, the pelvis contracted, and the bladder much distended. Soon afterwards, an ear could be felt under the pubes, through a transverse laceration in the cervix uteri, anteriorly. An attempt was made to deliver with the forceps, but un-

successfully, and craniotomy was performed. After the birth of the child, a circular mass was found in the vagina, consisting of the os and cervix uteri, separated from the body of the uterus at every point, with the exception of an inch and a half posteriorly. The os uteri was of the size of a florin, and its margin thin, even, complete, and well defined. The lacerated edge was jagged, rough, rather thicker than the margin of the os, but not so thick as the intervening uterine texture. The colour of the entire mass was of a purplish-red, venous tinge. There was no hemorrhage. It sloughed away on the third day. The catheter was required for seven days afterwards. The patient recovered, and the catamenia returned. With regard to the treatment, Dr. Uvedale West, in a letter to the author, writes that in such a case he would endeavour to dilate the os uteri by introducing first one, then a second, then a third and a fourth finger, side by side, to procure sufficient dilatation to admit either the vectis or the forceps.—*Lancet, Dec.* 15, 1860, *p.* 586.

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94.—*On Turning.* By JOHN PEARSON, Esq., Staleybridge.—[Mr. Figg, in a paper lately published in the Medical Times and Gazette, attributed to Dr. Simpson the merit of having revived in Great Britain the operation of turning in cases of pelvic distortion. Mr. Pearson says:]

Much as I admire that eminent Physician-Accoucheur, and however grateful I feel for the very valuable contributions he has made to Obstetrical Science, I cannot allow such an assumption to pass unnoticed, as I am personally able to state that for fifty years past at least, that excellent method of terminating difficult labours, has often been practised in this town. When I joined the late Mr. Hutton of this place as assistant in the year 1823, he very early instructed me in this method of safely conducting difficult parturition, stating that in the course of his long practice, extending from the year 1800, he had often had recourse to it successfully. The most remarkable instance which I now recollect of the value of this operation, in which I was concerned, occurred in the year 1835, when I was called in consulation to a case of great pelvic distortion, formed by the sacrum projecting so much as seriously to diminish the conjugate diameter. This was a second labour, the female having been delivered of her first child two years before, by craniotomy, under the care of a very talented surgeon in Bolton. When I was called in she had been in labour about twelve hours, the os uteri was well dilated, but the head had not made the slightest advance into the pelvis. I at once suggested to the medical attendant the possibility of effecting the delivery by turning, instead of again having recourse to craniotomy, which offered the only alternative. This gentleman declined to make the attempt, but consented to my undertaking the management of the case. I am glad to say, I succeeded in delivering a male child, asphyxiated at first, but which soon recovered, and is now a healthy man. Two years afterwards I delivered the same female of a second child by the same process, a female, and who is now a mother. This woman is still living.—*Med. Times and Gazette, Nov.* 10, 1860, *p.* 466.

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95.—*Spurious Pregnancy.*—[The following is extracted from a lecture by Dr. Priestley, delivered at the Middlesex Hospital.]

Perhaps the most curious of all the imitative diseases are those conditions named respectively hysteric tympanitis and spurious pregnancy. These affections have often been mistaken for ovarian or uterine tumours, or for genuine pregnancy. The abdomen is projected forward, as if it contained a solid tumour or gravid uterus; and if there is much fat in the subcutaneous tissue, the muscles of the abdomen, being unusually tense, yield a sensation to the hand very similar to that experienced when a solid mass is really present. In cases of this kind, the abdomen has in fact, been tapped, and on several occasions been laid open by incision, for the purpose of excising an ovarian tumour, when none was present. The use of percussion ought, of course, in these cases, to do much in clearing up doubts concerning the nature of the abdominal swelling; but more conclusive still is the evidence

afforded when we administer chloroform to the patient. If the case be one of hysteric tympanitis or spurious pregnancy, uncomplicated with solid tumour, the swelling entirely disappears when deep anæsthesia is induced, but it reappears as consciousness returns.—*Brit. Med. Journal, Feb.* 23, 1861, *p.* 190.

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## 96.—A NEW CRANIAL PERFORATOR.

By Dr. GAILLARD THOMAS, Secretary to the New York Academy of Medicine.

[There are some difficulties in the use of Smellie's instrument, (the one generally used,) which Dr. Thomas thinks will be avoided by the employment of that invented by him.]

The instrument consists of a steel or iron tube, ten inches long, which ends in a screw, and hides within itself a cutting blade which is thrown out of its bed by the hand of the operator compressing the handles. Two inches from the extremity a shoulder is placed, which prevents its entering the head too far, and the blade is fixed upon its pivot by a slot, which renders its removal easy for cleansing the instrument. It is hoped that this instrument will possess these advantages:

1st. There is no difficulty of introduction, for the screw, being a double one, catches readily in the scalp and penetrates the skull with the same ease, and by the same force which would cause a gimlet to enter wood, or the screw of a ramrod to engage the wadding of a gun.

2nd. It will not slip, for it is not pressed against the skull at all, but is wormed in by the process of boring.

3rd. The operator himself throwing out the blade, he can regulate its progress and thus prevent its cutting the surrounding parts; an advantage, however, possessed by Naegele's instrument.

4th. There is only one cutting edge, and consequently, this is easily guarded; the attention of the operator not being distracted by one on the other side acting in a different direction. Two or three essays have convinced me that the instrument will penetrate the skull with great facility and certainty, and safety for the mother.

In presenting it to the Academy, I wish to be distinctly understood as in no wise entering a plea for a more frequent resort to craniotomy, an operation which must diminish in frequency of performance *pari passu* with the advance of the obstetric art. But if, (as no one will deny) this sad operation must in some cases be done, it were better that it were well done.—*American Med. Monthly, Dec.* 1860, *p.* 429.

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## 97.—CASE OF CÆSAREAN SECTION.

By Dr. JAMES EDMUNDS.

[The patient was tall and well-formed, and thirty-eight years of age. She had had two dead children, the last six years ago. She had had pain in coïtu and fœtid sanguinolent discharge since this last confinement. Labour commenced of the third child on Dec. the 4th. On the 6th,]

I was requested to see her, and found projecting into the vagina a mass about two inches in diameter, hard, tuberculated, and superficially ulcerated. I had great difficulty in making out the os uteri; but with firmness and care I got the left index-finger through the mass, and managed with its tip to feel the head, my finger being then immersed over two inches in a dense, unyielding tissue, referable only to cancerous enlargement of the os and cervix uteri. Pulse about 90; pains frequent; muco-sanguineous discharge, and soft, warm, dilatable vagina. Nourishing diet, opium, and patience were prescribed. Afterwards I saw her daily: the pains continued; her pulse increased in frequency; she became much wearied, and on Sunday exhaustion was coming on, but the os had not yielded. She was a calm, intelligent woman, and I now explained the cause of her suffering and danger, gradually breaking to her the alternatives of her position—1st, exhaustion and death from obstructed labour; 2ndly, laceration of the os uteri, and probable death from arterial hemorrhage; or, on the other hand, two practicable operations for her relief—1st, slitting up the os uteri, and delivery by the forceps or embryotomy: 2ndly, *the Cæsarean section.*

I made her fully understand each procedure, with its risk and contingencies. I told her that the first would cut through a mass of diseased tissue, immensely supplied with bloodvessels, whose mouths would be out of reach, and would bleed so largely that both herself and child would probably perish; that I thought the latter, by direct incision into the bowels, though so frightful to contemplate, was the less dangerous. I left her to consider the matter, to consult her husband and parents, and then send them round to me, when I would further explain to them, hear their decision, and arrange to meet any other medical men they might name.

Her friends came—said that she had fully made up her mind, and if I would do what I thought best in the case, that, whether she died or recovered, they also should be satisfied. I thought it expedient for the responsibility of the case to be shared by others, and Dr. Giles, of Limehouse, saw her with me at midnight.

She had now been nearly six days in labour; there was considerable discharge; the vagina was soft and relaxed; her pulse, when recumbent, was 120, sharp, and weak; and her face showed anxiety and impending exhaustion; but there was the same hard, unyielding os uteri, and careful examination showed that there was no part healthy or dilatable. I had seen two thousand cases of labour, and my mind had decided for abdominal hysterotomy. Dr. Giles concurred without hesitation, and the operation was fixed for three a. m.

We left her, and my assistant, Mr. W. P. Dukes, administered a terebinthinate enema. Three other medical friends then met us at my house, and I read through a list of instruments and programme of procedure, which was discussed and adopted, and the operation then carefully rehearsed.

I ascertained that no one present had lately been dissecting, or was likely to be a source of infection; the instruments, needles, and silk were new and in perfect order; lastly, we all cleansed our hands most thoroughly, and made our way into the bedroom in detachments shortly before the appointed time.

Mr. Edmunds, senior, Dr. Giles, Mr. Nightingall, Mr. Nelham, Dr. Meeres, Mr. Dukes, and myself were now present; and as the friends had been unable to engage a first-rate nurse, Mrs. James Edmunds had consented to be present, and take charge of sundry minor arrangements.

I now requested the four eldest to examine her most carefully; to wedge the finger through the cancerous mass, and reach the head if possible, to satisfy themselves that no fissure or dilatable part existed; and, in short, to finally decide upon this formidable operation, and share with me the responsibility of the proceeding, whatever its result. Unanimous concurrence followed, and the arrangements proceeded.

I had heard the brisk tic-tac of the fœtal heart in the left iliac fossa, where the child's back and shoulders proved to be, but could not satisfy myself of *the position* of the placenta. Dr. Meeres thought he heard it in the right

iliac fossa, and Dr. Giles corroborated him; but I pointed out that a similar sound was also audible on the left side, and we then thought both proceeded from the respective iliacs sounding through the uterus; and as nothing like the souffle was discovered anywhere else, and through the os the head was distinctly felt to be covered by the membranes only, we conjectured that it was seated at the back or fundus, and was placed well for the operation. In fact, however, it was attached to the front of the cervix, extending across, and simulating the sounds often heard from the iliacs when the patient is recumbent. She had passed urine just before our arrival; nevertheless, I used a male elastic catheter, and drew off a teacupful of urine; then with a large silver catheter I broke through the membranes, and let out some ounces of liquor amnii.

The patient, having a warm flannel petticoat loosely tied round her waist, was now seated on the edge of the foot of the bed, her legs hanging over, and the head and shoulders resting comfortably on pillows. Mrs. Edmunds was superintending the water, basins, bath, and other fire-place arrangements; Mr. Dukes had charge of the instruments, ligatures, &c.; Mr. Nightingall and Mr. Nelham each took one side of the abdomen, to keep its wall in close contact with the uterus while the incisions were being made, so as to prevent blood or fluid from flowing into the cavity of the peritoneum, and to hold back the intestines when needful; Dr. Meeres had charge of the chloroform and I was to use the knife.

I now most thoroughly cleansed my hands, to free them from urine, or other possible contamination. The chloroform was commenced, and in a few minutes the patient was insensible. I drew her flannel down to the hips, and with a large heavy scalpel made an incision upon the inner edge of the rectus muscle, and cut into its fibres from the navel down to near the pubis where I deepened it cautiously on account of the bladder.

I then opened the cavity of the abdomen at the middle of the incision, carefully completed it downwards, and then, making a director of my two left forefingers, I rapidly slit up the peritoneum, the knife's back being to the intestines, and its point preceded and carefully guarded by the fingers. The opening exposed the uterus, of a purplish-rosy colour, everywhere smooth and glistening, except at the top of the incision. Here a roundish projection appeared, resembling a cancerous nodule; but it yielded readily to pressure, and was, in fact, a space where the muscular tissue was absent —a sort of uterine hernia. In horses which have been staked sufficiently to break the muscular wall, but not to lay open the cavity of the abdomen, analogous bulgings are often seen. Doubtless, unexpected uterine ruptures often spring from such imperfections of structure.

I now cut into the uterus from above downwards in the median line, and its fibres retracted; but, on deepening the incision, blood gushed rapidly out. I involuntarily said, "We have the placenta," and some one else said, "She is dying;" but I instantaneously plunged the knife completely through the uterus and placenta, cutting upwards as I withdrew it. I then dropped the knife from my right hand, thrust the two left forefingers through the wound, and hooked out a portion of the placenta, drawing it carefully through, and seizing it again with the right and left hands alternately until I had extracted the whole, when I dropped it, and recovering the knife again, used the left fingers as a director whilst I slit up the uterus, as I had previously done the peritoneum.

The child was in the first position, and, relinquishing the knife, I seized the feet, and, steadying the uterus with my left hand above the incision, carefully withdrew the child and passed it to Mr. Dukes, who made it cry, and separated the placenta.

As the child was drawn out the uterus embraced the head; and when this escaped, it further contracted and immediately receded, the intestines at the same moment flowing out from above in a most troublesome manner. A piece of membrane showed through the uterine gap. I tried to drag it out, but it broke away, and feeling that the membranes were adherent, I left them, but carefully pushed back into the uterus what shreds I could not tear

away. The uterine wound now seemed about two inches long, only slightly gaping, of a pale flesh colour, and free from blood.

Mr. Nightingall and Mr. Nelham, on each side, now sustained the intestines most judiciously; whilst I, using my left hand to back in and guard the viscera, put in the first stitch less than an inch from the top of the incision, including no muscular fibres, only skin and adipose tissue, Mr. Nelham seizing the needle and drawing it through, whilst my hands held the intestines and skin in place. In this way the wound was closed with ten stitches of the interrupted suture—the first six of double silk, and four single threads at the intervals. The needles were three inches long, and moderately curved; the silk as large as small whipcord, and well waxed.

The abdomen being sponged, the edges were accurately adjusted, and secured with strips of non-resinous adhesive plaster on unglazed calico; then a firm roll of lint, the size of a cane, was held on each side of the incision, whilst I fixed them with plaster, so as to press together the deep parts; and afterwards a long four-inch bandage was rolled round the abdomen.

The pillows having been shifted, she was drawn up into a nearly horizontal posture, and covered with a woollen shawl, so as to be cool, yet free from draught; more clothing was to be added when she felt chilly. A drachm of nepenthe, in a little brandy-and-water, was now given to her, absolute repose enjoined, and she was to drink from a teapot beef-tea, broth, and milk-and-water, at her own discretion.

After careful examination, we estimated the entire loss of blood at about fifteen ounces.

She afterwards took mild nourishment, with fruit, and occasionally a little wine-and-water.

The after-pains were relieved by a few doses of anodyne, and the catheter was sometimes required.

On the fourth day there was general peritonitis and constant vomiting, but the breasts filled. Ice, and nothing else, was taken by the mouth; beef-tea and anodyne per anum; and on the next day her pulse was down to 120, the sickness passing off, and the bowels responding to the presence of the long tube. At night the plaster was softened and removed, the six principal stitches taken out, and the wound secured with plaster and bandage. The other stitches were removed on the eighth day. On the eleventh day, the wound was beautifully united; and on the fourteenth it was so firmly consolidated that coughing hurt her but little.

After the sixth day, the peritonitis gradually subsided, and the bowels and bladder acted naturally; milk continued to run from the breasts, but no lochia appeared till the ninth day, after which they were abundant. On the tenth day, she was on the sofa to her dinner; on the twelfth, she sat up for a few hours; and on the fourteenth day she was convalescent, and said she had got on as well as after her former confinements.

She has since gained strength rapidly, is now suckling the baby, and doing marvellously well.

*Remarks.*—Irrespective of the actually successful results, we believe that Cæsarean section gave this mother a better chance than embryotomy. Therefore we incurred not the painful task of weighing the infant life against increase of maternal risk.

The position of the placenta was a curious coincidence, and had we known it, would perhaps have deterred us from the operation; yet this case will establish the fact that, if it be done with resolution and rapidity, the placenta may be cut through and dragged out first, without losing either mother or child, and also that it is unnecessary to endanger the arteries on either side of the uterus in order to avoid it. The gush of blood which followed the second stroke of the knife left no time for consultation or even for thought, and I instinctively adapted Dr. Simpson's practice to the emergency. The placenta was transfixed a little above and to the left of its centre.

The remaining of the membranes caused much anxiety, still they seemed firmly adherent, and it was best to leave them. I think it most important *that the* protruding shreds were pushed carefully back into the uterus, else

the peritonitis would probably have been fatal. No membranes have passed per vaginam. The lochia were delayed, though the breasts filled on the fourth day, and the milk remaining through the after symptoms sustained my hopes of her recovery. The turpentine was used to clear out the gaseous and feculent contents of the intestines, and to prevent hemorrhage. The exclusion of all sources of infection, the purity of the instruments and sponges, and thorough cleansing of the hands are points to which great care was given. Excepting a simple anodyne, no drugs were administered; no mercury on the one hand, and no brandy on the other. She had simple and varied nourishment with fruit.

I thought these great incisions more likely to be healed by nature in her own way than by nature under the influence of either mercury or brandy. The peritonitis, though most severe, was treated simply, and the result will confirm me in the practice.

This case will go towards diminishing the hitherto frightful death-rate of our Cæsarean operations.—*Lancet, Jan.* 5, 1861, *p.* 4.

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## 98.—ON A NEW SIGN OF POST-PARTUM DETACHMENT OF THE PLACENTA.

By J. Clay, Esq., Senior Professor of Midwifery, Queen's College, and Surgeon-Accoucheur to Queen's Hospital, Birmingham.

[The rules usually given in obstetric manuals for the management of the placenta, after the birth of the child, are:—to wait for a pain, or, to feel for the insertion of the cord into the placenta. But both of these may mislead, and the frequent examination necessary annoys and pains the patient.]

Four years ago I was led, from these causes, to investigate the subject, with the view of improving, if possible, upon the old mode of managing the deliverance of the after-birth. I thus ascertained certain facts, from which I came to the conclusion, that a very simple sign existed by which the separation of the placenta, after the birth of the child, might be indicated; and having tested it in upwards of nine hundred cases, I hope I may be considered to be fairly entitled to lay the results at which I have arrived before the profession.

Before dividing the umbilical cord, I always apply two ligatures, and make both sufficiently tight to prevent the occurrence of hemorrhage. If the maternal part of the cord is now examined, it will be found to be in a flaccid condition, and almost free from blood; but if it be again examined, at an interval, say from one to three minutes, it will be found to have acquired increased specific weight, and that the vessels are more or less filled with blood. The one fact may be ascertained by poising the cord on the fingers; the other by slightly grasping the cord near the vagina, with the thumb and forefinger of the left hand, and with the fingers of the right hand, suddenly compressing it, when a well-marked sense of fluctuation is perceived underneath the fingers of the left hand,—a kind of resiliance similar to the feeling produced when an elastic tube filled with fluid is suddenly compressed.

When the placenta is thrown off, or sufficiently detached to give rise to a tolerably free hemorrhage, the cord loses its increased specific weight and the hydrostatic property just mentioned. These phenomena occur so invariably, that *the loss of the previously acquired hydrostatic properties of the cord after the birth of the child constitutes the sign of detachment* previously referred to.

The whole of the phenomena are manifested in three stages, viz.:—1st, a state of flaccidity; 2nd, a state of repletion; 3rd, a state of flaccidity.

If the umbilical cord be tightly grasped by a spasmodic contraction of the os uteri, or by irregular contractions of the body of this organ, the *loss* of the particular hydrostatic properties may be delayed for a brief interval; but in a few seconds the spasm subsides, and those phenomena are produced which indicate the separation of the placenta, and that this structure may

be safely extracted. These signs are not, of course, always equally marked in every case,—often requiring experienced tactile management, in order to detect their presence. When the uterus is in a flaccid condition, the phenomena are manifested in a very slight degree, but are still perfectly reliable. On the other hand, when the uterus is contracted, with some degree of firmness, on the placenta, they are so well marked that the most inexperienced may readily detect them. In cases of partially adherent placenta, the disappearance of the hydrostatic properties, after being once fully developed, and the failure to deliver the placenta by the usual manipulations, have always indicated to me the necessity for promptly adopting artificial detachment by the introduction of the hand. In twin cases, if the cord is firmly tied, I have invariably found that the signs persisted until the birth of the second child. In one case, where the hydrostatic properties disappeared, after being well marked, before the birth of the second child, I found, on examination, that the corresponding placenta was detached, and I at once removed it, which otherwise would probably have been suffered to remain. Neither mother or child incurred any risk.

It sometimes occurs that the placenta is separated simultaneously with the birth of the child. In this instance, the first series of phenomena may be absent; and it may be prudent to wait before proceeding to extract the placenta, although it may be generally effected with safety.

The practical value of the application of these facts to obstetrics is obvious, as by merely compressing the cord in the manner previously indicated, the precise time of separation may be easily ascertained, the placenta at once extracted, and the patients thus freed from those frequent annoying examinations usually employed. The prompt delivery of the placenta, on the first efforts of the uterus, is very important, as this organ contracts then more efficiently, and the risk of hemorrhage is not so great, and it may be fairly assumed that the puerperal convalescence is not so protracted as under a more dilatory proceeding.

To students, or inexperienced practitioners, it might be a safe instruction to impart—not to interfere in the extraction of the placenta so long as the hydrostatic properties herein defined are persistent.—*Dublin Quarterly Journal, Nov.*, 1860, *p.* 372.

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## 99.—ON SUB-INVOLUTION OF THE UTERUS AFTER DELIVERY.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[The patient, whose case forms the text of this lecture, had suffered from disordered menstruation, and a feeling of weight and discomfort in the pelvis, since the birth of her last child. By examination it was found that the uterus was equally enlarged to about the size of a uterus in the third month of pregnancy, and readily admitted a sound to the extent of three and a half or four inches into its interior. The patient was suffering from an arrest in the retrograde metamorphosis that normally occurs in the uterus after parturition.]

After parturition, the greatly enlarged uterus begins regularly to involve or absorb, and rapidly diminishes in size, until it has been reduced almost to its pristine dimensions. I have had occasion, in another department of our course, to call your attention to the wonderful changes that go on in the uterus in consequence of impregnation, and to tell you how, from being a small body, two or three inches long, it comes, in the short space of nine months, to attain the length of a foot or more, while it is correspondingly enlarged in all its other dimensions and diameters. And I may here repeat the conviction that I then expressed, that if ever the laws of nutrition are to be clearly made out, the inquiry will probably be most successfully pursued by investigating these laws as they are seen in operation on a large and gigantic scale in this very organ; for in no other organ, so far as I re-

member, do we ever see nutrition and growth going on so rapidly as here, where out of a mere mass of nucleated fibres and cells an enormous body of numerous and well-marked muscular fibres become developed within the course of the nine months of pregnancy. Hardly less wonderful than this great development of the uterine walls during the progress of gestation, is the still more sudden diminution that occurs in these walls after parturition has been completed. The muscular fibres—perhaps weakened and exhausted as a result of their violent action during the parturient process, and so rendered prone to degenerate—and deprived also, to some degree, of the supply of blood brought to them so profusely during the time that the uterine circulation was so much exaggerated, now undergo, after parturition, a fatty metamorphosis, in consequence of which they almost all melt down and disappear; so that in the brief space of five or six weeks the whole organ dwindles down, and diminishes to nearly its original dimensions. The muscular walls of the uterus are not absorbed as muscle, but, like many other effete structures, they first undergo fatty degeneration, and are absorbed as fat. This fatty degeneration commences on the inner layers of the walls, and passes from them to the outer layers. The fatty metamorphosis of the uterine muscular fibres appears under the microscope, a few days after delivery, as a series of glistening particles deposited in the course of each individual fibre. Now the patients, with whose cases I introduced my present observations, are suffering from this curious condition of the uterus, that after the birth of their last children, this retrograde metamorphosis of the uterus has not taken place during the puerperal month, or, rather let me say, has taken place only to such an imperfect degree that the uterus is of the size we usually see it have at the end of the first week or so after delivery. They are suffering, then, from a hypertrophy of the uterus, which is pathological in its permanency, but which results from a hypertrophy purely physiological in its origin.

*Its Ætiology.*—What, you will be ready to ask, is the cause of this enlargement of the uterus remaining permanently? How is it that the retrograde changes in the uterus become arrested and the absorption or involution of its walls is prevented? To these questions it would be difficult, or indeed impossible, with our present limited knowledge of the processes of development and degeneration that go on in the uterus, to furnish a satisfactory reply; but let me point out to you some causes which occasionally appear to lead to the production of this hypertrophy from arrested involution of the uterus.

1. *Rising too soon after Confinement.*—We know that both in the healthy and in the morbid state the uterus is apt to become more congested when the patient assumes the erect position, and that a morbid degree of congestion interferes with various physiological functions. And we can easily imagine that if a delicate woman gets up too soon after her delivery, and remains for a lengthened period erect, while the womb is still more than usually large, the circulation in its walls might get so impeded, and such an amount of congestion be produced, as would prevent the normal changes in its walls, and impede the free absorption of its disintegrated particles.

2. *Repeated Miscarriages.*—However it may be, we know, further, that in a number of cases an enlarged condition of the uterus of the same nature as that of which I am speaking, results from the frequent and rapid recurrence of a series of miscarriages and abortions in the same patient. You are aware that a woman who has once aborted is extremely liable again to abort at the same period whenever she again becomes pregnant. In such cases it is, moreover, remarkable that the patient often conceives again in a very short time after the occurrence of the abortion; and when this process has been several times repeated—the uterus undergoing the enlargement of a new pregnancy, before it has had time, as it were, to recover perfectly from the hypertrophy of a preceding one, the case sometimes ends in a complete disturbance of the normal physiological process of degeneration and diminution in the uterine walls; and the womb is left in a permanently hypertrophied condition.

*etritis*.—Again, if you inquire minutely into the history of patients
l with this form of disease, and ask them particularly as to whether
ng has gone wrong with them in their puerperal state, you will very fre-
y find, as in the case of the second of our Hospital patients, that with-
hort time after their confinement they have been the subject of an at-
f inflammation in the uterus or ovaries, or neighbouring pelvic organs.
will, perhaps, tell you that a week or two after their child was born
had a shivering, followed by pain in the region of the womb, and more
ss fever, which compelled them to remain in bed for some time. It
d appear as if the occurrence of metritis, or perimetritis, in the puer-
l female, exerted such an influence on the substance of the uterus as to
vent the occurrence of those changes that lead normally to its diminution
ize.

*ts Semeiology*.—The patient whom I have imagined you to be questioning
to the history of her malady, will probably tell you, further, that, after
e had recovered so far from her inflammatory attack, and from the conse-
ences of the bleeding and blistering to which she had been subjected for
cure, as to be able to walk about again, she began to be conscious of a
eling of discomfort in the lower part of the abdomen, to which she had
ever before been accustomed. There is frequently a sense of weight or
earing down of the uterus, of distress in the lower bowel, and of uneasiness
n connection with the action of the bladder, and a weakness, sometimes
amounting to actual pain, in the lower part of the back, and, in some cases,
a numbness of the lower limbs, which are all new to the patient. She at-
tributes, most likely, these feelings to weakness, and expects that as she
gains more strength the uneasy sensations will disappear. But some months
may elapse, and she begins to be disappointed at her never getting entirely
relieved of this local trouble; and after lactation is accomplished, if, indeed,
she have been able to nurse her child at all, she finds that her menses do not
return regularly, or become too profuse and painful. Leucorrhœa, too, is
sometimes present to a greater or less extent; and altogether the patient is
in a state of confirmed and anomalous bad health that is difficult of endur-
ance. By means, perchance, of tonics and sedatives, she strives to regain
her health and alleviate her uneasiness, and continues for a time to hope that
her former strength will still return, and that her ailments will disappear.
Gradually, however, the conviction becomes forced upon her that something
besides time will be needful for her cure, and that some kind of medical
treatment is required for her relief. Accordingly she comes to you, and
when you have heard a history such as that I have endeavoured to narrate,
you come to the conclusion that she is labouring under some disease of the
uterus. But what the precise nature of that disease is you will not be able
to fix and determine by studying the mere history of the symptoms. To
make out a correct differential diagnosis, you must institute an examination
of the uterus.

*Its Physical Diagnosis*.—On placing the hand over the abdomen, you can
usually feel the enlarged uterus rising in the form of a tumour out of the
pelvic cavity, and lying above the pubes, more especially in those cases
where the hypertrophy is very considerable. In all cases you can discover,
on making an examination per vaginam, that the vaginal portion of the cer-
vix uteri is enlarged—for this is a form of hypertrophy in which every part
of the organ is implicated; and you find the whole uterus to be unusually
large and heavy. This kind of local examination is, in the majority of cases,
most satisfactorily carried out when the patient is laid on her back; some-
times it is more convenient to have her placed somewhat laterally. But in
every instance of this kind you must bear in mind the importance of making
an examination with *both* hands simultaneously,—the forefinger of one hand
being employed to explore the uterus through the vagina, while the fingers
of the other are applied to the fundus through the medium of the anterior
abdominal wall. There is, perhaps, no variety of uterine disease in the di-
agnosis of which this sort of examination can be employed to more purpose
than in the case of simple enlargement of the organ. In a few patients the

abdominal walls are too thick to admit of your feeling anything very distinctly on palpitation; and there are others, still fewer in number, in whom a certain degree of uneasiness renders the necessary pressure painful to the patient. When you have thus got the uterus between the two hands, you can easily recognise the existence of the hypertrophy, and even determine the degree to which it has taken place. You can feel that the swelling is not due to the presence of fibroid masses in the walls of the uterus, for there is no irregularity and no peculiarity in its shape. It has unmistakeably the shape and contour of the healthy organ; it is only that organ in a state of equable hypertrophy. Such a condition of matters might still, however, possibly be due to the presence of a fibroid tumour growing from the submucous layers of the uterine wall, and projecting into its interior; and to make sure that there is no such tumour present, you must have recourse to another simple means of exploration, viz., the introduction of a uterine sound. The cavity of the healthy uterus measures usually, as you know, about two and a-half inches in length; and when the sound is introduced, you find its point is arrested at the fundus, when the knob placed on the convex side of the instrument, at two and a-half inches from the extremity, has reached the level of the external orifice of the uterus. Where the organ is enlarged, however, the knob I speak of slips past the guiding finger, and the instrument runs up into the interior to a depth of three or more inches; and when the enlargement of the uterus is of the kind I have been describing as dependent on defective involution of the organ after delivery or miscarriage, the instrument usually slips in at once without the slightest difficulty, for the uterine orifice and canal are preternaturally patent. When, on the other hand, the enlargement is due to the presence of a tumour in the cavity of the uterus, the point of the exploring instrument usually meets an obstruction immediately on traversing the canal of the cervix, and it may require, in such a case, the exercise of some skill and a little gentle manipulation in order to pass it into the interior of the uterus at all. Besides, after the sound has been fully introduced, you can often feel with it the projecting body; while in the case of the simply hypertrophied organ the sound passes freely and unobstructedly round in all directions. If with the sound thus introduced into its interior you raise the uterus towards the hand placed over the abdomen, you will be able to make out more distinctly and definitely than ever the real nature of the case. There is one difficulty in connexion with the use of the sound in such cases, of which I ought, perhaps, to warn you, and it is this: The great weight of the fundus of the uterus, sometimes aided by inflammatory adhesions, gives a strong tendency to various displacements of the organ, and more particularly to retroflexion of it. Such a complication you must, accordingly, be prepared to expect; and in passing the sound you must then remember to turn it with the point and concavity looking backwards towards the sacral promontory. It was not long after I had begun to make use of the sound in the diagnosis of uterine diseases and disorders, that I first fell in with and recognized a case of sub-involution of the uterus. The patient, whom I saw in consultation along with the late Dr. Abercrombie, was the wife of a medical gentleman, and there was much perplexity as to the nature of her disease. There was a large, rounded tumour lying over into the right iliac region, which was thought to be possibly either an inflamed ovarian tumour, or an abscess, as there was considerable pain on pressure. On passing a sound, however, it was found to run right up at once about four inches into the very centre and top of the tumour. The supposed tumour was thus shown to be neither more nor less than the uterus considerably enlarged and turned somewhat to one side, as sometimes happens in such cases. That was the first occasion, so far as I know, in which this kind of uterine hypertrophy was clearly made out; but since then I have seen it very frequently, usually as a result of inflammation after delivery, but sometimes associated with repeated miscarriages in the relation both of cause and effect.

*Its duration and degree.*—In some cases you will detect the form of uterine hypertrophy which I am describing, within two or three weeks, or two or

e months after delivery. These may be called the acute, or subacute, s of the disease. But oftener perhaps in practice, you will not be ap- d to till the disease is chronic,—till many months, or even years, have psed since the date of the last delivery, or of the last abortion, which left e uterus in the sub-involved or unreduced condition which we are consid- ing. The degree or amount of remaining enlargement varies infinitely in fferent cases; and is, perhaps, mainly regulated by the date of the confine- ent at which the arrest of the uterine involution takes place, and the com- leteness or not of that arrestment. I have seen instances where the uterus was large enough to be felt some inches above the pubis, and where the uterine sound passed into the uterine cavity to the extent of four or five inches. In other examples you will find the uterus in form and length re- maining hypertrophied only to a slight extent, and the sound passing per- haps not more than two and three-fourths of an inch or three inches. Be- tween these two extremes, you will meet in practice with all intermediate de- grees of this permanence of the puerperal hypertrophy of the uterus.

*Treatment of the disease.*—In the more acute forms of the disease, I have almost invariably found that by the immediate employment of local antiphlo- gistics the symptoms were speedily and successfully alleviated, and the pro- cess of absorption was set up. Where any traces of inflammation still re- main, the importance of this indication is at once apparent, and its fulfilment is in most cases attended with rapid relief. But even in cases where all in- flammatory action seems to have died out, and only its results remain, we often find—I cannot well tell you how—that a local antiphlogistic course of treatment has the effect of setting up absorption in the enlarged organ, and leading ultimately to its restoration to its normal state. In following out this line of treatment, then, you will do well—if the patient is not very weak and debilitated—to begin with the application of eight or a dozen leeches to the vaginal portion of the uterus, or simply to the perineum, or circle of the anus. I need hardly repeat, that the abstraction of a small quantity of blood in this way will be more particularly called for, and more especially benefi- cial in those cases where there lingers any degree of congestion or of inflam- matory action in the uterus. But even in such acute or subacute cases, and in all the more chronic forms, your chief reliance must be placed on the use of counter-irritants applied externally to the surface of the abdomen or sacrum. You may use antimonial or croton ointments, or paint the hypo- gastric surface twice a day with strong tincture of iodine till the skin be- comes tender; but the vesicant which I have found to be most efficacious in exciting the function of absorption in the womb that is now in abeyance, is the ordinary cantharides-blister, especially in the chronic forms of the dis- ease, and when the bladder is not liable to be irritated. You may order a series of small blistering-plasters to be applied for a time over the lower part of the abdomen, or you may paint a succession of small spots—one every second or third day, with blistering liquid, until the uterus begins distinctly to diminish in size. I think you will find the latter mode of applying the vesicant to be the simplest and the most certain. The plan I usually follow is to apply the fluid to a patch of about the size of a crown, and then gradu- ally to surround this patch with a number of others of like diameter, one being added every third or fourth day, until the original central point has been surrounded by a ring of five or six distinct blistered spots. At the same time that you are thus trying to excite absorption by the application of counter-irritants to the cutaneous surface, you must endeavour to promote this object by keeping the vaginal portion of the cervix uteri immersed con- stantly in ointments of mercury or iodide of lead, or bromide of potassium, or other remedies likely to have the effect of stimulating the absorptive pro- cess. This you can do by introducing, or making the patient herself intro- duce, into the vagina one or two pessaries medicated with these substances every night and morning.

But you will find that these local measures are not always, or in most cases, sufficient to effect a cure. You will be obliged to have recourse also al administration of some of the class of deobstruent remedies;

and, of these, the most efficacious are the iodide and bromide of potassium. I used, at one time, to rely chiefly on the administration of the iodide of potassium. But of late I have employed the bromide much more extensively, for it has this advantage over the former salt, that its use may be kept up for almost any length of time by a patient without her becoming subject to the kind of marasmus which we sometimes find attendant on the prolonged use of the iodide. The bromide of potassium may not only be given with safety for a lengthened period, but it may be administered with confidence as a good tonic, as well as perhaps the best deobstruent in the Pharmacopœia. It may be depended upon as an active stimulant to absorption, besides possessing the property, beyond all other remedies that I know of, of acting as a special sedative on the reproductive organs. In cases of this kind you must administer it in larger doses than usual, making the patient take six, eight, or ten grains of it three times a-day. Sometimes patients suffering from this form of uterine hypertrophy present themselves to you in a debilitated and anæmic condition. Under such circumstances, you need not hesitate to add iron, manganese, or some form of metallic tonic, alone or in combination, to the more specific remedies, and endeavour, always in the more chronic cases of the disease, by means of good diet and other hygienic means, to raise the patient's general standard of health.

You will occasionally meet with cases of this disease so obstinate, that the process of absorption does not begin, or does not go on to any satisfactory degree, even after you have had recourse to repeated leechings, and have kept the uterus imbedded for a length of time in discutient ointments, and have duly administered all kinds of deobstruents and tonics. In such cases you may follow up the course of treatment which I have sketched for a very lengthened period without producing any appreciable change in the size of the womb, or any marked alleviation of the patient's symptoms. If, however, by any means you can induce the uterus for a time to take on an action of increased growth, you may confidently hope that this temporary hypertrophy will be followed by a process of absorption which will go on, perhaps uninterruptedly, until the organ is reduced to its normal dimensions. Such a transient increase in the size of the uterus you may at any time produce by taking advantage of the physiological tendency of this organ to enlarge and become developed around any foreign body that happens to be lodged with it. You know that the uterus begins to enlarge when stimulated by the presence of an ovum, or of a fibroid tumour, or of a clot of blood in its interior; and in like manner it becomes developed in size when a foreign body, such as a sponge-tent or an intra-uterine bougie, is introduced artificially. By introducing, then, a succession of very small sponge-tents into the interior of the womb, or by making the patient wear for a time an intra-uterine pessary, you can cause the uterus to take on this hypertrophic action; and by afterwards actively and fully taking advantage of the tendency of the organ to undergo a process of degeneration and diminution on the removal of the stimulus, you may succeed, by the due employment of the various discutients and deobstruents I have already mentioned,—as rest, counter-irritation, and bromine,—in promoting the process of absorption to such a degree that the uterus at last becomes reduced to the natural standard. In some obstinate instances I have been obliged to repeat from time to time this process of artificial irritation and dilatation before a perfect cure was effected.—*Med. Times and Gazette, Jan. 5*, 1861, *p.* 1.

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## 100.—ON SUPER-INVOLUTION OF THE UTERUS AND AMENORRHŒA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[The condition of super-involution of the uterus is precisely the opposite of sub-involution or permanent enlargement, which is owing to deficient disintegrating process after delivery. In the disease under consideration the size

of the uterus is excessively reduced, so as to become smaller than its normal dimensions in the unimpregnated state.]

The hypertrophy resulting from a defective degree of degeneration and diminution of the womb, or *sub-involution*, is one of the most common of uterine disorders. The atrophy that results from excessive resorptive action in the uterus, or *super-involution*, is, on the other hand, a phenomenon of comparative rarity; but that it does ever and anon occur, is a matter to which there can be no dispute. I have seen in practice a very considerable number of cases of this morbid state; but I have rarely had opportunities of examining into its nature on the dead subject. Several years ago I published the history of a case of this kind occurring in a young woman of twenty years of age, who never menstruated after her first delivery, and where the morbid condition was traced, both during life and after death. It was two years subsequent to her confinement that this patient sought admission to our Clinical ward, in consequence of amenorrhœa and great constitutional debility. She was then subject to various distressing symptoms in connexion with the pelvic organs, and stated that she was subject to frequent attacks of diarrhœa, which she believed to be most severe at recurring monthly intervals; the dejections being then sometimes tinged with blood. The mammæ were shrunk and flat; and she was thin, feeble, and anæmic in appearance. On making a vaginal examination, I found the uterus small and mobile, the cervix uteri much atrophied, and its vaginal portion scarcely forming any projection into the cavity of the vagina. The os uteri was so contracted as to admit with difficulty a surgeon's probe. It was dilated by a slender bougie left in it for a few days, and when the sound was subsequently introduced, the uterine cavity was found to be only an inch and a-half in length. Various remedial measures were adopted without any very marked effect; and as the uterine symptoms seemed to be only of secondary importance, she was transferred to another ward, where she died in the course of a couple of months in a state of prolonged coma. During her stay in the hospital she suffered from frequent, and sometimes severe attacks of diarrhœa, and about a month before her death, albuminuria and general dropsy had set in. At the post-mortem examination crude tubercles were found in both lungs. The liver was enlarged, and showed fatty degeneration. The kidneys presented also some stearoid degeneration; and in the right there was a small tubercular abscess. The large intestines were very much thickened in their parietes, and contracted in their calibre; their mucous membrane was ulcerated in various parts. At the lower end of the ileum several large ulcerations were seen running circumferentially around the interior of the bowels. One or two ulcerations were also found in the stomach. The uterus was very small, and atrophied in its length and breadth; its size being diminished about a third below the natural standard in all its measurements, and its parietes were correspondingly thin and reduced. The whole length of the uterine cavity from the os to the fundus, was not more than one inch and a-half, instead of two inches and a-half as in the normal condition. When a section was made of the posterior wall of the organ, the thickness of its parieties at their deepest or most developed portion was not above three lines, instead of five or six. The tissue of the uterus appeared dense and fibrous. The ovaries were also much atrophied, and smaller than natural; their tissue was dense and fibrous, and presented no appearance of Graafian vesicles. There was no inflammatory deposit on the peritoneal surface of the uterus or its appendages; but there was some thick pus, or tubercular matter, in the distended cavity of the right Fallopian tube.

In this case we have evidence of the most unmistakeable kind that the uterus of a woman, in whom all the genital apparatus was so perfectly developed as to allow of her giving birth to a fully-formed child, became atrophied to such a degree, subsequently to her delivery, as to be incapable of again resuming its functional activity. How this excessive wasting of the womb is produced it is difficult to understand. Remembering that when inflammation is going on in any organ or tissue, the function of absorption is there in abeyance, we can understand how the process of absorption after

delivery should be checked in the uterus, by the occurrence of inflammation in its walls; and how in such a case the womb should be left in a permanently hypertrophied condition, when the absorptive action is not subsequently re-excited. We can even imagine that interruption to the free flow of blood through the uterine vessels, resulting from the patient getting up too soon after her confinement, should lead to a certain degree of congestion in the uterus, and should so far interfere with the process of absorption as to leave the organ in a state of abnormal hypertrophy. But we do not yet know enough of the normal process of involution of the uterus; the reason why the fatty metamorphosis and the subsequent absorption go on exactly to such a point that the uterus is restored almost precisely to its previous dimensions, has not been sufficiently elucidated to enable us to form any probable idea as to the causes which lead to its super-involution and permanent atrophy. Observations even are wanting to show whether this atrophy of the uterus depends upon a morbid degeneration and destruction of fibres that ought to have remained entire; or, as is more probably the case, on some defect in the development of the new cells and fibres that should have taken the place of those that had become effete. A uterus, which has undergone this morbid change, is found after death, as I have said, to be abnormally small. Instead of measuring two and a-half inches or so in length, and one and a-quarter in breadth, it is seen to possess little more than a half, or two-thirds of those dimensions; and this diminution in size affects, not only the body and fundus, but the cervix as well, so that instead of projecting into the vault of the vagina the os uteri seems simply to form a depression in the upper extremity of that canal. The walls of the uterus are at the same time extremely thin—hardly thicker, in extreme cases, than the coats of the intestines, and so friable as to be readily perforated by the point of a sound incautiously introduced. The whole organ, in short, presents all the appearance of the uterus of a girl not yet arrived at puberty; and differs from it only in this, that whereas the latter is in a condition to become enlarged, and to furnish the ordinary menstrual secretion so soon as a sufficient stimulus shall have been applied to it, in consequence of the changes connected with ovulation, the super-involuted uterus has undergone such morbid alteration as renders it unable any longer to respond to the ordinary ovarian influences. And here, again, arises an interesting question in regard to this subject which it remains for future histological research and pathological experiment to answer, viz., what is the cause of this insensibility of the super-involuted uterus to the reflex irritation which seems to be conveyed towards it from the ovaries at each menstrual period? Is it merely that the muscular fibres of the uterus have been so degraded and destroyed as to be insusceptible to the stimulus that usually calls them into functional activity? Or have the nervous filaments that traverse the uterine walls also shared to such an extent in the general atrophy as to be incapable of conducting the influence they ought to convey to the structures in which they ramify? Or have the uterine vessels been so wasted as to furnish an insufficient supply of blood? Or, finally, are the ovaries themselves in a perfectly normal condition, or have they, too, become morbidly altered in structure and impaired in function as a consequence of their intimate association with the diseased and degenerated uterus? These are all questions to which we are at present unable to furnish any adequate reply; but they offer such a tempting field for investigation that I trust some one will ere long try to discover their solution.

*Physical Diagnosis.*—To ascertain the true state of matters in such a case you must have recourse to a local examination of the uterus itself. On passing the finger into the vagina you feel the cervix uteri unusually small, and quite on a level with the vault of the vagina; or projecting into it so slightly as not to be at once easy of recognition. The womb feels unusually small and light, and moveable; and although the abdominal walls may be very thin and relaxed, yet the organ cannot well be felt between the hand placed over the fundus and the finger exploring per vaginam. The os is small, and admits only a narrow-pointed sound; or, as in the

case I have detailed to you, nothing larger than a surgeon's probe. In introducing the sound great care must be taken not to press it rudely on, for the walls are thin and very friable: and I have seen a sound thus rashly introduced, and forced with but little effort through the uterine parietes into the cavity of the peritoneum. I have known of this accident happening in more than one instance; and although no bad consequence has ever, so far as I am aware, resulted from it, and although the sound may, doubtless, be passed into the peritoneal cavity with impunity, yet it is not an experiment one would willingly and wittingly repeat. When the sound, then, is introduced in such a case with all due amount of caution, its point will be found to be obstructed and stopped when it has passed only to a distance of one and a-half or two inches, showing that the organ is preternaturally diminished in size.—*Med. Times and Gazette, Feb.* 9, 1861, *p.* 137.

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## 101.—ON THE TREATMENT OF PUERPERAL MANIA, AT ITS ONSET.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[As this disease is much more amenable to treatment in its commencement, than when fully formed, we extract that part only of Dr. Simpson's lecture on the subject of puerperal mania and hypochondriasis, bearing upon this period of the disease. It is now an almost established rule, that bleeding is seldom or never permissible in any case of puerperal mania. Supposing there is very great excitement, a few drops of tincture of aconite or of tincture of veratrum viride, may be administered. We must endeavour also to counteract, as far as possible, the super-polarity that exists in the nervous system by the administration of the various]

*Nervous Sedatives.*—Of these camphor was the remedy most frequently employed by Dr. Gooch in the treatment of puerperal mania; and you will do well to have it in recollection as a remedy that may sometimes prove serviceable. Opium is beneficial in some cases. In others it proves of no avail. You may remember that I called your attention particularly to the great degree of wakefulness that sometimes precedes the attack; and, bearing that in mind, you will see that in some cases it may prove of great service to administer to the patient an anodyne which shall counteract her restlessness, and send her off to sleep. But it is often as difficult to get a patient to sleep who is threatened with an attack of puerperal mania as it is to put to sleep a patient with commencing *delirium tremens*—sometimes even more so. A patient threatened with *delirium tremens*, as you know, usually feels sleep stealing over him in sixty or seventy hours after the commencement of the attack, whether he has got opium or not; but a patient commencing with puerperal mania may remain restless and wakeful for several days and nights before the excitability begins to subside. When once the patient does fall into a good sound sleep, however, her case becomes more hopeful, and the probability is that she will waken up tolerably well. This kind of sleep it must be your endeavour to promote or procure. If the patient is not so violent as to forbid the use of a warm bath, and such a bath be at hand, it sometimes acts as an excellent anodyne in this disease. Medicines are oftener used for the fulfilment of this indication; and I have seen some cases where an attack of mania has been cut short by a good full opiate administered with this view. The great difficulty is to get the patient to take the medicine. Sometimes she is too mad to swallow it, and too outrageous to be controlled. Whatever may be the way in which you give the drug, remember always, as the grand rule to guide you in its administration to such patients, that it must be given in very large doses. If you expect to have any good effect from it, you must give in general not less than two or three grains of solid opium, or an equivalent dose of some of the cognate preparations. You may give it by the mouth, or in the form of an enema; or, when the patient cannot or will not swallow, and will not admit of an injection, you may succeed in introducing

the drug in the form of a suppository into the rectum. A suppository containing half a grain or a grain of morphia is one of the most convenient and efficacious means of administering an opiate, especially for the relief of pelvic or abdominal pains in cases where the stomach is too irritable to tolerate any other preparation of the medicine. But to produce any effect on a patient threatened with puerperal mania, you will require to use a suppository containing perhaps one or two grains of the morphia. Some years ago I was sent for to see a lady about forty miles from Edinburgh, the second wife of a gentleman whose first wife had died of puerperal mania. The lady whom I had come to see was still in the stage of restlessness, had been chattering continually for some time, and refused to take any kind of medicine, or to admit of the administration of opium in any form. She recognised me when I entered her room, and rebelled at once, declaring she would have none of my remedies. With some little management, however, I succeeded in passing into the rectum a suppository with two grains of morphia, with this result, that in an hour or so she fell asleep, and after sleeping about sixteen hours she awoke up well, and had no recurrence of her maniacal symptoms. In some cases you may get the patient to sleep by bringing her under the influence of ether or chloroform. I have sometimes found that a patient, after being anæsthetised by means of chloroform, has continued to sleep on, and has afterwards wakened up quite well. More frequently, however, she awakes in the same state as when she went to sleep—no better, but also with no aggravation of the symptoms. In all cases, therefore, where such remedies are not contraindicated, you ought to give the patient the chance of recovery offered by the use of opium or chloroform. In using the chloroform, you must see to have the patient fairly anæsthetised and fully asleep; and you must either remain with the patient, or have some competent person beside her, to keep up the anæsthetic condition, by making her inhale a fresh quantity of the drug whenever she gives signs of awaking, as she is likely to do every half-hour or so. In some cases, as I have shown you, you may expect by this kind of treatment to cut short an attack of the disease. I have known the remedy successful in several instances, producing at once a cure. But this happy result, alas! does not follow in many. In nine cases out of ten, after the patient recovers from the influence of the narcotic or the anæsthetic, the symptoms recur, and the malady continues its progress unchecked. In these—the large majority of cases—therefore, you must seek to combat the disease by other means.—*Med. Times and Gazette, Dec.* 8, 1860, *p.* 550.

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## 102.—PUERPERAL HYPOCHONDRIASIS TREATED BY TINCTURE OF ACTEA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery at the University of Edinburgh.

When a patient has been much pulled down by hemorrhage, or becomes exhausted by nursing, a state of anæmia or chlorosis, attended by more or less mental depression, want of energy, and loss of memory, particularly of proper names, will supervene, requiring the administration of an improved diet, wine, and tonics, such as iron and quinine. But occasionally an analogous degree of mental misery and depression comes on without any preceding hemorrhage, and when the mother has not acted as a nurse at all. These cases are generally cured by the tonic means I have just alluded to; by change of air and scene, when that is practicable; and in some obstinate instances, where these measures fail, you will find Dr. Seymour's plan of steadily giving an adequate opiate every night, to be a mode of treatment followed by the best results. Of this type of disease I lately saw a very marked case that had utterly defied all the proposed modes of treatment, and that yielded at last with a rapidity which astonished both the patient and myself, under the use of the tincture of the black snakeroot or actea. This plant, the *Actea* or *Cimicifuga racemosa*, has been long spoken of as a remedy for rheumatism, and particularly in the more acute forms of the disease. In the edition of

Gray's Suplement to the Pharmacopœias, published in 1821, you will find the use of it in rheumatism stated. Latterly it has been employed by some American physicians as their most valuable remedy in acute rheumatic fever. My very intelligent and excellent friend Dr. Voris, of Rochelle, New York, told me, two years ago, that since employing the tincture of actea in rheumatic fever—and it is a very common disease in his district—he had seen the disease almost always cut short before the eighth or tenth day; the drug acting apparently as a simple antidote to the rheumatic poison, and curing without diuresis, diaphoresis, or any other discharge. The American physicians give a strong tincture of the root in acute rheumatism in doses of thirty to sixty drops every two, three, or four hours. It may be given, if you choose along with alkaline salts, or other anti-rheumatic drugs. I have found it, in my own case, repeatedly cure an attack of lumbago with wonderful rapidity. Some of the American practitioners who have written upon actea, have spoken of its use in terms that are, no doubt, exaggerated. Thus, Dr. Davis, of Chicago, says that, after much experience, he has no more doubt of the efficacy of actea in the early stage of acute rheumatism, than he has of the power of vaccination as a preventive of small-pox. But our American brethren have used actea also extensively in chorea and other anomalous forms of nervous disease. However unlike rheumatism and chorea may look to the superficial observer, yet the able investigations of Dr. Begbie and other pathologists have shown, as you are aware, an analogy, if not an identity, between the blood-poison which produces rheumatism and that which produces chorea. Dr. Physick, of New York, and Dr. Jesse Young, of Pennsylvania, about thirty or more years ago, recommended actea strongly in chorea. Latterly, Drs. Lindsey, Kirkbride, Otto, and others have published their experience in favour of the same drug in this disease. In a case of anomalous and severe chorea of long standing, which was under my care some months ago, the actea was given with excellent effect. The patient had been previously treated, both in France and in this country, with zinc, iron, arsenic, and all the usual remedies employed in this malady. But I have made all this long episode regarding the actea, not so much to speak of its uses in the preceding diseases, as of its use in puerperal hypochondriasis and depression. A lady, the mother of several children, was twice the subject of the most painful mental despondency a month or two after delivery. On one of these occasions she was confined in London, and had the advice of several eminent physicians; but the disease took a very long and tiresome course, seemed to defy entirely all remedies, and gradually and very slowly terminated. On the last occasion on which the attack occurred, this patient was confined under my care here, and went home to England some weeks subsequently, perfectly well. She returned, however, in about a month to Edinburgh in the lowest possible state of depression, a perfect picture of mental misery and unhappiness. I tried many plans to raise her out of this dark and gloomy state. All failed. At last, fancying from some of her symptoms and complaints, that there might be a rheumatic element in the affection, I ordered her fifty drops of tincture of actea thrice a day. After taking one dose she refused to continue it, as the drug had a taste so similar to laudanum, and as all opiates had made her worse. On being re-assured that there was no opiate in the medicine, she re-commenced it, without any faith, however, in the results, as she had in a great measure lost faith in all remedial means. When I saw her next, some eight or ten days afterwards, she was altered and changed in a marvellous degree, but all for the better. On the third or fourth day, as she informed me, the cloud of misery which had been darkening her existence suddenly began to dissolve and dispel: and in a day or two more she felt perfectly herself again in gaiety, spirits, and energy. But nothing would induce her to give up the actea for six or eight weeks longer; and the last time she passed through Edinburgh she told me that she had prescribed her own remedy to more than one melancholic subject with nearly as great success as she had used it in her own case.—*Med. Times and Gazette, Dec.* 8, 1860, *p.* 552.

## 103.—ON THE CONNEXION OF PUERPERAL INSANITY WITH ALBUMINURIA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery at the University of Edinburgh.

[Puerperal insanity is well known to depend upon various conditions, as, hereditary predisposition to insanity; anæmia; the peculiar state that exists after delivery; and upon toxæmia, or blood-poisoning. The connexion between puerperal insanity and albuminuria, is, however, much less known. Albuminuria precedes and attends the first access of puerperal insanity in a large proportion of cases, but not perhaps so frequently nor so constantly as it precedes and attends upon attacks of puerperal convulsions. The coagulability of the urine generally disappears within a short time after the attack of puerperal insanity commences, in some cases within two or three days of the access of the malady. When the insanity recurs in the form of successive attacks or explosions, each attack is connected with a new attack or advent of albuminuria. How can the presence of albumen in the urine originate two such diverse morbid states as convulsions and insanity? Pathological chemistry cannot yet solve such a question, but]

The presence of an excess of albumen in the urine is usually accompanied with other and far more important changes directly in the chemistry of the renal secretion, and indirectly in the chemistry of the blood itself. When albuminuria exists, there is always liable to accompany it a diminished elimination of the urea, and perhaps of other excrementitious matters that pass off by the urine; and, consequently, there is a corresponding retention within the circulating system of these effete substances that should be duly and systematically thrown off by the action of the kidneys. Urea when thus, or when otherwise, accumulated in morbid excess within the circulating system, does not, according to the observations and experiments of Frerichs and others, produce any very special intoxicating or poisonous effects upon the nervous system, merely *per se*, or simply as urea. But the accumulated urea is apparently liable to decompose and become altered under varying influences into the various new organic compounds within the body, some of which, like morphia, strychnine, coniine, and other vegetable alkaloids, produce different diseased states, by exerting different toxicological effects upon the cerebro-spinal and other systems. Dr. Frerichs holds that the mere simple and common decomposition of the accumulated urea into carbonate of ammonia is the cause of albuminuria ending in convulsions and coma. Urea, as urea, does not in his opinion, and according to his experiments, produce convulsive and comatose effects; but these effects are produced by a sudden excess in the circulating system of carbonate of ammonia, a salt which urea readily forms when it becomes decomposed. But when the whole constitution and chemistry of the blood becomes deranged and altered by the sudden accumulation of the excrementitial materials of the urine within it, as is always liable to happen in puerperal albuminuria, other organic toxicological agents may become developed within the system from the decomposing urea or other components of the urine—possibly some of these newly-formed products or agents of an *alkaloidal* character—and one or other of which may be as certain of exciting delirium and insanity, as an overdose of morphia or brucine, or other poisonous vegetable alkaloids, is certain of exciting their special toxicological effects upon the economy. Mr. Calvert and other chemists have lately found organic alkaloids of various, and most of them of still unknown, types, formed during animal decomposition. In the blood of the puerperal female,—greatly modified as it is in the normal states of pregnancy and delivery, and containing as it does after parturition the effete elements of the involving or disintegrating uterus, and the materials for the new lacteal secretion,—ferments and agents may possibly exist, which are more apt to develope special morbid poisons out of the retained renal excretions than happens in other states of the system. But I repeat, the whole subject is yet quite dark and conjectural, and will remain so till pathological chemistry is able to cast some light upon it.

*First Deliveries.*—It has been well ascertained that puerperal convulsions occur much more frequently in connexion with first than in connexion with subsequent labours, and that, consequently, puerperal albuminuria is correspondently more common in primiparous than in multiparous patients. Puerperal insanity is apparently also in the same way, and probably for the same reason, but not perhaps to the same extent, observed more frequently after first than after future labours. When speaking of the relations of convulsions to insanity, Dr. Reid quotes authority to the effect "that each of them is more liable to attack the female in her first accouchement than in after ones." Marcé found that among 54 cases of puerperal insanity, 14, or about 1 in every 4, were primiparæ. Out of a list of 53 cases collected by Dr. Gundry, 18, or about 1 in every 3, were found to have been attacked with the disease in connexion with their first pregnancy and first parturition.

*Mental Emotions.*—We occasionally find, in the case of puerperal patients, in whom all the circulatory and secreting functions are disordered and deranged, in consequence of parturition and the resulting changes in the generative and mammary organs, that a shock which, in other circumstances would have produced no more dire result than a passing mental emotion, may have the effect of disturbing completely the mental balance, and calling forth all the symptoms of one of the most distressing and dangerous forms of madness. Many years ago I had under my care a patient who had just recovered from her first labour, and had been going on extremely well for about a month, when she was abruptly told by a gossiping old lady whom she received in the drawing-room, that her father had become a bankrupt during the period of her confinement. This distressing news had, previously been most sedulously kept from the patient, and when thus suddenly and senselessly informed of it, she was led back to her bedroom quite insane, and died of a rapid and acute attack of puerperal mania. Another case, of a still more painful character, happened in the practice of a friend here. A lady—one of that rather numerous class of people who have a constitutional horror o mice—had made her husband happy by giving birth to a child, and was making a favourable recovery. A sister-in-law, who had a deep dislike to this lady, sent her a few days after her confinement a box, which she opened for herself in bed, in the expectation of finding a present for her infant, when she was literally "frightened out of her wits" by the escape of a lot of living mice; for such was the cruel gift of her evil-minded relative.

The forms which insanity usually assumes in the puerperal female are those of mania and melancholia—most frequently the former. But there is nothing special in the character or symptoms of puerperal insanity as distinguished from other forms of insanity. The symptoms, when the disease is established, are simply those of common mania or common melancholia, or of some intermediate type of mental disorder. Before the attack the patient is usually for a time restless and irritable, and does not sleep. If you have a puerperal patient complaining of great or total want of sleep, watch her narrowly and anxiously; for she may speedily lapse into insanity. If, in addition to this wakefulness, you discover the urine to be albuminous, the probability of insanity impending over her becomes only the greater. The first suspicious symptom of the actual disease generally consists in some oddity o thought, or of expression, or of affection. I lately saw a case in the country where the patient's insanity began with her insisting upon her doctor, a one of his visits, baptizing her infant. She is markedly different in some o these points from what she is naturally. She begins betimes to utter nonsense, or to talk very volubly, and falls at last into a state of complete delirium, when, unless she be constantly watched, she may make some unhappy attempt on her own or another's life. Or, again, the patient may begin by being low-spirited and dull, refusing her food, and taking no interest in her offspring, or even showing an aversion to it; she takes an unreasonable and unnatural dislike to her husband, her nurse, or her medical attendant: or harbours peculiar suspicions regarding them or regarding her food, fearing that she is to be poisoned or otherwise killed, and gradually settles down into a state of confirmed melancholia.—*Med. Times and Gazette, Nov.* 10, 1860, *p.* 446.

## 104.—ON THE INSTRUMENTS EMPLOYED IN THE OPERATION FOR THE CURE OF VAGINAL FISTULA.

By William Buxton Hilliard, Esq., Instrument Maker to the Glasgow Royal Infirmary.

[The operation for the cure of vesico-vaginal fistula is one which has now been frequently performed, but it is acknowledged to be one of a tedious and troublesome nature, trying the endurance of the sufferer and the patience of the operator. Mr. Hilliard says:]

The first successful operation in this city for the cure of vaginal fistula was performed by Dr. Bozeman, who had had much experience, having frequently operated on similar cases both in this country and America; the operation occupied an hour an a-half, which appeared a longer period than the work executed should require; but it was not a want of skill or dexterity in the operator that protracted the operation, nor was it any untoward occurrence or opposition of the patient that caused the delay. I thought then, as now, that the instruments which Dr. Bozeman employed, though devised expressly for the operation, were not so well adapted for it as could be wished, and that, if other, more suitable, and fewer instruments were used, the operation might be performed in much less time, and probably with better results.

The first cause of delay was attributable to the speculum, which falls far short of satisfying the reasonable demands of the operator; this speculum is usually designated the duck-bill speculum, and is nothing more than a retractor, for it has no dilating power; in employing it to retract the posterior part of the vagina, it draws together the sides of that cavity, and thus affords but a very imperfect view of the fistulous opening, and greatly limits the scope for the surgeon's manipulations. So very defective, indeed, is this speculum, that in most of the operations which I have witnessed, an additional retractor, held by an assistant, had to be employed on each side of the vagina before a proper view could be obtained, or the parts put in position for operating.

In the second place, the operation was protracted by the tedious and uncertain method adopted in paring the edges of the fistula, and by the unnecessary number of instruments made use of in accomplishing that part of the operation. The plan which Bozeman, and some other operators pursue, is to seize the lips of the fistula with a sharp hook, or with a favourite kind of forceps, and then with knives of various shapes and curves, assisted with scissors, endeavour by "cautious steps and slow," to pare the flaps, bit by bit, until the entire circumference of the abnormal opening is supposed to have been traversed by the knife. I say supposed, for the bleeding which occurs obscures the view, and sometimes so confuses the operator that he can rarely say more than that he hopes he has accomplished this all-important part of the procedure. Surely the number of instruments which are usually paraded before the operator, should enable him to give a more encouraging account of the progress of his operation.

The third cause of delay arose from using a metallic plate with a detached perforated pellet to secure each suture; here there was time lost in threading each of these separate articles, and there was a great difficulty in keeping down upon the closed wound the plate and pellets at one uniform tightness, and when time and care were freely bestowed, still it was not unlikely that some of the sutures would be less tense than others, and the operations fail in consequence.

These objections to Bozeman's instruments it may be thought are imaginary, for his operations are generally successful. To this I reply that Bozeman's success is the result of long experience, and that had his instruments been better adapted to the work, his operations would assuredly be less tedious and perhaps even more successful. In proof of the inefficiency of the instruments, every subsequent operator who has published or made remarks on the operation, has had something to urge against them, and improvements to suggest. Thus other kinds of knives and forceps have been

tried, an instrument for twisting the sutures employed, a wire splint for supporting the flaps proposed, a nipple plate instead of the perforated shot, recommended, and best of all a tubular needle invented for the insertion of the metallic sutures. With these improvements I believe the operation is now performed by many surgeons in less time than the first one required. I have frequently seen it executed by Dr. Eben Watson in one-third of the time it then took, and so admirably done that his success, in every case submitted to him, has been hitherto uninterrupted. With further improvements there appears a probability of facilitating the operation yet more, and I have therefore endeavoured to devise a set of instruments, few in number, and of easy application, in the hope of realising so important a desideratum. These instruments may be best described in the order an operator would require them, beginning with the speculum. As a substitute for the duckbill-shaped one, I have designed an instrument of the form here represented, which, as

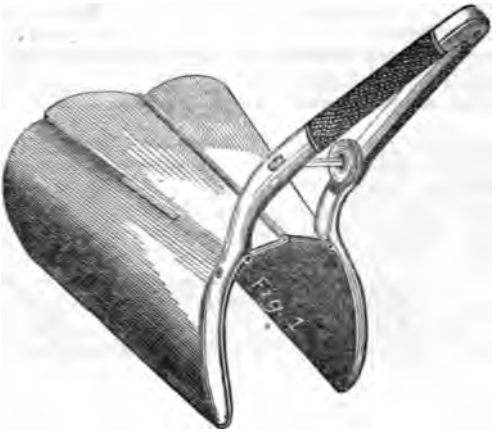



Fig. 1 shows the speculum expanded to its full extent.
Fig. 2 is a full-sized section of the speculum when closed, in which state it is introduced with great facility.

will be seen, is a quadrivalved dilating speculum, and differs from other specula in that it dilates the interior and orifice of the vagina in normal proportions; other dilators either expand in parallel lines, or if they dilate the interior, they contract at the orifice.

This speculum, I submit, is well adapted for vaginal fistula operations; it elevates what may be termed (in the position in which the patient is placed) the roof of the vagina, and at the same time retracts the walls, causing the part in which the fistula is situated to present a smooth horizontal surface, like stretched canvas prepared for elaborate needle-work, affording to the operator a full view of the fistula, and ample scope for his manipulations. It will also be found, in most cases, a self-retaining instrument, requiring no assistant to hold it in position, or to take the charge of it. This quality is the result of the correct adaptation of the speculum to the interior of the vagina. A good view being obtained, the seizing and paring of the lips of the fistula follows. For the performance of this always tedious and difficult part of the work I have devised an instrument which the surgeon, I hope, will readily discover materially facilitates and insures the efficient completion of this portion of the operation. For want of a better name, I must *designate* it a "fistula-clamp," represented in Fig. 3.

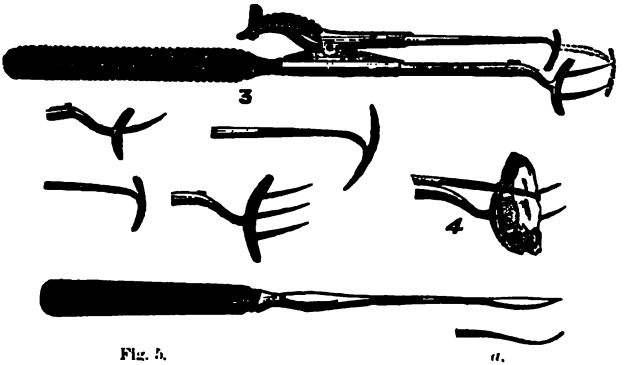



Fig. 5. a.

Fig. 3.—The fistula-clamp armed with points for a medium-sized fistula. Also moveable extremities of the instrument of different sizes to suit every fistula.
Fig. 4 shows the lips of a fistula transfixed and in the process of clamping together.
Fig. 5.—Fistula knife. *a*, showing the curve upon the cutting part.

With the fork-formed point of this instrument the lips of the fistula are transfixed, and firmly clamped together by passing the sliding rod over the points of the fork as they emerge through the posterior lip (see the dotted lines, Fig. 3); then, by raising the clamp, the edges of the fistula, which are grasped by it, are elevated somewhat above the surrounding parts, and with a straight bistoury; or with a slightly curved knife, here figured, can be instantly pared off in one slice; and an examination of the excised portion left within the clamp will show how admirably this hitherto difficult part of the operation is executed with the aid of this instrument. The edges of the fistula being properly pared in this way, union of the parts is more likely to follow, and the operation to be more frequently successful.

The metallic sutures are best inserted with Startin's tubular needle, having the sliding forceps attached to it, recently invented by Mr. Price—a contrivance which enables the surgeon to slide forward the suture-wire when he requires it to project beyond the point of the needle. To this excellent needle I have made another addition, shown in Fig. 6.

Fig. 6.

*b*. *a*. *c*

Fig. 6.—Startin's tubular needle.
*a*, Price's sliding forceps for projecting the suture-wire through the point *c*.
*b*, Hilliard's bobbin, for supplying wire to the needle.

It is a small bobbin affixed to the extremity of the handle, charged with a sufficient length of wire for any number of stitches it may be necessary to insert; with this addition the instrument is a self-feeding needle, and with it the surgeon can apply the sutures in quick succession, without the least assistance from any one.

Fig. 7 is a simple kind of forcep for seizing the suture-wire and drawing out a sufficient length to form the suture; it is also a suitable forcep for compressing the nipples of the plate, to be mentioned hereafter.

The lips of the fistula have next to be brought together, placed in correct apposition, and the sutures secured. For this part of the procedure I have devised the instrument Fig. 8, which is both a depressor of the flaps and a twister of the suture. The lips of the fistula are drawn together by passing the ends of each suture through the small aperture *b*. The edges of the fistula are adjusted and the surface regulated by manipulating a little with the smooth rounded bulb of the depressor, and the suture is securely fastened by

one or two turns of the wheel *a*, which twists the suture to the tightness desired: the operation is now, by some operators, thought completed; but many eminent surgeons, desirous of preventing the secretions from coming in contact with the wound, make use of a metallic plate to cover it. The

Fig. 7.

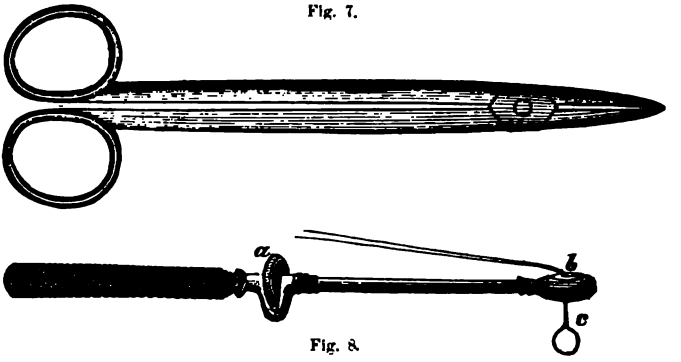
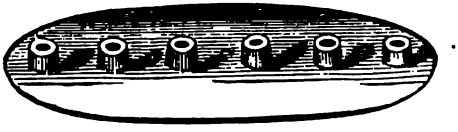



Fig. 8.

Fig. 8.—Metallic suture-twister and fistula depressor. *a*, Small wheel which rotates the twister. *b*, The twister enclosed in the depressor. *c*, The suture-wire formed into a loop by the twister.

plate which Bozeman employed was secured by lead pellets placed against its upper surface, and there held by being firmly compressed upon the suture-wires, but on seeing his operation, I immediately devised the plate (Fig. 9).



which has nipples upon its upper surface, to supply the place of the loose pellets used by Bozeman. This plate is found such an improvement that it has entirely superseded Bozeman's plate and perforated pellets. Mr. Baker Brown, I have observed, prefers my single suture-plate, and applies a separate plate for every suture. In doing this he has thought proper to honour the invention with his name. I beg to say that, if there is any merit in the simple contrivance, I certainly have the sole right to it; for the nippled plate was designed by me, and was used, and a description and figure of it published in the Lancet, by Dr. Eben Watson, several months before Mr. Brown made mention of it. The whole of the instruments figured and described in this paper (with the exception of the tubular needle) I claim to be my own original designs; the bobbin affixed to the needle is also my own idea; they form a complete set for vaginal fistula operations. I trust their utility may be discovered by the eminent surgeons who have shown a deep interest in the operation, and that these observations will be received by them, and by the profession generally, with indulgence.—*Med. Times and Gazette, Nov.* 24, 1860, *p.* 498.

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## 105.—ON THE OPERATION FOR VESICO-VAGINAL FISTULA.

By I. Baker Brown, Esq.

[Mr. Brown continues with his usual success to operate on these obstinate cases. We give the following in order to show that when a woman is suckling, the operation ought not to be performed without some urgent necessity. It often instructs us better to hear of an unsuccessful case than of a successful one, and we duly appreciate the candour of those surgeons who are not *afraid of* publishing such cases.]

Mrs. W——, aged 34, admitted April 18th, 1860, into the London Surgical Home, mother of five children.

*History.*—About three months before her admission she was confined of her last child. The labour was a rather protracted one, and she was attended by a midwife. After the labour she was unable to retain any urine, but gradually improved, and at the time of her admission there was a mere trickling. She was sent to be under the care of Mr. Baker Brown by Mr. Hemming, of Kimbolton.

On examination, there was found a very small fistula at the junction of the urethra with the neck of the bladder, which could hardly be discovered. The opening had originally been much larger, but was now filled up by a very unhealthy loose granulation.

April 26th. The patient being under chloroform, and in the lithotomy position, Mr. Brown performed his usual operation, three bar clamps being used, with iron-wire sutures. She recovered well from the chloroform; but towards the evening unusual sickness came on, which nothing seemed to allay. This continued till the 30th, when she became delirious, and on May 3rd she died, having been insensible for the last twenty-four hours, the cause of her death evidently being pyæmia.

*Remarks.*—As soon as she was dead, I began to inquire into the cause of so unusual a sequence to the operation. I then ascertained that there was milk in the breasts. This greatly surprised me, as she had assured me that she had weaned her baby some weeks before admission, and she had also led my friend Mr. Hemming to the same belief before he sent her to me. Had there been the slightest doubt in my mind on this head, I should never have attempted the operation until every trace of milk had disappeared, because I had long been satisfied on this head by past experience, especially in one case of death from pyæmia which followed an operation for ruptured perineum, where milk was still in the breasts.—*Lancet, Nov.* 17, 1860, *p.* 484.

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## 106.—REMOVAL OF WARTY GROWTHS AROUND THE VAGINA AND ANUS OF A PREGNANT WOMAN.

Case at the Middlesex Hospital under the care of Mr. SHAW.

[Can operations be performed with safety on a pregnant woman? The danger is, the induction of premature labour, and this has been known to result even from a tooth being pulled; but, on the other hand, some of the most important operations have been performed on women in the pregnant state, and no untoward result whatever has happened. The rule should be to perform any operation absolutely necessary, to defer it if not so.]

A pale, sickly and emaciated young married woman, of diminutive appearance, was recently admitted into Middlesex Hospital. She was about six months pregnant, and suffered from a large and prominent mass of warts surrounding the lower part of the vagina, and extending to the anus, which was also completely encircled by them. They had become so agglomerated as to present a tolerably firm epithelial growth. There were in addition numerous small isolated warts on either side of the vulvæ and thighs. All were the result of a former attack of gonorrhœa, existing previously to her pregnant condition. The vaginal outlet was diminished to the size of a few lines, and it seemed to be quite clear, that if she was allowed to go to the full time of utero-gestation, there would be a great risk of serious impediment to labour, in consequence of the state of the parts. Rupture of the perineum, if not some more serious lesion, might be anticipated under the circumstances.

Her case was carefully considered by the staff of the hospital, including Dr. Priestley, the physician-accoucheur, and it was determined to remove all the diseased mass under the influence of chloroform. This was accordingly put into practice on the 28th of November, by Mr. Shaw, who, with the aid of a scalpel and a pair of curved scissors, effectually dissected away

and cut off the various parts of the tumour, and all the smaller warts. This was done without any sacrifice of the integuments, for the whole were found to be superficial, having only involved the epithelial layer of the dermis. The anus and vagina were thus freed, and the outlet of the latter, although still a little constricted resumed nearly its natural dimensions. As might be expected, there was free bleeding during the operation, but it was readily controlled afterwards by a piece of lint steeped in a solution of the perchloride of iron.

Dec. 14th. No constitutional disturbance followed the operation, and a good recovery was made by the patient. The affected parts are sound, except a few warts which were left, as Mr. Shaw was desirous not to protract the operation, or cause her to lose any more blood. She has been out of her bed now for upwards of a week.—*Lancet, Dec.* 29, 1860, *p.* 632.

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## 107.—ON THE SURGICAL TREATMENT OF FIBROUS TUMOURS OF THE UTERUS.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[Mr. Brown, in the following paper, speaks only of uterine fibrous tumours of the non-pedunculated form, growing from the inside of the uterus by a broad base.]

*Case* 1.—*Intra-uterine fibrous tumour, of seven years' duration; operation; cure.*—A. E. E——, aged 35, unmarried, admitted into the London Surgical Home, April 14th, 1859, in a state of great prostration and anæmia, complaining of occasional sickness, pain in the epigastrium, and flooding at intervals of a fortnight, lasting for a week each time. On examination, Mr. Brown found a fibrous tumour, as big as a fist, within the uterus. On the 25th of May, her health being improved, Mr. Brown incised the os uteri, which had the effect of checking the hemorrhage, except at the menstrual periods (which occurred every three weeks.) On Oct. 27th, the patient having been for three months in the country, Mr. Brown gouged a piece out of the tumour. This second operation had the result of breaking up the growth. At every catamenial period she had increased pain, which Mr. Brown ascribed to increased determination of blood to the uterus at these times. This pain and discharge attending the dissolution of the tumour, ceased in Feb. 1860; and on the 21st she was discharged cured. Mr. Brown had heard from this patient that she continues well, takes vigorous exercise, and menstruates regularly both as to time and quantity.

*Case* 2.—*Intra-uterine fibro-cystic tumour; operation; relief.*—C. N.——, aged 30, married, admitted into the London Surgical Home May 19th, 1859. Mother of three children. Had suffered from excessive lochial discharge and frequent hemorrhages since her first confinement, the latter being abated during the time she was suckling, and recurring when she weaned her child. Two years since the birth of the last child she felt a tumour in the abdomen, which has increased in spite of much medical treatment. On admission she was quite anæmic, the abdomen enlarged, and a leucorrhœal discharge from the uterus. Under tonic treatment, &c., the latter disappeared; but she had a severe attack of hemorrhage, continuing in spite of styptic treatment, for nine days.

Oct. 1st. Left the Home better in health, and menstruating regularly.

May 27th, 1860. Re-admitted; extremely weak, and almost bloodless. During her absence from the Home had been pretty well till February, when she lost at one menstrual period three pints of blood; the hemorrhage recurring every eight or nine days till re-admission. Tonic treatment and generous diet.

July 5th. Mr. Brown divided the os uteri; the tumour could be felt just inside it. This operation stopped the hemorrhage, and on the 26th Mr. Brown broke down the tumour with sharp-pointed scissors. This had the *immediate* effect of lessening the tumour, which was passed away in lumps,

with an offensive discharge. She had symptoms of pyæmia in August, which readily succumbed to treatment.

Dec. 10th. She left the Home, the tumour exceedingly diminished, and causing no inconvenience.

Feb. 1861. Patient in good health, stronger daily, and able to perform her usual home duties. Menstrual discharge very moderate. Since this paper was written, Mr. Brown saw her. She had had a fresh attack of hemorrhage, and has since been re-admitted to the Home.

*Case* 3.—*Intra-uterine fibrous tumour; operation; death.*—J. M——, aged 46, unmarried, admitted into the London Surgical Home, Nov. 1859. Has suffered from fibrous tumour twelve years. It is now the size of a six months' fœtal head, causing great uneasiness, and frequent desire to micturate. On examination, the hymen was found almost imperforate; os and cervix normal.

Nov. 19th. Mr. Brown operated in the presence of Dr. Hall Davis and Messrs. C. Mann and P. H. Harper. Mr. Brown divided the os and cervix, and, finding the tumour imbedded in the left side of the uterus as low as the os internum, he cut through the capsule, gouged a piece out, and broke down its tissue. Little hemorrhage.

20th. Rigors occurred, pyæmia set in and progressed, pus was effused into the pleura, and the patient died on the tenth day after the operation.

Post-mortem examination showed diffused purulent infiltration. Concerning this case, Mr. Brown remarked that the fatal termination was due to having gouged the tumour at the same time that he incised the os, and in addition to having broken down the hymen. He had determined in future to divide the operation into two stages.

*Case* 4.—*Intra-uterine fibrous tumour; operation; cure.*—E. B——, aged 41, married, no children, admitted into the London Surgical Home, Feb. 18th, 1860. Had suffered for four years from hemorrhage and consequent debility. On examination, a fibrous tumour was diagnosed.

Feb. 27th. The os uteri incised.

May 7th. The patient having improved in health, which had been indifferent since the last operation, the tumour was freely incised in its centre, and the vagina as usual plugged with oiled lint. There was an offensive sanguineous discharge for two or three weeks; the tumour gradually diminishing.

June 20th. Catamenia appeared in nearly normal quantity.

July 7th. Left the Home.

Mr. Brown had since heard from this patient. She can take exercise without pain or annoyance, and has no return of the hemorrhage.

*Case* 5.—Mrs. M——, admitted into the London Surgical Home Nov. 16th, 1860, having been sent to Mr. Brown by Messrs. Alfred Cooper and Paget, of Leicester. Is a widow; had been married eight years and a-half; without children. Has lost much blood at each menstrual period during the last five years, but only discovered an enlargement of the womb two years ago. This had increased rapidly during the last few months, and she had suffered from a white watery discharge after each period.

On examination the uterus was found completely retroverted, the os being so close to the pubes that it could not be reached. Within the uterus a fibrous tumour could be felt as large as a fœtus at the fourth month. There was also distinct fluctuation at the posterior wall of the uterus, arising from accumulation of the menstrual fluid. This showed that the tumour was situated on the anterior wall of the uterus.

Nov. 29th. Mr. Brown operated in presence of Mr. Paget, Mr. Alfred Cooper, and many other gentlemen. Finding that the os could not be reached, he made an incision in the posterior wall of the uterus, which was very thin, letting out a quantity of fœtid sanguineous fluid. The incision was carefully carried up to the os and towards the fundus, taking care to avoid wounding the peritoneum; and the fibrous mass could now be seen and felt.

Dec. 29th. The tumour was freely gouged, and consequently greatly diminished.

Jan. 24th, 1861. The tumour was again gouged, as it had not disappeared rapidly enough. The tumour decreased to half its size, there being much muco-purulent discharge.

Feb. 7th. Mr. Brown again gouged the tumour, the same results following the operation. He had no doubt the tumour would eventually under treatment disappear entirely.

*Case* 6.—*Three fibrous tumours; operation; cure.*—E. P——, aged thirty-seven, unmarried. Seven years before, Mr. Brown had removed a fibrous growth from the uterus about the size of a walnut. Two years ago the patient complained of uterine pains and hemorrhage. A year since, in consultation with Dr. Ferguson, three fibrous tumours had been diagnosed, growing from the inside of the uterus, near to and on each side of the os internum.

Jan. 15th, 1861. Mr. Brown, assisted by Dr. Cockle and Messrs. Philip Harper and Wratislaw, cut through each of the tumours. A muco-purulent discharge ensued.

Feb. 15th. On examination two of the three tumours had quite disappeared, and the third was only half its former size. The patient has not suffered from hemorrhage since the operation.

*Remarks.*—The cases related, having been selected from the case-books of the institution with which Mr. Brown was connected, had consequently been under the observation of other practitioners during the whole course of treatment pursued. Other examples might have been adduced from his private practice, but those related showed—

1st. That this class of tumours may be destroyed by cutting into and destroying a portion of their tissue; in other words, by gouging a portion out.

2nd. That this operation could be performed successfully and safely. Out of the dozen or more cases in which this plan had been pursued, Mr. Brown had lost but one, that related in the present paper.

3rd. That the preliminary division of the os and cervix has in itself the very important advantage of nearly, or quite arresting the uterine hemorrhage, the most serious symptom attendant on these tumours.

Mr. Brown concluded by adding a few observations on what had been done for these tumours by Dr. Atlee and others, and stated that the principal difference between his plan and that of Dr. Atlee was, that whereas the latter gentleman makes an incision through the capsule of the tumour, with a view to its detachment, and to second this purpose gives ergot of rye, Mr. Brown aims at the disintegration of the tumour, by cutting into its centre, and breaking it down, thus setting up a process of ulceration, but making no attempt to separate the tumour bodily from the uterine wall.

Immediately after the paper had been read, Mr. Brown showed the instruments which he had been in the habit of using in the operation described in this paper. They very much resembled an ordinary centre-bit, twisted in a spiral manner, with various cutting ends. He had experienced great difficulty in getting those instruments to hold, so as to gouge a piece quite out, and had thought something was required to hook the tumour while it was being cut. This idea had been very ably carried out by his friend and colleague, Mr. Philip Harper. This gentleman's instrument consisted of a hollow tube of steel, with cutting knives. Contained in this tube was a hook, which could be pushed through by a spring, and thus grasp the tumour, whilst the circular knives were carried through it by means of a screw. In this way a piece was actually cut out of the tumour. The instrument was graduated, so that by measuring the tumour from without, a pretty correct estimate could be formed as to how far the tumour was cut into.—*Lancet*, *March* 16, 1861, *p.* 266.

## 108.—MR. PHILIP HARPER'S INSTRUMENTS FOR CORING FIBROUS TUMOURS OF THE UTERUS.

The instruments shown in the annexed engraving are those mentioned by Mr. Baker Brown as having been invented by Mr. Philip Harper, for the purpose of gouging out pieces from fibrous tumours of the uterus. [See preceding article.]

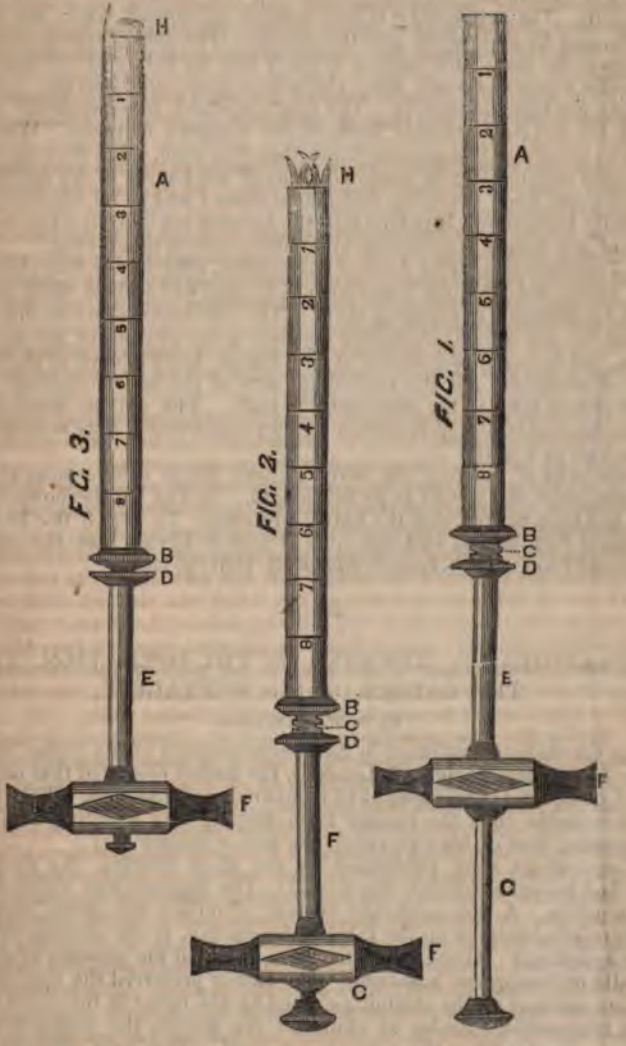


Fig. 1 shows the instrument as it appears before introduction against the tumour. It consists of a hollow tube of steel nine inches in length, graduated at intervals of an inch, so as to show the depth to which the tumour is penetrated. It is open at its upper extremity, but closed at the lower by a moveable nut B. Through the centre of this nut passes a hollow rod of iron, E, having at one end a handle, F, and at the other two knives, either straight,

as at H, Fig. 2, or one straight and the other bent at right angles, as at H Fig. 3. At a point on this rod, ten inches from the points of the knives, is placed a male screw, C, working through a female screw in the centre of the nut B. Passing through the tube E is a rod of steel having at its upper end a double hook, as shown at H in Fig. 2. Connected with this rod also is a strong spring, contained in the interior of the instrument, and which therefore cannot be shown.

The mode of its action is as follows:—The instrument in the state shown in Fig. 1 is held in the left hand, passed up the vagina, and placed against the tumour. The screw rod G is then pushed and turned, when the hooks H protrude, and fix themselves into the tumour. The spring acting upon them, draws upon the mass, and tends to bring it into the hollow of the instrument. The handle F is now turned, when the knives gradually protrude, and at the same time cut into the tumour. The depth to which they penetrate is regulated by the nut D, previously fixed at the required distance. When it is desired to penetrate to a greater depth than one inch, after the nuts D and B are brought into contact by repeated turns of the handle F, the movement is still continued, when the whole instrument turns; and as its upper extremity is carefully beveled off, it passes into the incision made by the knives. Thus a piece of any length from one to nine inches can be taken away.

Fig. 3 represents another form of instrument, adapted for that class of tumours which are brought into view easily, and when, from their position, there is no difficulty in reaching them. The difference between this instrument and the one already described consists in the absence of the hooks and rod G H, and in one of the blades of the knives being placed at right angles instead of straight. All the remaining parts of the instruments are similar, and the letters refer to the same parts in each. The curved blade is made very strong, and is placed in such a cutting angle that it acts somewhat as a hook as well as a knife.

For the sake of showing the screw C, each figure differs somewhat. In Fig. 1 the screw is exposed to its full length. In Fig. 2 it is shown after the handle has been turned, and the knives protrude. In Fig. 3 the knives are out to their full length, and the nuts B and D having met, the screw has disappeared from view.—*Lancet, March* 23, 1861, *p.* 292.

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## 109.—OVARIOTOMY; REMARKS ON THE OPERATION, AND ON THE CAUSES OF ITS MORTALITY.

By Dr. W. Tyler Smith.

[Among the statistics adduced in the commencement of his paper (read before the Obstetrical Society of London), the author observed that out of 395 cases of completed ovariotomy, 212 recovered, and 183 died. This is much more favourable than the results from tapping and injecting with iodine. After relating four of his own cases, the author remarked:]

The operations were performed by the short incision, the cysts being tapped and brought through the wound. The ligature was allowed to fall into the pelvis. A very moderate amount of opium was given, and stimulants were administered on the third or fourth day, in the absence of inflammatory symptoms. Great care was taken to guard the patients against any miasmatic or contagious influence. The author preferred the ligature to the clamp, on account of the straining caused by the latter in the event of vomiting or tympanitis. As far as statistics are known, the proportion of recoveries is greater when the clamp is not used. The only objection to the ligature is the length of time before it comes away. The following remarks on the causes of mortality, and the means of diminishing it concluded the paper:—

"Nearly 17 per cent. of the fatal cases have occurred from shock or collapse. This source of mortality should be met by resorting to the operation *before* the patient has reached a state of exhaustion. It is not in many cases

decided on till the patient is in such a state of weakness as to be unable to bear this or any other operation. Except in cases of extensive adhesion, there is nothing necessarily belonging to the operation which should produce dangerous shock. This source of danger will be diminished when the operation becomes generally recognised as an established and necessary proceeding in suitable cases. Improvements in diagnosis will doubtless lessen the number of cases in which the abdomen is opened without finding ovarian tumours. Earlier operation would also lessen the number of cases in which adhesions occur.

"Exactly 16 per cent. have died from hemorrhage. All or nearly all of these may be prevented by care in trying the pedicle, and in securing hemorrhage from the separation of adhesions. The ligature should never be left until we are quite sure that all bleeding has ceased. One source of the bleeding has been the use of the ligature without transfixing the pedicle. There is a notable increase of mortality where the pedicle is tied in one mass, as compared with transfixion and tying in two or more portions.

"Peritonitis is by far the most fatal complication which can occur. Not less than 43 per cent. of the fatal cases have died in this way. Looking to the histories of the cases thus lost, it can, I believe, be shown that peritonitis does not depend so much on anything inherent in the operation itself, as upon miasmatic or contagious influences. This seems proved by the great mortality after the operation in hospitals as compared with private practice. It is difficult to get a successful result in a large hospital, yet the operations are nowhere more skilfully performed, nor the after treatment better. Peritonitis following ovariotomy is evidently in the majority of cases not simple inflammatory peritonitis, but a disease closely resembling puerperal fever, and like it commonly due to some external poison. This is the only way in which the excessive mortality after the operation in hospitals in this country and on the continent can be explained. The ovariotomy patient is as susceptible as the puerperal woman, or even more so. Such cases cannot safely be collected together or mixed with other patients; and in the long run, I believe the results cannot be as favourable either in general or special hospitals as under other circumstances. In the operations detailed every possible care was taken that they should be performed under good sanitary conditions; that as few persons saw the patients or were present at the operations as could well be; and that nothing came near them in the shape of contagion or infection. Each patient was treated, in fact, as we should treat and guard a lying-in woman, and to this, more than to anything else, I attribute their success. By care of this kind we may probably diminish this the greatest source of mortality to a considerable extent. The three causes which I have dwelt upon—namely shock, hemorrhage, and peritonitis—represent upwards of 75 per cent. of the total deaths after ovariotomy."

Mr. Baker Brown congratulated the author on his great success, and also on his having become a convert to ovariotomy, and hoped that many other hospital physicians and surgeons, who had hitherto opposed the operation, would follow his example. He (Mr. Brown) observed that the author had only mentioned two plans of treatment—namely, tapping simply, and tapping with injection of iodine. Besides that of complete extirpation, there was, however, another which he (Mr. Brown) had for many years advocated, and which had received the sanction of many members of the profession—namely, tapping and pressure. This mode of treatment was only applicable to monocystic forms of the disease. He could mention many cases of several years' standing, which had been cured by this treatment. In one particularly, where the lady afterwards married, and has since been delivered by him of four healthy children. In the last year he cured three patients in the London Surgical Home, and there was another, apparently cured, still under observation. He thought that the author had not fairly stated the advantages of tapping with injection of iodine, which in unilocular cysts had been often used with advantage. He had himself reported some cases of cure, as also did Mr. Spencer Wells about two years ago. As regards the administration of opium after the operation, he (Mr. Brown) had, as the au-

thor observed, been in the habit of giving opium in all cases, as had also been done by Dr. Clay, of Manchester; but latterly he (Mr. Brown) had been guided in this matter by the pain, proportioning the amount of opium to the severity of the pain, which he regarded as a physiological evil to be removed if possible. He generally gave it by the rectum, as in many cases it does not cause the same amount of sickness as if given by the mouth. He then alluded to the length of time the ligatures had remained in the abdomen: in one case seven, in another case eleven weeks. He thought that this must have arisen from one of two causes: either that they were not tied tight enough, or that too much of the peduncle had been taken up by each ligature. The average time in his experience, and in that of Mr. Lane, had been from nine to ten days. He (Mr. Brown) could not agree with the author in his exclusive use of the ligature, allowing the peduncle to drop into the abdomen. The practice that he adopted was, to use the ligature where the peduncle was very short, and could not be kept outside the opening without dragging too much upon the uterus; on the contrary, where the peduncle was long enough, he preferred securing it by clamp or otherwise on the outside, as the wound healed quicker and the patient was sooner convalescent. The objection to the weight of the clamp was obviated by using a pair of common carpenter's calipers. These could generally be removed on the second or third day. In two cases lately, in which he had operated successfully in the London Surgical Home, he had removed them on the third day. In reference to the sutures, he thought that when the author had tried the silver wire sutures, he would find them far better than the silk; they were more readily applied, and three or four deep ones were sufficient without the interrupted sutures. He thought that the success of the author was due in a great measure to the fact that none of his cases had been tapped, except one, and that only once, as he believed that the best cases for operation were those which had not existed long, and in which the abdomen had not been disturbed by repeated tappings, and the constitution drained by the constant refilling of the cyst. He thought that the paper of the author would do much good towards a fairer trial of ovariotomy, and that the difficulties which the old operators had hitherto had to encounter amongst the great majority of the profession would be materially lessened by the addition of such an authority.

Mr. SPENCER WELLS said he concurred with Mr. Brown in the opinion that the author had rather undervalued iodine injections in cases of single cysts. He (Mr. Wells) had employed iodine in five such cases, and four of the patients had remained without refilling. In the fifth, the injected sac had either refilled, or others had grown. In compound cysts he quite agreed with the author in regarding iodine as worse than useless. He differed entirely from Mr. Brown in the opinion that pressure was of use after tapping. Doubtless some single cysts had remained without refilling after this treatment; but tapping alone was quite as useful without as with pressure. The pressure could have no effect on an empty collapsed cyst. In two cases in which he (Mr. Wells) had been prepared to inject iodine he had trusted to simple tapping, because, in each case, he found the fluid so clear and limpid that he believed the cyst was not truly ovarian, but one of those thin-walled, simple cysts of the broad ligament which collapse when emptied. and do not fill again, whether pressure be employed or not. As to the position of the patient during ovariotomy, he had tried, in three cases, the sitting posture as recommended by the author; but he very much preferred the recumbent position. The chloroform was less likely to induce faintness; there was less tendency to escape of intestines, greater facility in securing the peduncle or bleeding vessels, and it was not difficult, by holding the cysts well forward as they are emptied, to prevent the ovarian fluid from entering the peritoneal cavity. He quite agreed with what the author said as to the extent of incision; and partly so as to the mode of securing the peduncle. After using the clamp in four or five cases, he had abandoned it, *except* as a very convenient and rapid mode of securing the peduncle for a *few minutes* while the tumour is separated and the wound closed. Ligatures

may then be used below it, and it may be at once removed. In the two cases of tetanus, he did not believe the clamp had anything to do with this occurrence. In one case the tetanic symptoms did not appear till the fourteenth, and in the other till the eighth day after operation; yet the clamps came away on the third or fourth day. The objection to the fixing the peduncle to the abdominal parietes suggested by the author, that increase of the uterus during pregnancy might be interfered with, was not borne out by a case in which a patient upon whom he had so fixed the peduncle had borne a living child thirteen months after ovariotomy. He differed very strongly from the author in his practice of leaving the tied stump of the peduncle within the peritoneal cavity. He (Mr. Wells) had been obliged to do this in two cases out of the twenty-one in which he had operated. One of these patients recovered, but her state was very alarming for some days, and convalescence much more protracted than in any other he had seen. The other died thirty hours after operation with all the symptoms of putrid infection, and he had no doubt such putrid matter as may be seen to come away when the peduncle is kept outside the wound was absorbed from the peritoneal cavity, and poisoned this lady. The length of time occupied by the separation of the ligature when it was left inside, was a great source of anxiety to both patient and surgeon. In some recorded cases, it had remained from thirty to forty days; whereas, when fixed outside, it separated in three or four, or at most eight or ten days, and then the wound healed, and all anxiety ceased. He thought the profession greatly indebted to the author for bringing forward these cases, as they would strengthen the hands of those who were endeavouring to prove that ovariotomy has at least been as successful as several other important operations, and a means of restoring many patients from a state of hopeless suffering to one of perfect health.—*Lancet*, *Feb.* 16, 1861, *p.* 166.

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## 110.—REMOVAL OF OVARIAN TUMOUR.

[At a meeting of the Obstetrical Society of London lately, Dr. TANNER exhibited an ovarian tumour weighing about 8lbs, which had been successfully removed eleven days before, the patient doing well.]

With regard to the operation itself, there was one point deserving of mention,—namely, that after applying ligatures to the pedicle, the tumour, instead of the pedicle, was cut through, and a portion about the size of a small hand left attached, which was retained outside the abdomen. By this proceeding the use of the clamp was rendered unnecessary, while all risk of hemorrhage was avoided. The latter point was of no little importance, since bleeding from the pedicle had been the cause of death in no less than 16 per cent. of the fatal cases. The edges of the wound were carefully brought together with twine sutures; these latter not being passed through the peritoneum.

In reply to a question from Dr. Tanner,

Mr. SPENCER WELLS said he had attached the greatest possible importance to the practice of passing the metallic sutures or hare-lip pins through the peritoneal edges of the wound. He had been led to originate this practice by observing the inner aspect of the wound in the first fatal case of ovariotomy which had occurred in his practice. The sutures had been passed through integuments and muscle, but not through the peritoneum; and the consequence was, that a raw surface of considerable breadth was only partially protected from the general peritoneal cavity by a portion of intestine, which was already adhering to it, although the patient died the day after the operation. Had she lived longer, there could be no doubt that pus or other secretion from the wounded surface would have been poured into the cavity of the peritoneum. Struck by this observation he (Mr. Wells) made a number of experiments, in concert with Dr. Richardson (who narcotised the animals by puff-ball smoke or chloroform), upon guinea-pigs, rabbits and dogs, opening the peritoneum and closing the wound in some cases by sutures which included the membrane, and others which only passed near it, and kill-

ing the animals at various periods after the operations, which were made as accurately comparative as possible. The result was that nearly all the animals recovered; but in those where the more superficial sutures were used there was a portion of the parietes left destitute of the peritoneum, and this defect was supplied by adhesion of intestine or of omentum. In two cases where omentum became adherent, the motions of the dogs were materially interfered with. In those cases where the peritoneal edges of the wound had been included, the union was so perfect that it was difficult to detect the line of union two or three weeks after the operation. This was explained by the observation of a case in which a women died within two days after ovariotomy. The hare-lip pins which had passed through the peritoneal edges were quite concealed from view and protected from contact with intestine, partly by folding of the peritoneal edges together, and partly by effused lymph. Mr. Wells said he had preserved many of the most striking specimens, and would be happy to show them to any gentleman who was interested in the subject.

Dr. TYLER SMITH had recently attended a case which strongly exemplified the good effects of bringing the edges of the peritoneum together by the sutures. After union of the external wound and the removal of the sutures, suppuration took place along the whole line of incision, extending through the entire thickness of the abdominal wall. The peritoneum had, however, healed so perfectly that no pus found its way into the peritoneal cavity. In this case, silk sutures had been employed. The patient recovered, but this would have been hardly possible unless the edges of the peritoneum had been brought together, so as to produce union by the first intention. It was unnecessary to pierce the edges of the peritoneum to effect this. It was sufficient to take up the tissue immediately above the peritoneum at the edge of the incision, without passing the needle through the peritoneum itself.

Mr. Spencer Wells said that when metallic sutures were used, and the deep ones were removed on the third day, he had never seen any suppuration in their track.—*Lancet, Dec.* 15, 1860, *p.* 585.

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## 111.—SUCCESSFUL CASE OF OVARIOTOMY.

By Dr. D. LLOYD ROBERTS, St. Mary's Hospital for Diseases of Women and Children, Manchester.

[The patient was 35 years of age, married, and had had three children. The tumour had been noticed fourteen months, it was distinctly moveable in the abdomen, the patient supporting it with her hands when she turned in bed. Before the operation was performed the tumour was twice tapped. Chloroform being administered,]

The operation was commenced by making an incision three or four inches long, from one inch below the umbilicus downwards. The peritoneum was in this manner exposed, and then divided on Key's director: some ascitic fluid now escaped, but not much, as she had been tapped only fourteen days before. The peritoneum covering the tumour was next carefully divided on the director, and a large cyst came into view. This was opened by Mr. Spencer Wells' large trocar, and about 10 pints of thick mucilaginous fluid were rapidly drawn off. It was now perceived that the remaining, and by far the larger portion of the tumour was solid, only a few small cysts being scattered here and there on its surface. I passed my hand carefully round the tumour, and found that there were no adhesions.

My friends, Dr. Radford, Dr. Stephens. and Mr. Lynch, the two latter of whom were kindly officiating as my assistants, here suggested that the incision should be enlarged, as it seemed altogether insufficient for the passage of the tumour; accordingly, it was extended above and below, to the length of nine inches. Through this opening I drew the tumour, which was then firmly held by Dr. Stephens and Mr. Lynch. The pedicle, about four inches *long*, and an inch and a half broad, was tied by being transfixed twice by *strong* ligatures, and was then divided close to the tumour. The surface of the

peritoneal cavity was now carefully cleared, by means of a sponge, of some fluid and clotted blood which adhered to it; the left ovary and the uterus were next examined, and found healthy; and the wound was carefully closed by means of four silver hare-lip pins, passed through the divided edges of the abdominal parietes, including the peritoneum, and the adaptation of the edges still further secured by superficial sutures of silver, which were placed in the intervals between the pins. The pedicle was brought outside, and secured by passing a superficial suture through it and the integuments, to prevent its receding into the abdominal cavity. Strips of plaster were used to steady the ligature, and the skin was also protected from the edges of the pins in the same manner. Strips of plaster extended across the abdomen, and the application of a pad of cotton-wool and a flannel binder, completed the dressing. The patient was put to bed, expressing herself as very comfortable, and "wishing to know if the tumour had been removed."

The tumour, exclusive of the contents of the cyst, which had been tapped, weighed sixteen pounds: the weight of the fluid contents of the cyst was twelve pounds; making altogether twenty-eight pounds, as the weight of the tumour in its entire state. It was a good specimen of the pseudo-colloid variety of ovarian tumour, so well described by Spencer Wells. An incision into it showed it to be made up of a number of chambers, varying in size from a filbert to that of a clenched hand, and filled with two different kinds of fluid; one kind presenting the appearance and consistency of very thick mucilage, the other resembling prepared chocolate; the parietes of each chamber consisted of a layer of firm fibrous tissue.

I made a careful microscopic examination of the fluid, but could find none of the morbid cells which are characteristic of malignant growth, but only a number of non-nucleated cells, aggregated together, so as to form corpuscles three or four times the size of blood-discs, and having the appearance of fatty granules.

An hour after the operation, the abdomen was covered with a linseed-meal poultice. At 5 p. m., after four hours' sleep, the patient awoke, and expressed herself comfortable, but fatigued, and rather sick.

In the evening she vomited a little, the effect probably of the chloroform; at 11 p. m., a morphia suppository was introduced into the rectum; pulse, 104; skin, moist. She passed a good night.

[The progress of the patient was satisfactory, and the last report states that she had much recovered her health and strength.]

I believe that this poor woman owes the preservation of her life to an adherence to the following principles, laid down by Mr. Spencer Wells, viz.:—

First. In making an incision the smallest possible compatible with the passage of the tumour. I may here remark, that all the cases I have seen in which the large incision, "from sternum to pubes," was used, proved fatal.

Second. In bringing and securing the pedicle, external to the incision, instead of leaving it to slough (as was the practice heretofore), within the abdominal cavity. It has fallen to my lot, on more than one occasion, to witness the death of patients about the third week after ovariotomy; the result, as there is the strongest possible reason to believe, of pyæmia,—the abdomen, on a post-mortem examination, having been found filled with pus, which could be traced to no other cause than the sloughing pedicle.

Third. In the use of silver pins passed through the abdominal parietes, so as to include the peritoneum, thus securing union, by first intention, of the deep parts of the wound, including the divided edges of the peritoneum; and so, by closing the peritoneal cavity, securing it against the entrance of pus, which might otherwise fall into it from any superficial suppurating portions of the wound.

It is my opinion, also, that part of the successful result is due to *the very free* administration of nutriment and stimulants, "per rectum," when the patient was unable or unwilling to swallow them, and at a time when death appeared imminent from the want of them.

I would draw the attention of my professional brethren to the remarkable extent which nutriment and stimulants are capable of being absorbed into the system when administered by this channel.

Great relief was also afforded by the substitution of a light chamomile-bag, covered with oiled silk, instead of the heavy linseed poultice; the latter having occasioned the patient much discomfort by its weight. I have already mentioned that the opiate injection was the only remedy which was successful in checking the obstinate vomiting.—*Dublin Quarterly Journal, Feb.* 1861, *p.* 43.

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## 112.—SPONTANEOUS CURE OF OVARIAN TUMOUR (?).

By John Fox, Esq., Weymouth.

[The patient, who was 22 years of age, had perceived a steady increase in her size for two years, no pain experienced. There was evidence of the presence of a large quantity of fluid.]

She gradually increased in size, until it was evident that there could not be less than from thirty to forty pints of fluid. On the morning of the 25th July, she suddenly felt considerable pain about mid way between the end of the sternum and umbilicus, which she described "as if a person's hand were inside twisting the parts." She then became rather faint and weak. These symptoms soon subsided, and she afterwards took a good deal of exercise, and in the evening took her part in a dance with a few young friends. Suddenly, an urgent desire to empty the bladder was felt, and she passed at least two quarts of fluid, and at the end of about two hours a similar quantity, and so on greater or less quantities until the following Saturday morning (July 28th). She then found all trace of enlargement gone, and the integuments of the abdomen lying in loose folds, as they would do after tapping. I then saw her again, and found her weak but cheerful, and free from pain. She assured me that not a drop of urine had passed involuntarily; that she had perfect control over her bladder, and that the whole of the fluid was limped and almost odourless. After a careful examination of the abdomen, I could not detect the slightest trace of tumour or even of thickening, and the vagina and uterus appeared to be perfectly healthy. I ordered her the tincture of sesquichloride of iron, and a generous diet with wine, and after a few weeks sent her to see Dr. Lee again. He also failed to detect any tumour or lesion of any kind, after a most careful external and internal examination, and pronounced her to be quite well.

*Remarks.*—This case suggests several questions. Could it have been an ovarian dropsy finding its way through the Fallopian tube and uterus? I think not; as during the intervals of passing urine she was sure that not a single drop had passed from her. Was the tumour ovarian? and did the cyst give way at the time when she felt the pain and faintness, thus converting the case into one of ascites? If so, could the whole of the fluid have been taken up by the absorbents, and conveyed through the kidneys into the bladder in the short space of between fifty and sixty hours? I think this the most probable explanation; and it agrees with the description of a case mentioned by Dr. Blundell, in which a lady affected with ovarian dropsy had a large discharge from the bladder after striking her abdomen against a stone. When she died, some years afterwards, with retroversion of the uterus, it was found that an ovarian cyst had burst into the peritoneal cavity, and that its contents had been absorbed. Also with a case, No. 20, related by Mr. Beaumont, of Gravesend, in which the ovarian dropsy disappeared after rupture of sac, and passed off by kidneys and bladder. (See Clinical Memoirs on Abdominal Tumours, by the late Dr. Bright, page 121.) Whilst Kiwisch, in his chapter on Diseases of the Ovaries, translated by John Clay, mentions rupture of the cyst into the peritoneum, followed by profuse diuresis; and Churchill refers to rupture through the bladder. The patient now wears a belt, and continues in the enjoyment of good health. She has gained flesh and strength, and is able to walk several miles daily.—*Brit. Med. Journal, Dec.* 22, 1860, *p.* 996.

## 113.—CASE OF OVARIAN DROPSY TREATED BY THE INJECTION OF IODINE.— SUBSEQUENT EVACUATION INTO THE RECTUM.

Under the care of Dr. DAVIES and Mr. CURLING, at the London Hospital.

Mrs. W., aged 25, was originally admitted under care, at the London Hospital, in July 1858. She was of fair complexion and well-formed. Her disease, which was ovarian dropsy, was believed to have commenced in the summer of 1856. She had previously borne one child. The first ovarian enlargement was mistaken for pregnancy, menstruation being for a time suspended. The enlargement of the eyes was gradual, and at the time of her admission, two years from its commencement, the distension was somewhat more than that of the full period of pregnancy. Her health was a good deal undermined.

July 20, 1858. Having been in the hospital a few days, paracentesis was performed, and fifteen pints of a greenish-yellow fluid were withdrawn. After the tapping the topical application of iodine was tried and tonics were given. She left the hospital in ten days.

Having remained at home about nine months, Mrs. W. was admitted for a second time in April 12, 1859, being now much larger than ever. Paracentesis was performed on the 14th, and thirty-five pints of brown fluid were withdrawn; the fluid on this occasion was thicker than on the previous one, but not much so. She recovered well from the operation, the cyst, however, commencing to refill almost immediately. She left the hospital on the 28th.

After the second tapping, her husband, who had been in India, returned home; and as shortly afterwards the catamenia ceased, she supposed herself pregnant. Her abdomen enlarged very considerably. While in this condition she had a fall and struck her abdomen on a plank, after which she became suddenly very faint and ill. The accident happened in the country, and the surgeon who was called in performed paracentesis and withdrew thirty-nine pints of fluid.

During July the woman was again in the London Hospital, but she declined on this occasion to submit to another tapping. In August she was readmitted, her condition having become unbearable. A fourth paracentesis was performed, and thirty-eight pints of dark-brown fluid were removed. After this tapping she remained in a very low state for some time, and left the hospital at her own request, apparently about to sink under her disease. She remained at home six weeks, being confined to bed the whole time, but having been somewhat improved in general strength.

August 27th, re-admitted. She was now in a very feeble state, unable to stand, her legs swollen and painful, countenance swollen and anxious. Face thin and drawn. Her abdomen was very large indeed. It was now determined in consultation that treatment by the injection of tincture of iodine should be tried. On October 18 Mr. Curling accordingly tapped the cyst for the fifth time. Forty-five pints of thick brown fluid having been withdrawn, and the cyst having apparently been emptied, a quantity of the pharmacopœial tincture of iodine was injected. After the injection she passed a good night, and did not suffer very much pain. On the next day she said she could taste the iodine. During the following week the urine was frequently examined, and found to contain iodine. The cyst rapidly re-filled, and on the 20th she was almost as large as ever. On the 27th Mr. Curling again performed paracentesis, removing twenty-seven pints. A solution consisting of a drachm of iodine, a drachm and a-half of the iodide of potassium, and three ounces of water, were injected, and allowed to remain in. About eight hours after the injection, she complained of a burning pain in the lower part of the abdomen, but no severe symptoms followed. During the next fortnight she rather improved in general condition, but the abdomen rapidly increased. On the 17th, whilst sitting on the close stool, she felt what she described as "something hard pressing into the rectum," and the sensation was followed by a snap and a gush of fluid. Twenty-seven pints of thick light-coloured fluid, like gruel, escaped per anum. She

became faint, and it was necessary to give brandy, after which she quickly rallied. On the day following about a pint and a-half were passed. On the 20th about a pint escaped, and during the following week the quantity discharged varied from one to two pints daily. She improved in health slightly, but towards the middle of December a tumour was discovered in the region of the left ovary. From this time she gradually sank, the discharge per anum steadily continuing. Death took place on December 25, eighteen months after the first tapping, and three years and a half from the date of the commencement of her disease. Unfortunately no post-mortem examination was permitted.—*Med. Times and Gazette, Feb. 9*, 1861, *p.* 143.

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114.—*Treatment of Vaginitis.*—By Dr. E. J. TILT, Consulting-surgeon to to the Farringdon Dispensary and Lying-in Charity.—Whether vaginitis occurs spontaneously or as the result of uterine catarrh, it is best cured by the injection of a solution of nitrate of silver. This is an excellent idea of Dr. Jewel, but if the solution be sufficiently strong to do good it cannot be safely trusted to the patient. The patient being placed on her back, a small glass speculum should be introduced as far as possible, and an ordinary glass syringe full of a solution of nitrate of silver, containing forty grains to the ounce, should be injected. The speculum should then be withdrawn to the vicinity of the vulva, and the fluid should be left in contact for three or five minutes, after which the speculum is to be withdrawn, and the fluid received in a small cup. Sometimes I apply a speculum of appropriate size, and as I withdraw it I pretty freely touch the vagina with the solid nitrate of silver diluted by chloride of silver, as prepared by Mr. Squire. This is a modification of a plan recommended by Ricord.—*Lancet, Feb.* 23, 1861, *p.* 184.

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115.—*Treatment of Follicular Inflammation of the Labia.* By Dr. E. J. TILT.—In follicular inflammation of the labia, in eczema and prurigo pudendum, or pruritus, both external and vaginal, a piece of cotton wool should be soaked in the solution of nitrate of silver, and carefully rubbed for two or three minutes over the diseased portions of the skin and mucuous membrane. I can speak with confidence of this plan, for I have lately cured several patients who had been suffering in this way for four, eight, and thirty years. When cases have lasted so long the pudendal skin looks and feels like parchment. It was so in the case of a lady in whom the disease had lasted thirty years, and I first rubbed in the solution every day, then every other day, then every fourth and fifth day, until the skin become soft and pliable, and the sleep was no longer disturbed by darts of pain flashing along the nerves. This patient was cured in three months, and has had no relapse during the year.—*Lancet, Feb.* 23, 1861, *p.* 184.

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116.—*On Affections of the Hip-Joint in consequence of Uterine Disease.* (Abstract of a paper by Dr. HOPPE.)—The author reports two cases of women, one unmarried and forty-five years of age, one a widow, thirty-six years old, both of whom had been suffering for a length of time from a disease of the womb, and an affection of the hip-joint. The principal symptoms relating to the latter are:—1. Pains which occupy the whole of the thigh, especially in its anterior and exterior aspect, or only in the circumference of the trochanter: they are very intense, lasting through day and night with very short intermissions; 2. Contraction of the adductor muscles, and of some of the flexors. Both symptoms are referable not so much to an affection of the joint itself, as to an irritation in its neighbourhood; this becomes more likely by the fact that there exists no deformity about the nates.

As a further proof of the statement that the named affection was owing to a disease of the womb, Dr. H. remarks that he has frequently observed painful sensations around the trochanter, in connection with uterine disease. These patients complained of a sensation of pressure and burning in the

region of the trochanters, which diminished gradually, and finally disappeared under a treatment adapted to the disease of the womb. The author points to a fact, more generally known, viz., the occurrence of gonalgia as a consequence of morbid affections of the womb.—*Dublin Hospital Gazette, Dec.* 15, 1860, *p.* 375.

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117.—*Large Uterine Polypus.*—At a Meeting of the Medico-Chirurgical Society of Edinburgh, Dr. MATTHEWS DUNCAN exhibited a uterine polypus of very large dimensions, which he had recently removed from an unmarried woman, aged 47 years. It measured 5½ inches in length by 4½ in breadth, and was of the ordinary fibrous structure. It was, as he had pointed out in former specimens, quite destitute of muscular or mucous coverings,—being, in fact, an enucleated fibrous tumour which had been expelled from the uterine wall, its original seat, into the vagina, from which it was removed by operative interference. In this case it was remarkable, that during all this growth, enucleation, and change of situation, the woman's health had been scarcely disturbed in the least degree. She had irregular vaginal discharges; but as she had no suffering, she paid little attention to them. Indeed, it was not until the supervention of a cough, which forced the tumour down upon the outlet of the pelvis, and compressed the urethra, giving rise to retention of urine, that she had any complaint to make of suffering in the hypogastric regions.

It is a mistake to suppose that the difficulty of dealing with these large polypi lies in dividing the pedicle. In the present case, the pedicle could not be reached by the finger, until the tumour was removed from the vagina by traction. This was a very laborious process, effected only by repeated and persevering efforts, assisted by the bearing down of the patient. It was, indeed, a difficult labour in a primipara; only, instead of a child's head, a relaxed vagina, and labour pains, there were a fibrous tumour, an undilatable vagina, and traction by hooks fixed in the tumour. There was no difficulty in dividing the pedicle; for it snapped across as the tumour descended, and scarcely a drop of blood issued from it. If the pedicle had not broken, it would have made little difference in the operation; for the extent to which the easily moveable uterus is dragged down in such a case, is not considerable.—*Edinb. Med. Journal, April* 1861, *p.* 954.

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118.—*Chronic Mammary Abscess; Successful Treatment by Strapping, &c.* By W. COULSON, Esq.—Chronic mammary abscess, or, as it is sometimes called, encysted abscess of the breast, is a disease of considerable interest, inasmuch as it is in itself a tedious and often painful affection, and, closely simulating other more serious diseases of the same part, has many times been mistaken for tumour of the breast. A good illustrative case is now under the care of Mr. Coulson in St. Mary's Hospital:—

S. T., aged 20, single, a female servant, the history of whose affection dates back two years and a half. As is usually the case with chronic abscess at this site, its origin was unconnected with any acute lacteal inflammation, but arose without any distinct cause; it was attributed by the patient to cold. The first attack was aborted by prompt treatment. The nipple has remained retracted, and the breast subject to occasional attacks of painful swelling. A month ago, it had been unusually inflamed and painful, and presented typical features of the disorder.

The classical authority in surgery says that "the cure of these encysted abscesses of the breast may be most conveniently effected by making a puncture into them, and then passing a seton across them in a perpendicular direction; the inflammation thus excited in the tumour will speedily lead to its being softened down, and eventually disappearing." The treatment which Mr. Coulson adopted in this case, and which he commonly employs with success, was of a milder character. Scott's ointment was applied to the breast on lint; strips of plaster, of an inch and a half in breadth, were

then placed tightly around this dressing, and the whole carefully bandaged. Four days subsequently, Mr. Coulson caused the dressing to be removed and renewed. The improvement in the case was very marked, and the disease is fast progressing to a cure.—*Lancet, Nov.* 24, 1860, *p.* 510.

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## 119.—ON THE EMPLOYMENT OF APIOL IN AMENORRHŒA AND DYSMENORRHŒA.

By M. Joret.

During the investigations into the antiperiodic power of apiol, carried on some years since by the author and M. Homolle, they had occasion to note its emmenagogue power, and subsequent trials have proved it to be not only one of the most active and most certain, but also a very harmless emmenagogue. In his preliminary observations, the author strongly insists upon the important precept that the treatment of amenorrhœa and dysmenorrhœa must be based upon an exact appreciation of the cause, thus:—1. When the cause is plethora, general or local bleeding, baths, and emollients are the best emmenagogues. 2. When it depends upon a chloro-anæmic condition, whether spontaneous or accidental, martial preparations, good diet, a healthy abode and exercise in the open air, are the means most likely to act in restoring the menstrual flux. 3. When the disturbance of menstruation is connected with general nervous excitability, prolonged cold baths should predominate in the treatment. 4. But when the cause is a deficiency of general nervous stimulus, or of that localised in the genito-uterine apparatus, a perversion in the vitality of this apparatus, and especially of the ovarian sensibility, the stimulants termed emmenagogues are indicated; and it is in these cases, which are very numerous in practice, that apiol seems to possess decided superiority to any other means. 5. In those complex cases, in which several causes seem to combine in the production of the amenorrhœa or dysmenorrhœa, we should combat the various general or local conditions already adverted to before having recourse to emmenagogues.

Not only must we first investigate the nature of the cause of the amenorrhœa or dysmenorrhœa, but also determine the exact time at which the emmenagogue should be administered. This should be always at the period at which menstruation ought to return. There may be difficulty in exactly fixing this, especially when the menses have been long absent: but there are almost always symptoms arising from a congested state of the uterus which will sufficiently fix the period for commencing the remedy. The apiol may be given in capsules, each capsule containing four grains of apiol. Generally a capsule is given morning and evening, with a little sugared water. These are continued during the four or five days of the menstrual epoch. The month after, and even the third month, if menstruation has not become sufficiently abundant and quite regular, exactly the same procedure is to be observed. As a general rule, the menses appear more or less abundantly after the first course of doses, and it is rare for the medicine to be required to be taken after the third month. In dysmenorrhœa precisely the same course is to be observed. M. Joret quotes several cases exhibiting the decided efficacy of the apiol in primary amenorrhœa, or deficiency of secretion; in accidental amenorrhœa, or suppression, and in dysmenorrhœa.—*Bull. de Therap. Med. Times and Gazette, Jan.* 26, 1861, *p.* 97.

## 120.—ON THE TREATMENT OF THE NEURALGIA OCCURRING IN CASES OF AMENORRHŒA.

By Dr. J. Y. Simpson, F.R.S.E., Professor of Medicine and Midwifery in the University of Edinburgh.

[Neuralgia is only one of many symptoms occurring in cases of amenorrhœa. Of course our treatment in such cases must be in great measure directed to the constitutional affection. One very singular symptom occurs in these cases—a "uterine asthma." The best treatment is inhalation of chloroform or of carbonic acid. The first the patient may be taught to inspire out of her own hand; the second may be readily procured by mixing equal parts of crystallized bicarbonate of soda and tartaric acid, in a common wide-necked bottle.]

Pains under the left mamma, in the intercostal spaces, in the sacral region, or in the temples, are among the most constant and clamant symptoms seen in connexion with amenorrhœa. To relieve them you may administer some of the various narcotics—a practice, however, that may lead the patient into dangerous habits, and which, therefore, I would earnestly counsel you, as much as possible, to avoid. The use of small doses of arsenic—two drops of Fowler's solution thrice a-day will be of more ultimate benefit to your patient, although not so immediately effectual. Where there is any indication of a rheumatic taint of constitution, you will probably find more satisfactory results from the administration of thirty or forty drops of the tincture of actea racemosa, or black snake root,* three or four times daily in a little water. Locally, the application of sedative and anæsthetic liniments and plasters occasionally decreases or removes these neuralgic pains. Perhaps the most rapid relief you can give your patient will be by the subcutaneous injection of a watery solution of morphia. She will point to an undefined space, say, in the side, as the seat of pain; but if you press carefully with the finger at different spots, you will succeed in touching a point, in most cases immediately under one of the ribs, where pressure causes more acute suffering. If you adopt Dr. Wood's plan of thrusting a fine-pointed syringe through the skin at this spot, and then inject ten or twenty drops of a watery solution of the bimeconate of morphia, equalling the ordinary strength of laudanum, you will in some cases succeed in relieving the pain at once; and if it does return, it will probably be in another nerve, or in another part of the same nerve, whence a repetition of the narcotic injection may possibly again expel it. Latterly, in this subcutaneous injection of morphia I have usually contented myself by sending the anodyne into any spot at the pained part without trying to search out for the purpose the supposed site of the principal branch of the neuralgic nerve. In some cases the remedy perfectly succeeds; in others it perfectly fails. And when it does fortunately succeed, the wonder is, not that it acts as a temporary local anæsthetic merely, but that occasionally the pain does not return again after the effect of the opiate has passed away. One of the first cases in which I ever tried this subcutaneous injection was in this respect one of the most successful which I have ever met with. The patient, a young Australian lady, had been suffering severely from the limited pain so often attendant upon deranged uterine health for two years. Many means, I was told, had been tried for its removal, but all in vain. I injected a few drops of a solution of bimeconate of morphia into the pained part. In five minutes the pain was gone, and has never, I believe, in any degree returned. It had much more the speed and brilliancy of a surgical than of a medical cure. But such instances are rare, very rare. And when you do use this subcutaneous injection, remember to warn your patients that sickness and vomiting may follow. Otherwise they may be alarmed by the supervention of these symptoms. The neuralgic headaches, which are sometimes so distressing to the patient in cases of amenorrhœa are usually most effectually relieved by the aid of heat, applied by means of a sponge squeezed out of hot water, or otherwise. Patients dis-

* A favourite remedy in the United States for acute and chronic rheumatism.

cover this for themselves; and you will sometimes find them applying mustard or turpentine to the forehead to relieve the deeper pain. In cases where the headaches have become of a chronic character, and of constant continuance, I have found the most admirable result from the use of nickel, administered in the form of the sulphate or phosphate, in doses of half a grain or a grain three times a-day.—*Med. Times and Gazette, May* 18, 1861, *p.* 515.

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121.—*On the Treatment of the Eruptions occurring in Cases of Amenorrhœa.* —By Professor SIMPSON, Edinburgh.—It holds good with regard to the acne that is wont in some cases to appear each month, perhaps more distinctly than with respect to any of the other secondary symptoms arising from suppression of the menses, that the secondary disease disappears at once when the menstrual discharge is restored. But in a few cases, it still lingers on, and sometimes the disfigurement is very distressing to the patient, so that you may be called upon to make some direct application to the affected part. The application which I have found most useful in these cases of chronic catamenial acne is the oil or butter of antimony. This is usually said to be a powerful caustic; but if you apply it to the skin, with a brush, and neutralize it speedily with some bicarbonate of soda, you will find that it causes almost no pain, and is not followed by any violent effect. What the explanation of its action may be in cases of acne I am not prepared to state; but certainly when applied in the way I have described every two or three days for a week or two, it has usually a very wonderful effect in causing the disappearance of the eruption. If, after being employed for a time, it should appear to be losing its effect, its use should be interrupted, and applications should be made alternately of citrine or some other mercurial ointment, and of some of the preparations of iodine. Most of the preparations of this last drug, labour under the disadvantage of producing a discolouration of the skin, which renders their application to exposed parts always somewhat repulsive to the patient. But my friend, Dr. Arkwright, of Accrington, has recently brought under my notice a preparation which seems to possess all the activity of the tincture of iodine, and which is quite colourless, so that it may be applied to the face or painted over strumous glands in the neck, without any discomfort to the patient. It is composed of one part of the ordinary compound tincture of iodine of the London Pharmacopœia, and two parts of the milder aqua ammonia. This mixture, which is at first brown, but after standing for forty or fifty hours, becomes quite clear and colourless, may be applied to the skin two or three times a day for a lengthened period, without producing any of the pain or excoriation which attend the prolonged use of the ordinary tincture, and in obstinate cases of chronic acne it forms an admirable adjuvant to the other remedies of which I have spoken. As a local application, Rumex ointment often answers well; and if the eruption is very irritable, bismuth or cucumber ointment, with the addition of a drachm of officinal prussic acid to each ounce of these ointments. Along with these external applications, however, it is often advisable to combine the internal administration of small doses of arsenic.—*Medical Times and Gazette, May* 18, 1861, *p.* 516.

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## 122.—ON LACTATICS.

By Dr. A. K. GARDNER.

Under this title, Dr. Gardner, Midwifery Professor at the New York Medical College, designates the means capable of influencing the secretion of milk, and he arrays them under two heads—Galactics, or promoters of lactation, and Anti-Galactics, or opposers of lactation.

1. *Galactics* are again divided into galactagentia, inducers of milk, and galactagogues, expellers of milk. (1.) *Galactagentia.* (*a.*) *Liquid food, as milk,* good nutritious soups, ale, or other malt liquors; spirituous liquors *diminish* rather than increase the secretion. (*b.*) *Fœniculum* has been lauded

from the most ancient times, and is still much employed by the Germans. Dr. Gardner says that he has found great advantage attend the use of Hufeland's formula (Sem fœn., ʒj.; cort. aur., ʒss.; magn. subc., ʒiij.; sacch. alb., ʒij. M. Fiat pulv.—A teaspoonful three times a-day), the secretion having been in one of the cases suppressed for three weeks. (*c.*) *Ricinus communis and jatropha curcas.* Dr. Gardner does not seem himself to have used the leaves of the castor-oil plant, or of the jatropha curcas, and refers to the observations of Dr. Routh and Dr. Tyler Smith; but he says that other substances of the same class, vanilla and tonqua beans, have proved efficient in his hands. The former, when given in half-drachm or drachm doses of the extract, or when even used to flavour puddings, ices, &c., frequently exerts a marked effect, although not a very reliable one.

(2.) *Galactagogues* are articles which tend to promote the flow of the milk when secreted, relieving its retention. "Retention of milk may be caused by its becoming inspissated and thickened in its proper tubes by spasmodic stricture of the milk-tubes, by acute inflammatory swelling thereof, or as the result of chronic inflammation of any number of milk-tubes near the nipple by closure of the aperture with obliteration of the canal, sometimes for an inch or more. These I have described in a former lecture, under the title of "Stricture of the Milk-Ducts and System of Sphincters or Muscular Fibres surrounding the Outer Terminus of the Reservoirs of the Milk-Ducts at the Extremity of the Nipple." In this last case, medicines are insufficient, punctures of the tubes being necessary to remove the obstruction. As local applications, M. Ranques strongly recommends the following mixture: —Cherry-laurel water, ʒij.; ext. of belladonna, ℈ij.; ether, ʒj. Velpeau states that he has found this sometimes useful and frequently of no avail. He himself recommends sweet oil, ʒiij.—ʒij.; ext. bellad., gr. xvss.; ammonia, ʒj.; camphor, ʒss.; yelk of egg, gr. xxx.; ether, ʒss.; to be gently applied three or four times a-day. "It is my opinion that, although so many stimulating and anti-spasmodic embrocations, as those of ammonia and camphor, iodide of lead, and especially mercurial friction, are strongly recommended, it is very probable that warm fomentations of water, systematic frictions towards the nipple, and systematic pressure (by means of compresses and rollers, adhesive straps, or compressed sponge) properly employed and persevered in, do in most cases all the good that can be locally done, besides natural and artificial suction."

2. *Anti-galactics.*—These agents may be sub-divided into two, Ischogalactics and Phygogalactics. (1.) *Ischo-galactics*, arrestors or suppressors of milk. As general means against the excessive secretion of milk, diaphoretics, mild refrigerants, arterial sedatives, saline aperients, low or moderate diet, a sparing use of condiments and liquids, and avoidance of sexual excitement, are to be recommended. Lever and Kennedy found nauseating doses of tartar emetic valuable, and Stillé speaks highly of full doses of castor oil. The special ischogalactics are as follow:—*Belladonna*—A great deal of evidence has been published in the British and American Journals in favour of the efficacy of belladonna in drying up the milk. Still, some conflicting opinions exist, and the author, in judging from his own experience, "must deduce the opinion that, while it exerts no influence upon the milk already in the breast, it does in some cases, although not very apparently in all, tend to diminish or suspend further secretion. A mode of employment which I have found very effectual is smearing the watery extract thickly over the whole breast, and repeating the application immediately after washing off the previous one, drying carefully by compression, and removing by suction or otherwise as much of the milk at the time as possible, once or twice in the twenty-four hours; or a plaster made from a cloth or kid may be smeared with the extract, and may be applied partially or entirely, leaving a hole for the nipple, so as to allow of the breast being drawn or the child being applied." *Conium*, much employed by the ancients, has never been tried bythe author, as he thinks it a somewhat dangerous application, likely to lead to atrophy of the gland. *Iodide of Potassium* has been strongly recommended by some French and German practitioners, and Dr. Gaillard Thomas says of its em-

ployment in full doses, while the breast is painted around the nipple with extract of belladonna, "These two remedies have found great favour with the vast majority of those who have tried them; and, although I have seen them fail in checking, or even in diminishing the secretion, I have much oftener observed that benefit resulted from their use." *Sage*, used with advantage in a strong infusion by Van Swieten, has often since been resorted to with success. *Camphor.*—In answer to Dr. Stillé's statements that spirits of camphor is very successfully employed to suspend the secretion, Dr. Gardner says that his experience shows that camphor possesses no ishogalactic properties whatever. *Tobacco.*—"The effect of the ung. tabaci has been fully as marked as that of belladonna when employed as a plaster, as recommended for the latter. I have never seen any constitutional effects from it as is seen from the tobacco poultice upon the testicles, although the effect of belladonna, similarly used is very marked upon the iris." *Peppermint.*—"The essence of peppermint, externally applied, is with many practitioners in great favour for arresting the secretion. I have used it in many cases, and, while it failed in some, yet I am convinced of its marked efficacy. I have generally used it in the following proportions:—Ol. M. P., 3jss.; Ol. ricin. ℥ iijss.; Ol. bergamot vel jasmin, 3jss.; camphor, 3ij. M."

(2.) *Phygo-galactics* are articles which tend to disperse the milk accumulated in the lactiferous tubes, especially when it forms indulated tumours. Their action is supposed to be exerted on the system of nutrition, or specially on the absorbents. They are mercurials, the tincture of iodine painted over the breast, bromine, preparations of gold and silver, belladonna, conium, camphor, and chamomile. As to belladonna, though strongly recommended by many, the author doubts whether it possesses any phygogalactic, as distinguished from its ischogalactic effect. That conium possesses phygogalactic power has been shown by D'Outrepont and others. It is possible that camphor may, as is represented by several authors, possess valuable phygogalactic properties, but the author has never derived much benefit from it, nor can he speak more decidedly in favour of chamomile.—*Med. Times and Gazette, March* 2, 1861, *p.* 231.

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123.—*Glycerine and Camphor for Suppression of the Secretion of Milk.*—Dr. HARRISS having carefully tried belladonna in cases in which the suppression of the secretion of milk had become desirable, with but little success, now employs in preference a saturated solution of camphor in glycerine, gently applied over the surface of the breast by means of flannel several times a day. The same solution is also of utility in sore nipples, although Dr. Harriss usually adds four grains of tannin to the ounce. He considers the next best means of preventing threatened abscess of the breast, is to fit the whole gland closely with adhesive plaster, leaving a hole for the nipple.—*North American Med. Chir. Rev.; Med. Times and Gazette, May* 18, 1861, *p.* 530.

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124.—*Brandy and Glycerine for Excoriations and Fissures of the Nipple.*—By Dr. W. FRAZER, Lecturer on Materia Medica to the Carmichael School of Medicine.—I have lately seen a mixture of brandy and glycerine in equal quantities applied with much advantage to fissures of the nipple, and from its effects I think it is worth further trials. The mixture is thick, resembling a mucilage, and may be applied as frequently as desired. Brandy itself is an old remedy for this purpose, but it soon dries up and evaporates; the glycerine has the effect of protracting its local effects, and also keeps the nipple soft and moist.—*Dublin Hospital Gazette, Dec.* 1, 1860, *p.* 365.

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125.—*Sulphate of Zinc in Cancer.* By HENRY GEORGE, Esq., Kensington.—In the year 1857 a lady consulted me on account of a tumour in the *right* breast; it had there existed more than four years, and was evidently cancerous; her sufferings were not trifling, and her general health was some-

what impaired. In the course of a short time high authority was appealed to, which confirmed my belief in the character of the disease, and a plan of treatment was recommended. In the space of two or three months her sufferings were somewhat increased, the tumour was enlarged, and her general condition altered for the worse, for there existed evident marks of decreased vitality. I determined to try the sulphate of zinc, a tonic whose powers I am persuaded are both safely available and very valuable. In the course of a month all pain had ceased, and her general condition was very much improved. In compliance with this lady's request I returned to the former mode of management. Scarcely a month elapsed before her sufferings returned, and her loss of strength was considerable. I persuaded her to return to the use of zinc, and the same happy result followed. This alternation of suffering and relief from discontinuing and resuming the use of the zinc occurred three times. That medicine has now been continued for many months without interruption, and the iodide of potassium has for a long time been also administered. This lady continues to the present hour free from pain, her general health being very good, and the tumour having decreased in size at least two-thirds. Six weeks ago, a portion of the skin about the size of a shilling, of a deep red colour, took to bleeding, afterwards discharging a watery fluid; this has now perfectly healed.

The absence of pain in such a condition, the improvement of health under such circumstances, and the great decrease in the size of the tumour, are, whatever may be the result of the case, facts well worthy of consideration.—*Lancet, March* 16, 1861, *p.* 276.

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## 126.—A SIMPLE INSTRUMENT FOR INFLATING THE LUNGS OF INFANTS.

By Dr. J. G. Wilson, Physician-Accoucheur to the Glasgow Lying-in Hospital.

[The usual methods by which the lungs of newly-born infants are inflated are all more or less objectionable and unsatisfactory. Read's double-action syringe, which Dr. Wilson has not had the opportunity of trying, was strongly recommended by the late Dr. Snow.]

The instrument essentially consists of a vulcanized india-rubber ball about the size of an orange, to which is attached a German-silver tube, about six inches long, and gently curved towards its free extremity. The tube is closed at the extreme end, but has two openings or eyes, like a female catheter, a short distance from the point. On compressing the ball, the contained air rushes along the tube and through the openings above-mentioned, and on removing the pressure the ball rapidly expands, and becomes instantly refilled with air, which may again be evacuated as before. On introducing the tube into the larynx, and acting in this manner, it is obvious that for the most part the same air would be used over and over again, which would be a manifest disadvantage and a decided objection. This, however, may be easily remedied, by making another opening in the tube, about an inch from its attachment to the ball, for the free ingress of fresh, cool, dry air. During the compression of the ball, the left thumb will easily cover the opening, which must, however, be removed to admit the entrance of pure air during the subsequent expansion of the ball. This opening being somewhat larger than the other two, and being much nearer the ball, readily permits the entrance of fresh air. The instrument can be readily introduced in the following manner:—After throwing back the child's head a little, pass the left forefinger over upon the root of the tongue, into the rima glottidis, then with the instrument in the right hand, slide the tube along the surface of the finger (which at once depresses the tongue and serves as a guide), and on reaching the rima, insert the tube at the moment you withdraw the finger. If this plan of operating be attended to there is little fear of introducing the tube into the pharynx instead of into the larynx. It is not so necessary, as when other methods of inflation are used, to push back or depress the larynx

in order to prevent the transmission of the air through the œsophagus. The insufflation of the lungs must be gently and slowly performed, so as to simulate as nearly as possible the normal respiratory process. After each inflation, the chest must be slightly compressed, in order to facilitate the expulsion of air from the lungs. The inflation of the lungs, alternated with pressure on the chest, should be steadily persevered in until respiration is regularly established, and ought never to be relinquished as hopeless whilst the least fluttering or quivering motion over the cardiac region is perceptible.

This instrument is abundantly simple, both as regards principle and construction. It can be easily introduced, and worked for any period with little exertion; and, from its small size, is very portable. The air injected into the lungs is at once pure, cool, and dry, and the force with which it may be propelled, can be easily regulated. It is not so liable to rupture or injure the pulmonary air-cells, which there is reason to fear is sometimes done in consequence of too violent restorative attempts at insufflation with the mouth or with the bellows. The irritation which the presence of the tube may occasion in the fauces and larynx, instead of being detrimental to the child, as I have heard remarked, ought of itself in many cases to have a salutary and beneficial effect, as tending to excite a gasping inspiration, or a convulsive sigh or sob. If the instrument is properly applied, as I have already remarked, the air is more certain to enter the lungs, and less likely to pass into the stomach and distend the intestines, as is too often the case when the mouth alone is used, even although the larynx be pressed back. Many infants, I am convinced, are lost from the process of respiration being impeded by the presence of fluid or mucus in the air-passages, and which, from inherent weakness and debility, they are unable to expel. This is indicated by a peculiar rattling or gurgling sound when the child attempts to breathe. This collection of mucus or fluid in the respiratory passages, is, I conceive most likely to occur when the child's head is the last part to be born, and particularly when retained in the vagina for some time. This may arise from two causes. The impression made on the cutaneous nerves of the chest by contact with the atmospheric air induces the child to inspire, and thus to draw in mucus or other fluid lying in the vagina in contact with the mouth. The entrance of any fluid into the larynx or trachea in such cases may also be occasioned by gravitation. Of course the fluid may enter the œsophagus, but this is of minor consequence. It appears to me that this case of suspended or interrupted animation in the newly-born infant has been too much overlooked and neglected. With the instrument above described I have, on several occasions, succeeded in withdrawing considerable quantities of fluid from the air-passages, with the effect of materially relieving the breathing. When the fluid is in large quantity, the tube may be clogged up, and the ball partly filled, when it becomes necessary to withdraw the instrument, and clear it of all the fluid, otherwise it will be again injected into the air-passages. In the slighter forms of asphyxia the use of the instrument is quite unnecessary, and, when employed in the more urgent cases, it does not preclude or interfere with the use of other means, such as the alternate immersion in the hot and cold bath, effusion or aspersion with cold water, stimulants applied to the surface, or introduced per rectum, rubbing with warm flannel, galvanism, or electricity if a proper apparatus is at hand, &c.

The instrument may be obtained from Mr. W. B. Hilliard, surgical instrument maker, Renfield-street.

In those cases of suspended animation, with lividity of the child's countenance, and other symptoms indicative of cerebral congestion approaching the apoplectic condition, the respiration will frequently be much facilitated by relieving the engorged and overloaded vessels of the brain, and this may generally be done by allowing a drachm or two of blood to escape from the divided vessels of the cord before applying the ligature. When much difficulty is experienced in the birth of the child's head after the delivery of its body, the *salvation* of the infant may, I am convinced, in many cases be secured by *establishing* intra-vaginal respiration. There can be no doubt but that, in the

absence of the forceps in such cases, a degree of traction on the neck of the child is sometimes exerted before the head is delivered, which is of itself almost incompatible with life. Indeed, in two or three cases which have lately come under my own observation, the child, although so far resuscitated as to respire by its own efforts for several hours, yet the injury done to the cervical region was such as to preclude all chance of its ultimate survival. The application of the forceps in such cases does away with the necessity of dragging or pulling much upon the neck; but these instruments are unfortunately often not at hand when required, and some time must necessarily elapse before they can be procured, thus occasioning a delay which will in all likelihood be fatal to the child if let alone. As soon then as it is ascertained during the course of labour that the head of the child will be the last part of it born—especially if any difficulty or delay is anticipated in its transit—the forceps should be sent for, and kept in readiness in the event of their being required. When the head is firmly retained after the birth of the body, and the forceps are not at hand, endeavour, if possible, to excite and maintain the respiratory process till they be procured. This may sometimes be accomplished by merely keeping the child's mouth open with the finger, and the perineum retracted, so as to permit the free access of air into the vagina, or the instrument above described—either as it is, or with an elastic catheter, or a flexible tube substituted for the metallic one—may perhaps be employed with greater prospect of success.

The infant at birth clings with such tenacity to life, that—in the absence of putrefaction, or any malformation, defect, &c., incompatible with life—it becomes often a very difficult matter to determine with any degree of certainty whether the vital principle is actually extinct, or animation merely suspended. We should, at all events, never too hastily despair of effecting the complete restoration of the infant, however unpromising the case may at first sight appear. There is good reason for believing that many infants, especially among the lower classes, are annually consigned to the tomb, whose lives might have been preserved had resuscitative means been fully and fairly tried.—*London Med. Review, Dec.* 1860, *p.* 299.

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127.—*Treatment of Prolapsus Ani of Children by Subcutaneous Injection of Sulphate of Strychnia.* By M. FOUCHER.—A child, 4 years old, came under the care of M. Foucher, who had suffered for several months from prolapsus of the mucous membrane of the rectum. When not returned immediately, great pain and difficulty attended its reduction, owing to its being closely grasped by the sphincter. M. Foucher determined to apply Wood's plan of subcutaneous injection to this affection, in order to act directly upon the fibres of the sphincter. He therefore, with one of Pravaz's syringes, injected in the direction of its fibres, and just external to the anus, ten drops of a solution of sulphate of strychnia, in the strength of twenty centigrammes to twenty grammes of water. Twenty-four hours afterwards fourteen drops were again injected. The cure was immediate, and six months afterwards had remained durable. Without attaching undue importance to the cure of a case of disease which is sometimes very easily relieved, it is deemed desirable to direct attention to a mode of treatment so simple, and accompanied by so little pain.—*Gazette des Hôp.; Med. Times and Gazette, Nov.* 10, 1860, *p.* 463.

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## 128.—POLYPUS OF THE RECTUM IN A CHILD, SUCCESSFULLY REMOVED.

Case under the care of THOMAS BRYANT, Esq., at Guy's Hospital.

Polypus of the rectum, although recognised by surgeons as a distinct disease, has not received from them that attention which it deserves; and children, labouring under this affection are too often, from the want of a due appreciation of the subject, allowed to go on for many months losing blood

from the anus, and tortured with the irritation of a prolapsed bowel. A good illustration of this fact, and of the disease, has been brought to our notice under the care of Mr. Thomas Bryant, who, it may be remembered, brought the subject before the profession by an interesting paper published in the Lancet in Nov. 1858. (See *Retrospect*, vol. xii., p. 255.)

The case was that of a boy two years and a half old, who for upwards of nine months had been daily losing blood from the bowel: at times this hemorrhage taking place during defecation, but occasionally at other periods. The mother had observed the whole bowel to be now and then prolapsed, and intense straining and pain to accompany each motion. When the bowel had been returned, a cat-like growth would often remain, which was with difficulty reduced. The child had been taken to many surgeons, who had treated it with medicines, but none had ever made a local examination. The character of the disease was readily recognized upon a digital examination, a polypus of the size of a large nut being detected, fixed by a thick pedicle to the bowel about an inch and a half within the sphincter. Mr. Bryant with his finger hooked the growth down, and was about to put a ligature on the pedicle, when a sudden action of the sphincter and levator ani muscles took place, breaking off the growth, and thus removing the disease. All symptoms immediately ceased, and a good recovery took place.

Polypus of the rectum may, however, be found in the adult, although it is not so common as in children, according to Mr. Bryant's experience. The following is a good example lately under the care of that gentleman:—

A man, aged 66, for six weeks had observed that during defecation something protruded from the anus, which was with difficulty reduced, bleeding generally accompanying his descent. For twenty-four hours previous to his application to Mr. Bryant this growth had remained prolapsed, and as he had been unable to return it, he sought advice. Upon examination, a firm fibrous polypus, of the size of an almond, was detected growing from within the sphincter. A ligature was applied to its pedicle, and on the second day the growth fell off, recovery taking place.

We would remark, that the occurrence of polypus of the rectum in the adult is very rare, as pointed out by Mr. Curling in his work on "Diseases of the Rectum." He states that it generally occurs in early life, and that in children it usually makes its appearance external to the anus after a stool, the growth resembling a small strawberry, being of a soft texture, granular on its surface, and of a red colour. It has a narrow pedicle, of variable length; it produces no suffering, but requires to be replaced by the fingers when protruded, and causes a very slight bloody discharge, which excites some alarm. The description of the complaint given by the mother or nurse is liable to mislead the practitioner into the belief that it is simply prolapsus. The nature of the disease can be determined only by an examination of the tumour on protrusion, when the treatment will be obvious enough.

[These polypi are not so rare as here expressed. The rarity we believe consists in not detecting, or even suspecting them: at any rate they are not so very uncommon in females. The reader will find some good illustrations of this remark in our last volume by Mr. I. Baker Brown, (vol. xlii, p. 265), who finds that many fissures in the anus in females have each a polypus hanging or pushing into the fissure from the opposite side of the rectum, which cases are easily cured by ligaturing the polypus and dividing the fissure with Copeland's blunt-pointed straight bistoury. To divide the fissure only would not suffice.]—*Lancet*, *Oct.* 27, 1860, *p.* 410.

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## 129.—ERRORS RESPECTING DENTITION.

By Dr. Jacobi.

Introductory to his lectures on "Dentition and its Derangements," Dr. Jacobi makes the following observations:—

"You know that, among the public at large, even among the educated por-

tion of the community, teething is regarded as one of the two scape-goats of all diseases of infantile age. Teething and worms are among matters acknowledged as the universal and all-powerful sources of disease. Whenever an innocent ascaris or puny oxyasis is observed in the fœces of a child, worms are, for years to come, considered as the undoubted cause of any disease that may occur. Teething, a normal, physiological development, taking place at an age which, for many reasons, is subject to a large number of diseases, has a strong hold on frightened maternal minds. The first dentition generally occupies the first two years of infantile life, a period in which the child is peculiarly liable to diseases both numerous and frequently dangerous. As the protrusion of a tooth (and on the average a tooth will cut every month) is a remarkable phenomenon, and is something new and visible, it is believed to be the cause of every unfavourable occurrence in early life. A mother will bring to you her child, thin, emaciated, and anæmic, with sunken eyes and the wrinkled physiognomy of old age, and tell you she is well aware the poor thing is suffering from teething, and that, therefore, nothing can be done to alleviate its sufferings. She will never be convinced that the child is dying from her own neglect; but she has allowed a slight catarrh of the intestines, perhaps, to degenerate into incurable follicular ulceration. . . . . Teething is thus considered the most efficient cause of most of the terrible diseases which prove fatal to thousands of the rising generation. I can assure you that the readiness to attribute all the diseases of infantile life to teething has destroyed more human beings than many of the wars described in history. For, though parents are so much impressed with the belief of the dangers of teething, still they never think of attempting to save the lives of their children by counteracting the supposed life-endangering power of a normal process.

"What is now the belief of the public has been the conviction of the medical world through centuries down to the present time. General experience shows that the persuasion of the scientific world, after having been given up to make room for more correct opinions, has remained in the public at large, and it is to be feared that it will not soon be removed. And it would be fortunate if the prejudice were confined to the public. But, unfortunately, it still lingers in the medical profession, and it is for this reason that I have dwelt upon it thus lengthily. Nothing is more common than to hear doctors, young and old, in cases of infantile disease, diagnosticate 'teething,' after mother and nurse have done so before; and nothing is more frequent than to be told that the death of a child was the consequence of dentition. I have seen in this city (New York) a certificate of death in which the direct cause of the death of a child five years of age, with his jaws full of teeth, was stated to be 'teething.' Consider for a moment the absurdity of the conclusion, that a normal, physiological process is fatal to the existence of a living being! Who has ever ventured to assert that menstruation, pregnancy, or the climacteric years are the direct causes of death? It is equally absurd to assert it of dentition; and yet such statements are daily made by physicians. According to the census of England for 1857, there were in the United Kingdom (England alone) 3992 deaths from teething, 3791 of which occurred in children of less than two years, and 201 in children from two to five. Between 1845 and 1850, no less than 3466 infants are reported to have died in London from teething and the disorders caused by the general irritation attending dentition—the number of the deaths from all causes being 258,271, giving the proportion of 1 death for teething to 74 from all causes. In the state of New York there died 626 children from teething in 1855; but it is not stated whether a part of these unfortunate children had not the full contingent of teeth of the first dentition."

After adverting to the graver symptoms sometimes met with during dentition, Dr. Jacobi continues:—"In a certain number vomiting will stop, but the diarrhœa continues. The deluded mother, who felt a little uneasy at the severe character which teething seemed to have assumed, is gratified, after the main symptoms have passed by, to find that her child is suffering from diarrhœa only, and that, in this manner, teething will be made easy and com-

fortable. But, alas! this deception on the part of the mother is too often fatal to the child. The diarrhœa is allowed to go on for days and weary weeks; the digestion becomes hopelessly destroyed, the abdomen immensely distended with gas, the mesenteric glands swollen and impermeable to chyme, the catarrh and over-secretion of the glandular follicles of the intestine lead to deep ulcerations of the intestinal canal, the diarrhœa becomes also more frequent, serous, mucous, or bloody, the arms and legs of the little sufferer dwindle away, and the countenance becomes emaciated and senile. The scene closes with a consoling certificate from some doctor or druggist, affirming that teething was the cause of death. Thus 'millions' of infants are destroyed by ignorant, prejudiced, and incorrigible advisers. I say incorrigible. I know that mothers will always consult their prejudices first, the prejudices of their neighbours next, perhaps, at some later time, common sense, and finally they may seek the advice of an educated medical man. I know that a mother who has consigned a beloved child to the grave, will repeat the follies which cost her the child she has lost. If you remonstrate with her for neglecting the second as she did the first, she will reply, Was not the child teething? Would you prevent it teething naturally? Was it her fault that the child got teeth with difficulty? The true inference would be, that nature neglected much, and that it was greatly at fault in the matter of dentition. I once read a newspaper announcement of the death of a child, in which the parents affirmed that 'the Lord had hauled the dear child up to heaven by the teeth.' Now, in this case, neither the father nor the mother was at fault. . . . . It will better answer my design to give you a sketch of what dentition is anatomically and physiologically, in order to show clearly the normal and abnormal course it may take. I shall thus be able to explain and limit the numberless complaints generally attributed to it. If I can relieve your minds of the impression, that dentition destroys thousands, and even tens of thousands, of innocent beings, who are yearly sacrificed, in reality, to the prejudices of other times, I shall be abundantly satisfied."—*American Med. Times; Med. Times and Gazette, Dec.* 29, 1860, *p.* 643.

# MISCELLANEOUS SUBJECTS.

## 130.—REMARKS ON A CASE OF POISONING BY STRYCHNIA—RECOVERY.

By Dr. James Part, London.

I was called on the forenoon of the 1st of October, 1860, to see Eliza M., aged 31 years, a domestic servant, who was reported to have taken poison. About a quarter of an hour before I arrived, she had been found on the floor of the kitchen by her mistress, whom her screams had brought to her assistance, and when an attempt had been made to lift her up to place her on a chair, she had been instantly seized with twitchings, followed by powerful spasms of the limbs and trunk, with loud and piercing screams.

When I first saw her she was recovering from this paroxysm. She was sitting on a chair, with both legs stretched out, the feet being widely apart and everted, and the toes flexed; the arms were also powerfully extended, and the hands convulsively grasping the arms of two men who were supporting her on the chair, from which she appeared very much afraid of falling. The body was in a state of tetanic spasm, being powerfully extended, or indeed bent somewhat backwards, approaching a state of opisthotonos, and rested entirely against the top of the chair-back and the front edge of the seat. The expression of the countenance was wild and anxious; the face blue, very like a person suffering from cholera, and covered with a cold perspiration; the eyes staring and prominent; the breathing irregular and convulsive; the pulse was so quick that it could not be counted, and had a peculiar fleeting, indistinct character.

The patient was muttering indistinctly some prayers for forgiveness of her crime. On laying my hand on her shoulder, as I asked her what she had taken, the muscles of the trunk and limbs were thrown into a state of spasmodic contraction, and this repeatedly took place when any part of the body was touched; even the application to the lips of a glass of water produced these phenomena to a considerable degree. From these symptoms it was evident to me that she had taken strychnia in some shape, but either from unwillingness to tell what she had taken, or from the confused state of her mind at the time, I could not then ascertain in what form. I saw her at half-past ten, and the poison had been taken about a quarter before ten; consequently about three-quarters of an hour had elapsed, during which time the poison had been affecting her.

I immediately administered a strong emetic of sulphate of zinc and ipecacuanha, and sponged the head with cold water, though I must confess with little hope of a successful result, being apprehensive that absorption of the poison had taken place in sufficient quantity to destroy life. As the emetic did not act for nearly three-quarters of an hour, I had to consider the propriety of applying the stomach-pump; for during that period she continued to have slight tetanic spasms resembling electric shocks, which followed any movement or any touch of the body. She all the while manifested great fear of falling, would not quit her hold on the hands of those about her, and continually ejaculated, "Oh, hold me!" The legs were continually drawn asunder involuntarily, and the position appeared painful to her, as she repeatedly desired to have them placed together. I was deterred from using the stomach-pump by considering the effects produced by the application of the glass to the mouth, by touching the body, or by tickling the throat with a feather to excite vomiting, all of which brought on the tetanic spasms, the latter somewhat severely.

A second emetic was therefore administered, and cold water continued to be applied to the head. After a while she became very much exhausted, the

spasms were more frequent, and she begged to be laid on the floor, having lost the use of her legs. This movement brought on the most alarming attack of spasm it has ever been my lot to witness, except perhaps, one, which proved fatal, in a case of traumatic tetanus. The arms were powerfully extended; the legs also extended, and drawn widely apart; the muscles of the back were in a state of the most powerful contraction—in fact of complete opisthotonos; the expression of the countenance was wild; the face had a dusky, purplish hue; the eyes were wide open, the eye-balls protruding, and the pupils so widely dilated that the irides were barely visible After remaining some seconds in this condition, the whole muscular system appeared at once to quiver, the spasms relaxed, her hands loosed their hold, the arms falling by her side, the jaw dropped, the angles of the mouth were drawn down, she heaved a deep sigh, and I believed her to be dead. After some seconds, another inspiration, brought on apparently by cold affusion to the head and chest, seemed slightly to restore animation, and she gradually began to breathe—very imperfectly at first, but afterwards with some regularity; and at length she recovered from the attack. In about ten minutes after this she began to vomit, to facilitate which copious draughts of warm water were given. She continued to vomit at intervals for more than two hours. As the vomiting continued, so the spasms appeared to abate in their intensity; and in about four hours from my first seeing her, we were enabled to get her up-stairs to bed without the occurrence of the spasms, which previous to the last severe fit, had come on upon every attempt to move her from the chair. As soon as the sickness had in some degree abated, a mixture of extract of Indian hemp with chloric ether was administered, with a view of diminishing the spasms and counteracting the prostration she evinced; but most of the mixture was quickly rejected by the stomach.

On my next visit the spasms had subsided into slight occasional tremors, and the skin was becoming hot and the pulse fuller; reaction appeared fairly to have set in. An effervescing mixture was therefore prescribed, and, to clear out the bowels, a castor oil draught. At nine o'clock at night—eleven hours after she had swallowed the poison—she appeared tolerably well and free from pain, except some soreness in the muscles of the arms and legs. On the following morning she appeared as well as if nothing had happened. There was, however, some slight heat of skin, and the tongue was somewhat furred. As the castor oil had not acted, a dose of calomel and colocynth was given, followed by an aperient draught; the saline mixture was continued. The next day—forty-eight hours after she had taken the strychnia—she was quite well, and requested to be allowed to go home to her relatives. She has twice since called upon me, and states that she is quite well; she expresses great contrition for her offence.

In one of the intervals between the attacks of vomiting, the patient pulled out of her pocket the paper and envelope which she said had contained the poison, and which proved to have been a packet of "Battle's vermin-killer," the active agent of which is now too well known to be strychnia, from the numerous instances of fatal poisoning which that substance has caused. The poison had been procured on the preceding evening from the shop of a neighbouring druggist.

I am informed by my fried Dr. Letheby, who has had opportunity to examine this substance, that the poison contains about twenty-three per cent. of strychnia; and as each powder weighs, pretty uniformly, about fifteen grains, we may assume that three grains of strychnia had been taken in this case. Dr. Letheby was kind enough to analyse a portion of the liquid contents of the stomach for me, and obtained the most unequivocal evidence of the presence of strychnia, which my own more clumsy manipulation also obtained satisfactorily.

Having now given a concise, and, as nearly as I am able, a correct account of the more prominent features of this case, as I observed them, I am desirous of making a few observations, suggested by contemplating its various features, on the means which proved successful in my hands. Being few and simple, we shall easily arrive at a correct conclusion as to which the

recovery may be fairly attributed; and I am inclined to attach much importance to simplicity in our remedial measures, as being the only way of arriving at accurate notions on the subject of treatment. But before I make these observations, I will endeavour to bring under notice the principal facts connected with the treatment of those cases of poisoning by strychnia which have recovered.

Several substances have been brought forward, both by physiologists and practitioners, to which the designation of antidotes has been given, by which I understand remedies possessing the power of neutralizing injurious effects on the animal economy. These would, doubtless, first be present to the minds of gentlemen on finding themselves face to face with a case of this description. But we must bear in mind that these cases are not often observed until the effects of the poison are manifest, and when of course a deleterious, perhaps fatal, quantity must have found its way into the circulation. An antidote can, therefore, affect only what remains unabsorbed in the stomach; it cannot possibly interfere with that which is already circulating through, and exerting its baneful effects on, the system. When we consider that death usually takes place in less than two hours after the poison has been swallowed, we are reminded that no time is to be lost in the application of our remedies, when dealing with an agent,

> "Whose effect
> Holds such an enmity with blood of man
> That, swift as quicksilver, it courses through
> The natural gates and alleys of the body."

The antidotes that have been brought forward are somewhat numerous—namely, tobacco, and its active principle nicotine; woorari poison; chloroform; hydrocyanic acid; camphor; animal charcoal; tannin; tincture of iodine; and lard or fat; nine in all. The first four of these have their pretensions founded upon a physiological probability, based on the fact of their actions being antagonistic to the visible effects of strychnia on the system. The first, nicotine, was introduced to the notice of the profession by the Rev. Richard Haughton, of Trinity College, Dublin, in a paper read before the Royal Irish Society. Reasoning upon the known properties of strychnia, as compared with those of tobacco, to which they are diametrically opposed, —the former being a very powerful excitor, and the latter as powerful a depressor, of muscular contractions,—he came to the logical conclusion that tobacco ought to be the natural and appropriate antidote to strychnia. In order to prove the truth of his theory, he immersed two frogs in solution of nicotine, which caused complete prostration, or paralysis of motion, and the animals died paralysed in twenty-three minutes. Others he placed in solutions of strychnia, which produced the usual tetanic effects of that drug, and caused death in *four* minutes. He then placed two others in a compound solution of the two poisons. The first frog lived in the compound solution forty-seven minutes, at the end of which time it died. The second frog was kept in the solution ten minutes, and then taken out; but after its removal from the liquid, in thirty minutes it was seized with tetanic spasms of strychnia, in the form of emprosthotonos, but ultimately recovered.

We thus learn from these experiments that for a time the effects of the strychnia were suspended by the nicotine; but that in one case the fatal result was not prevented, and in the other the influence of the strychnia was only deferred. But I do not consider these experiments calculated fairly to test the power of nicotine or tobacco as an antidote, inasmuch as the remedy was given at the same time as the poison, whereas, to arrive at an accurate result, the experiment ought to approximate as nearly as possible to the realities of a case—that is to say, the antidote should not be given until the manifestations of poisonous action are apparent. The only case on record of the results of this agent is one by Dr. Byrne, of St. Louis, Missouri, who, having read the account of Mr. Haughton's experiments, administered an infusion of tobacco, that being ready at hand. This man, who had taken an emetic before Dr. Byrne saw him, and which had acted freely, recovered in twelve hours.

In support of the claims of woorali poison as an antidote for strychnia, we have some experiments of Dr. Harley, of University College, which are very conclusive of the neutralizing effects of these drugs one to the other. In the first experiment, a frog was poisoned with the 500th part of a grain of woorali; three minutes after he had become perfectly insensible, the 120th part of a grain was injected; in five minutes he became tetanic. The second experiment is the converse of the first. A frog was poisoned with 120th part of a grain of strychnia; in three minutes after tetanus was strongly marked, he was punctured with a 500th part of a grain of woorali; in seven minutes the tetanus disappeared. There are yet no recorded cases of strychnia poisoning in the human subject in which this remedy has been used. It has, however, been applied endermically in three cases of traumatic tetanus by M. Vella, in the French Military Hospital at Turin. In the first two, although the patients died, their sufferings were very much alleviated by the muscular relaxation produced by the woorali; the third case recovered under its use. Mr. Spencer Wells has recently also published an account of three cases of tetanus after ovariotomy, in which he used this remedy. His results were precisely similar to those obtained by M. Vella—two died, and one recovered. Its effects being exerted entirely on the voluntary muscles, give it a sort of parallelism to strychnia. To my mind there is a very valid objection to its use, in the fact that its preparation by uneducated savages must be very uncertain, and that when given by the stomach it is at times almost inert, and always uncertain in its operation; and the observation of Dr. Taylor, that "the patient is likely to be killed by the so-called antidote if he escapes from the poison," stands in great risk of being realized in reference to woorali.

The physiological claims of *chloroform* to be ranked as an antidote are not quite so great as those of tobacco and woorali; it is, however, better known to us, and has the *prestige* of having been administered in four cases of poisoning by strychnia, and with good results. The first is noticed in the Medical Gazette for August 2nd, 1850. An intemperate man, aged 40, had swallowed about one or two grains of strychnia. The usual remedies having proved unavailing, chloroform was administered by Dr. Munson, of Boston, U. S. One drachm was inhaled on a silk handkerchief, and we are told "the effect was decisive." Previous to inhaling the chloroform, he could not be moved without the spasms coming on; but immediately afterwards he requested to be laid in the recumbent posture, which did not now excite the spasms. The chloroform was carefully administered for some hours, and the patient rapidly recovered.

The second case is by Dr. Jewitt, of Boston, who states that the patient, a boy about fifteen years old, was kept in a state of partial anæsthesia for four hours and a half, and that as often as the chloroform was withdrawn, the spasms came on, but were immediately subdued by it. The boy recovered well.

The third instance is mentioned in the Pharmaceutical Journal for January, 1857, and is taken from a West Canada newspaper. A city police-officer swallowed, by mistake, four grains of strychnia. Finding out his error, he quickly swallowed an emetic; he subsequently took a second, and a third was administered by Dr. Bly, and vomiting was kept up by warm water. Chloroform was administered during seven hours, after which the spasms ceased.

Another, being the fourth case, was a young woman, who had intentionally taken about two grains of strychnia enclosed in a sweetmeat. After she had begun to feel the spasms come on she repented, and took an emetic of common salt, which caused her to vomit. When brought into University College Hospital, Dr. Hillier, by whom the case is reported, applied the stomach-pump, and chloroform was inhaled to allay the spasms. The symptoms were not severe, and she soon recovered.

The next case I have to notice was cured by the exhibition of chloroform by the stomach. A man was seen by Dr. Dresbach, of Ohio, in twenty minutes after having taken three grains of strychnia. Dr. Dresbach gave him *two* drachms of chloroform, and in less than fifteen minutes after swallowing *it, the* relief is said to have been complete, and he quickly recovered.

The greatest number of recoveries from poisoning by strychnia are those in which *camphor* has been exhibited. There are five in number. The first I find is reported by Dr. Pidduck, physician to the Bloomsbury Dispensary. A dissolute man, a pianoforte maker, of weakly constitution, had been ordered strychnia, in doses of one-sixteenth part of a grain for some neuralgic affection. Owing to an error in compounding, the medicine was prepared in doses of one-sixth instead of one-sixteenth. The first dose produced some twitchings of the muscles. A second was, however, taken, which produced the usual severe spasms from poisonous doses of this drug. Dr. Pidduck found him in great danger, and prescribed five-grain doses of camphor, suspended in almond emulsion, which at once stopped the spasms. The medicine was continued throughout the night, and the man recovered. Two cases are mentioned in the British and Foreign Medico-Chirurgical Review for January, 1857, as having happened in the practice of Dr. Rochester, and communicated to the Buffalo Medical Association, but in one of these cases half a grain of morphia was given. In the same number of the Review, Dr. Richardson, in the report on Forensic Medicine, alludes to the case of a gentleman who had taken two ounces of laudanum, and subsequently two pills containing ten grains of strychnia. Dr. Givens, by whom the case was reported, found him vomiting freely, so that he must have taken an emetic also. Two large doses of tartaric acid were given, followed by camphor mixture, alternating with doses of ether and turpentine. Amidst this variety of remedies the patient recovered. A successful case is also reported in the Lancet for April 25th, 1857, by Dr. Pritchard, of Filey, Yorkshire, in which three to five-grain lumps of camphor were successfully administered. It must be observed, that in this case emetics were given, and the stomach-pump used, with Dr. Marshall Hall's ready method of artificial respiration.

*Animal charcoal* has been brought forward as an antidote by Dr. Garrod, who found it exert a preventive influence on the action of strychnia, as well as on other poisons, in some experiments which he performed on animals. A case in which it was given was related by Mr. W. Chippendale to the Abernethian Society. A strong man took four grains of strychnia, with an equal quantity of morphia. In rather more than half an hour violent spasms came on, and when admitted into St. Bartholomew's Hospital he had lost the use of both his legs. Three or four ounces of animal charcoal, suspended in water, were thrown into the stomach, which was subsequeutly emptied by the stomach-pump. He continued very ill for some time afterwards; the paroxysms were frequent and violent; pulse 150. Chloric ether and brandy were given, and he rapidly improved; the convulsions ceased, he passed a good night, awoke free from convulsions, and was discharged next day.

Of *hydrocyanic acid* as an antidote we have only the evidence of one case, and that not in the human subject. It is reported by Dr. Bewley, who, wishing to poison a mangy cur, gave it half a grain of strychnia. The sufferings of the animal were so severe that he could not bear to witness them, and, in order to put a speedy termination to them, he gave the dog half a drachm of prussic acid mixed with a little milk, which the poor brute eagerly lapped up. Instead of falling down paralysed, as the doctor had expected, the dog got upon his legs, ran away, and recovered.

*Tannin* is the next remedy I shall mention. It has been very recently put forth by Dr. Kurzak, in the Art Journal of Vienna, as the best remedy in poisoning by strychnia, if promptly administered. The recommendation is based upon experiments on animals, but the particulars are not given. Dr. Kurzak, however, was not the first to recommend this agent. In the British and Foreign Medical Review for July, 1842, a case of recovery from poisoning by strychnia is reported by Dr. Sudicke, in which half a grain of strychnia had been taken in six hours. The symptoms became very alarming. Dr. Sudicke ordered ice to be applied to the head, and half a grain of tannic acid every half hour. Twelve grains were taken and followed by decoction of oak bark. Mesner, of Dresden, also recommends decoction of oak bark or galls.

We come now to another remedy, *tincture of iodine*, recommended by Dr. Bennett, of Sidney, N.S.W., who has recorded a case of recovery from poisoning by a paper of Battle's vermin-killer, in which recovery is attributed to the use of this remedy, but upon what grounds does not appear. It is worthy of remark that the lady had taken several scruple doses of sulphate of zinc previously, which acted freely, and the convulsions ceased in twenty minutes after she had vomited.

*Lard* has also been recommended as an antidote by Dr. Pindell, bu' its efficacy is disputed by Dr. Hammond, of Fort Riley, Kansas, who, in a letter to the editor of the American Journal of Medical Science, says: "I have tried Dr. Pindell's antidote to strychnia—lard. I gave two grains of the poison to one dog without the antidote, and two grains to another with the addition of a pint and a half of melte. lard. The best of the joke is, that the latter died in four hours, and the former—a miserable, worthless cur, who doubtless was too mean to die—is still running about in the finest possible state of health. So much for lard."

*Emetics.*—I now proceed to examine the claims of emetics to effect our object—or rather I would say vomiting; and to prove that this is the agent I will draw attention to its effects in some diseases manifesting phenomena not very dissimilar from those caused by the poison under consideration. I allude to those disorders having a paroxysmal or spasmodic character—namely, whooping-cough, hysteria, and infantile convulsions. In all these, in common with strychnia poisoning, we have first, the periodical return of the paroxysm or spasm; secondly, the intermittent and irregular performance of the function of respiration; and, thirdly, a dark and highly carbonized condition of the blood. My attention was many years ago directed to a fact which is no doubt familiar to the profession, that the paroxysm of whooping-cough is almost invariably cut short by a good fit of vomiting, and that the countenance, which before was dusky from the circulation of highly carbonized blood, soon becomes florid and animated; the respiration, which before the fit was laboured and irregular, now becomes free and easy, and the child returns to its play or its meals. In the convulsions of children we have the same irregular, intermittent and imperfect performance of the function of respiration, and the same irregular distribution, generally diminution of animal heat.

Reflecting on the results of vomiting on the paroxysm of whooping-cough, I was induced to try it in convulsions, and, where I could obtain it, have never found it fail in removing the convulsion.

In the allied disease of hysteria, where a very similar condition of the respiratory organs and functions exists, the breathing here being irregular, imperfect, and often at distant intervals, and where the external manifestations very much resemble those we have been considering, I have for many years looked upon vomiting as the remedy above all others to be relied on to remove the paroxysm or fit.

In the cases above mentioned it would be unfair to regard the mere emptying of the stomach as the *ratio medendi*. I have long since come to the conclusion that the return to healthy respiration has more to do with the relief of these diseases than is generally admitted. I believe the experiments of Dr. Harley have led him to the conclusion that strychnia produces death, not by its action on the nerve substance, but on the blood, by destroying its capability to absorb oxygen. This theory appears to derive considerable strength from the fact of the blue or dusky condition of the skin in the subjects of poisoning by strychnia, indicating a highly carbonised state of the blood. It has been suggested by Dr. Brown-Séquard, that in many cases of poisoning the loss of animal heat tends to a fatal result. Now, what is the meaning of this fact but that it is in consequence of the respiration being imperfectly and insufficiently performed, and the circulation through the lungs having been thereby impeded, that the blood is not passing with its accustomed energy through the capillaries, and this tendency to death takes place? A similar condition to this must exist in the cold stage of fevers; and here we give emetics—and what for, if not to bring about, by the action of vomiting, a more complete and full respiratory action, and a more perfect

series of that action, by which the blood is more quickly driven through the lungs, causing an increased absorption of oxygen, and a consequent greater development of animal heat, with its more general diffusion over the body? These results of vomiting, which are self-evident, cannot be the effect of the mere evacuation of the contents of the stomach; they must therefore be accomplished by the physiological action of vomiting. If this be true, then, *we have, in vomiting, a real and true antidote to poisoning by strychnia; and it is by the maintenance of the condition of system thereby induced that we must reasonably hope to combat the two morbid effects of the poison which are said to tend to death—I mean the diminished capacity of the blood for oxygen, and the consequent falling of the animal heat.*

But although I believe we have sufficient evidence to prove that this alone, when freely accomplished, is of itself sufficient to prevent death, yet I would not limit the treatment entirely to its production; for after the evidence we have had of the power of chloroform to subdue the spasms, I think we shall consult the comfort and ease of our patients by its careful exhibition, but I do not think it should be carried to the pitch of producing complete insensibility, nor, indeed, beyond the degree which causes the breathing to become regular. When the sickness has somewhat subsided, we may give camphor, suspended in almond emulsion, or any other fitting vehicle. With a view of assisting to maintain the animal warmth, I would place the patient near a good fire, or keep him well covered with blankets. It is of the utmost importance that the most perfect quiet should be observed by the patient and those about him, since it is obvious that any touch of the surface, or movement of any attendant in contact with the patient, may bring on a paroxysm of tetanic spasm which might prove fatal. During the very severe fit, I believe we derived most timely aid from cold affusion, which appeared to bring an inspiration, and restore the then suspended animation. These remedies, though useful auxiliaries, are quite subordinate to the more important act of vomiting.

When once vomiting has been brought on, we should assist it by copious draughts of warm whater, which will prolong the sickness, and also render it less painful. As regards the kind of emetic to be used, of course we give the preference to those of a non-depressing character; and were I to see another case, I should be disposed to use mustard, as being quicker in its operation than all others. Sulphate of zinc and ipecacuanha, however, afford a very appropriate form.—*Lancet, March* 30 *and April* 6, 1861, *pp.* 311, 335.

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131.—*Nitrate of Ammonia as a Tonic.* By Dr. WALTER COLES.—The attention of the profession has been recently called to the nitrate of ammonia as a means of introducing an increased quantity of oxygen into the system. A female is now under treatment in the second surgical division, in whom the nitrate of ammonia has been used with the most satisfactory results. She had suffered for a long time from disease of the knee-joint, when the limb was amputated in the thigh by Dr. A. B. Mott, in the early part of August. For the first four days after the operation the patient was in a most precarious state, and apparently liable to sink at any moment. She was freely stimulated with brandy and milk punch; but still she continued to decline. Suppuration from the stump was most profuse, and it seemed to be making little or no progress towards reparation. Quinine, carbonate of ammonia, and chlorate of potash, were each in turn tried with no better success. At the suggestion of Dr. Barker, they were now stopped, and nitrate of ammonia, grs. xv., three times a day, administered, while the brandy and ale were continued in small quantities. From the moment the nitrate of ammonia was begun, the stump assumed a more healthy appearance, and her general health began to improve. She expressed herself in the most decided terms in regard to the relief derived from it. The amendment has been gradual but steady ever since.—*American Med. Times, Nov.* 3, 1860, *p.* 311.

## 132.—A NEW SALT OF IRON AND QUININE.

By Dr. WALTER FERGUS, Lecturer on Chemistry at Marlborough College.

[The author states that he has used the following combination of iron and quinine for many years with very satisfactory results. He says:]

Iron and quinine are already represented in so many forms that the addition of a new one may be looked upon as an unnecessary undertaking. And it is most true of medicines in general, that a multiplicity of forms is a great evil; but there are certain points in this new combination which seem to me worthy of attention.

It is generally found that a salt of the protoxide of iron is preferable to one of a higher degree of oxygenation: but it is also difficult to obtain an absolutely permanent salt of the protoxide. Perhaps, without exception, the sulphate is the most practically useful of all the salts of iron, owing to the uniformity of its composition. Of the quinine salts, the sulphate is also the most available for general purposes. It is not difficult to form a simple combination of these two sulphates, but the resulting compound is not well fitted for general use. The addition, however, of a certain proportion of sulphate of magnesia, enables us to obtain a salt which is nearly as soluble as the sulphate of magnesia itself—quite unalterable in the solid state, and forming a solution perfectly clear at first, and remaining so for an indefinite period. The iron has no tendency to a further state of oxygenation; the solution has been agitated with oxygen gas, and kept in contact with it for several days, without the least change. A solution of gallic acid tinges a solution of the salt of a light bluish colour, after the lapse of two or three days, and many substances which produce an inky compound with the salts of iron may be mixed with it without causing any change of colour.

The proportion of the three sulphates which has been adopted, is 80 per cent of sulphate of magnesia, 15 per cent of the sulphate of iron, and 5 per cent of the sulphate of quinine, one scruple containing 16, 3, and 1, grains of the respective salts. These proportions have been found best for general use, and also for the purposes of manufacture. The proportion of quinine may be increased by prescribing an additional quantity, which is readily soluble in the solution of the salt.

One peculiarity is especially deserving of notice: namely, that in this combination, the assisting or adjuvant property of both iron and quinine are remarkably developed, the effect of both, particularly of quinine, being heightened in a very marked manner. At the same time, both of the remedies are less apt to disagree with peculiar constitutions which ordinarily refuse to tolerate either iron or quinine. If the heightened power be borne in mind in prescribing this combination, there will be very few cases found in which it will not be suitable whenever either iron or quinine are indicated.—*Med. Times and Gazette, March*, 16, 1861, *p.* 288.

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133.—*A New Anæsthetic.* By JOHN WILMSHURST, Esq.—[The article referred to by the writer is the rectified spirit of turpentine ordinarily used.]

The first case in which I tried its effects was that of Mrs. H——, matron on board the emigrant ship *Indiana*, of which I was then surgeon-superintendent. About twelve months ago, having exhausted my little stock of chloroform, and the patient suffering from violent neuralgia in the course of the supraorbital nerve, it occurred to me that of the remedies at hand the most likely would be the vapour of turpentine. This I immediately applied, sprinkled on a handkerchief, to the nostrils, similarly to chloroform, and was surprised to find it not merely soothe and allay the pain, but, after a few inhalations, produce a gentle sleep and state of anæsthesia, from which she awoke without any headache or other unpleasant symptoms, and quite free from pain.

I may mention, without going into detail, that I have since tried it in one or two slight but painful operations—as extracting a broken needle from a sensitive part, and in some cases of cramps, convulsions, nephralgia calculosa, &c. Its effect seems to be to allay nervous irritation, spasm, and pain, without deranging the action of the heart, and to produce a calm anæsthetic sleep. The remedy being simple, inexpensive, and easy of application, will, I trust, induce some of your numerous readers, more skilled and with better opportunities of testing its value, to experiment in the direction I have indicated, and to publish the result for the benefit of suffering humanity.—*Lancet, March* 2, 1861, *p.* 227.

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134.—*On the Effects of Chlorodyne.*—By Dr. BUCHANAN WASHBOURNE, Gloucester.—[Dr. Washbourne has observed effects following the use of Mr. Davenport's secret remedy, chlorodyne, which he characterizes as "very terrifying." He afterwards observes:]

I have never seen, but in one instance (a female of leucophlegmatic habit), any greater power over neuralgic complaints manifested by chlorodyne, than I have witnessed over and over again by morphia, hydrocyanic acid, chloroform, &c., and in using any of these last-named remedies we have the advantage of knowing what we are introducing into our patient's system. I have, however, seen the exhibition of small doses (ten minims) in cases of advanced phthisis followed by such overwhelming prostration as will effectually prevent Mr. Davenport's chlorodyne from achieving any wonderful cures in my hands. In another case, that of a lady suffering severely from "tic-douloureux," twenty minims were given: the pain was for a short time relieved, but such a distressing sensation of cerebral fulness took its place that the patient declared she would sooner suffer the misery produced by the disorder than the misery produced by the remedy. For some hours after this dose, involuntary twitches of the arms and legs were witnessed. A strong, powerfully-built man having heard of the wonderful powers of chlorodyne, being worn out with neuralgia, took forty minims. Immediately after he had swallowed them he became unconscious and did not recover his senses for some hours, and felt thoroughly unhinged for several days, to the horror and dismay of his family.—*Med. Times and Gazette, Jan.* 19, 1861, *p.* 79.

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## 135.—CAUSE OF DEATH FROM CHLOROFORM.

By Dr. JAMES PETRIE, Liverpool.

[In some cases, at least, the cause of death from chloroform is the position of the patient during its administration. The usual position is the supine one, with the face upwards, in consequence of which, when the insensibility is complete, the tongue by its own weight falls back, closes the epiglottis, and causes asphyxia. Only in one case has the author seen any dangerous symptoms during the exhibition of chloroform, which he relates as follows:]

The operation being a tedious one, I had left the patient for a minute or two without chloroform, but still unconscious, and had gone to see the progress made by the operator, when one of the house-surgeons anxiously called my attention to her condition, which was one obviously of imminent danger, but from which she quickly recovered by adopting the ready method of Dr. Marshall Hall,—namely, her tongue, which had sunk deeply into the fauces, was drawn out; she was turned on her side, and her chest was compressed intermittingly, when she began to breathe, and the pulse returned to the wrist. After a little time the chloroform was resumed in the altered position without the recurrence of any bad symptom. I have, more especially since this accident, taken care to give the chloroform in a position to obviate any casualty from closure of the glottis in this way.

In a conversation with my friend Professor Simpson, when here a couple of months since, on this subject, I expressed my opinion to him that the very

few deaths which apparently occurred in parturient cases from chloroform might be ascribed to the position of the woman. He told me certainly, among some thousands to whom he had administered chloroform (and the majority of those must have been in labour), he had no fatal case in his practice. Making allowance for the tact with which he no doubt gives it, there, however, can hardly be a question that the immunity from accidents which he has experienced must have arisen mainly from the position of the patients.

My object, it will be perceived, is to bring to the notice of the surgical operator the particular position necessary for avoiding a certain course of danger under the use of anæsthetics: therefore, I would have this matter looked upon as a preventive of untoward effects rather than, as hitherto, one of the means of their removal when they have supervened.

I am aware that in a great many operations on the human body we are obliged to place the patient on the back, but yet, when the lateral position is inconvenient for the performance of an operation, a sitting posture may be adopted, or the head may be turned awry and the tongue thus be prevented from falling back by its own gravity so as to impede respiration.—*Med. Times and Gaz., Dec.* 22, 1860, *p.* 619.

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## 136.—ON THE TREATMENT OF GOUT.

By Dr. C. B. Garrett, Hastings.

[Exhausted as we may almost suppose the subject of gout to be, any suggestion for the relief of those terrible twinges in the great toe will not be unacceptable. The following case is extremely interesting.]

Without entering into any discussion on the pathology of gout, it would appear, from the peculiar manner in which tumefaction takes place, that this swelling does not wholly arise from a congested state of the bloodvessels, nor from the œdematous extravasation of fluid in the tissues; for, in the first place, howsoever high the part affected may be elevated, no apparent diminution in size is effected, nor, indeed, any sensible relief produced, nor is there often any pitting on pressure. It would seem that there is to a great extent an emphysematous puffiness, by which the bones and muscles are loosened from their mutual attachments, as is evidenced by the intense pain produced by lifting the foot off a sofa, and allowing it to hang for a moment, and observing the extraordinary expansion which takes place on putting the foot on the floor; and, indeed, this seems to be corroborated by, and to warrant, the following treatment. I shall best elucidate this by giving a case in point.

An elderly gentleman had gout in the left great toe, with considerable swelling of the foot. The pain was excruciating, and the "twinges" most fearful. He screamed out violently if any one approached him, lest even the dress of a lady should come in contact with the toe. His sufferings were more acute than any, I think, I had ever witnessed. I suggested to him that by making steady pressure on the affected part he would gain magical relief. Had I proposed to cut his leg off he could not have been more startled. However, being a man of resolute mind, he permitted me to place the palm of my hand gently, yet firmly, against the joint, and was not only astonished, but delighted, to find the relief it afforded. He then consented to the use of a bandage, which was thus applied:—Having encased the foot in cotton wadding, so as to make the pressure more easily applied, I passed an elastic bandage twice round the ankle, crossing it, in the usual manner, over the dorsum of the foot, under the instep, and round the heel. I then carried it onwards, and gradually included the ball of the great toe and the toes generally. The pressure being progressive and towards the seat of disease diminished the nervous sensibility of the part, and lessened the apprehension of the patient.

Immediately after the bandaging my patient fell asleep for the first time for four days and nights, and slept uninterruptedly for eleven hours. The

other part of the medical treatment was of the usual character. But there is one other remedy which I rely on more than anything else, and that is, a large mustard plaster over the whole of the abdomen for fifteen or twenty minutes, to be re-applied as often as the skin will bear it. It instantly gives relief to the gout, it relieves dyspepsia, invigorates the digestive powers and the functions of the liver, as well as promotes a healthy action of the bowels, even when ordinary aperients have failed.—*Lancet, April* 20, 1861, *p.* 393.

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137.—*Chlorate of Potash and Glycerine as a Topical Disinfectant.*—Experiments instituted at Bicêtre under the direction of M. Martinet, have demonstrated remarkable disinfecting properties in a mixture of chlorate of potash and glycerine according to the following formula: Chlorate of potash in powder, 2½ drachms; glycerine, 3 ounces; mix. This mixture has been shown, by repeated trials, to present,—1st. A marked disinfecting power, due perhaps to the change which it produces in the secretion, and the mode of action of the wound; 2nd. The property of giving the pus, even when of a serous kind, a greater consistence, often like cream. This result is perhaps, according to M. Martinet, a physical effect of the affinity of glycerine for water, which it subtracts from the pus; but is partly due to the favourable modification which is produced in the suppurating surface. He is inclined to think that the preparation of glycerine and chlorate of potash may, by thickening the pus, tend to prevent the occurrence of purulent or putrid infection, which generally takes place in connection with suppuration of a serous and unhealthy character. An advantage of the glycerine is, that it prevents the dressings from sticking to the edges of wounds. According to M. Martinet, the glycerine chlorate of potash is not adapted for wounds or sores of a bright red colour, nor for those that are recent or of healthy appearance.—*Edinb. Med. Journal, March* 1861, *p.* 834.

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## 138.—ON THE TREATMENT OF THE DROWNED.

By Dr. Christian, Medical Officer to the Royal Humane Society, and Dr. Sharpey.

[The Royal Humane Society at present use Dr. Silvester's method, whilst the National Life-boat Institution employ Dr. Marshall Hall's "Ready Method." Dr. Christian, in a paper read before the Royal Medical and Chirurgical Society, says:]

After giving the Ready Method a full trial in about 15 cases, the very intelligent superintendent, the boatmen, and the author become so satisfied of its inefficiency to restore animation, and of the difficulty of properly carrying out the manipulations, that he felt himself justified in representing those facts to the committee, and in adopting the plan recommended by Dr. Silvester, which he believed in every way to be superior, more manageable, less likely to injure the patient, will fill the chest with and expel air from it more fully, and will not force the contents of the stomach upwards and in the way of respiration.

The following are the directions for treating the asphyxiated at the Receiving-house, Hyde-park:—

"Wipe the mouth and nostrils directly the body is taken from the water.

"Use Dr. Silvester's method; at the same time let the body be taken as quickly as possible to the Receiving-house, and place it in the bath up to the neck.

"Raise the body in twenty seconds from the water, and dash cold water against the chest.

"Pass ammonia under the nose.

"Use again Dr. Silvester's method, and the inflating apparatus if it fail.

"Remove the body from the bath, and rub the surface with dry hot towels, perseveringly continuing the other treatment."

After many experiments the author had come to the conclusion that infla-

tion of the lungs by Dr. Silvester's method or by the Society's apparatus is the first remedy, and the shock of the warm bath the second; that after eight minutes' complete submersion recovery is hopeless, and that when ten minutes elapse after being taken from the water without any effort at respiration it is equally so. On the subject of the warm bath, which has excited so much discussion as a remedy, he remarked that it must be understood that it is used as an immediate and powerful excitant; and it had so frequently happened (twice while he was actually present) that a person brought in as asphyxiated, who to the bystanders was apparently quite dead, immediately on being placed in the bath gave the sob or gasp which is the precursor of respiration, that it might be boldly stated to be a most valuable adjunct to treatment, and, properly managed, in no way pernicious. He concluded by citing a letter from Sir Benjamin Brodie to the Royal Humane Society, confirming the preceding conclusions.

Dr. SHARPEY having had the honor of presenting the paper to the society, could not let it pass without remark. His object was, however, rather to elicit opinions on the questions brought forward than to decide on the question, as he had had no practical experience on the subject. Dr. Christian had brought the subject before the society in order that both methods might be well considered, as they were both still *sub judice*. He would observe, with reference to the method of Dr. Marshall Hall, that he had on one occasion spoken favourably of it, but had seen reason to alter his opinion after more mature consideration of the subject; and after hearing the practical experience of the Royal Humane Society, Dr. Sharpey considered that Dr. Marshall Hall's method could only claim one advantage, and it was not clear that it had even that. This supposed advantage is that the tongue falls forwards and thus does not embarrass respiration. He thought that Dr. Silvester's method attained this object without any of the disadvantages of the Ready Method. The disadvantages of the Ready Method were several. It must be remembered that a body submerged for some time is practically a dead body, and serious mischief has not unfrequently arisen from rough handling. Again, the constant turning of the body renders it very difficult to apply warmth, or carry out the other auxiliary means systematically; but, above all, it does not even fulfil its first object, of changing the air in the chest. Dr. Marshall Hall cited experiments in support of his view; but the want of precision in making them is very striking. He (Dr. Sharpey) could not attach any importance to the results of experiments so conducted. Dr. Silvester has repeated them in a more precise way, and could not get a displacement of more than one cubic inch of air. It would be asked, however, what answer should be made to the many statements of the success of the Ready Method? He could reply, that many of them were in cases of still-born infants, a part of whom, he believed, would recover without any assistance if left to themselves, or at least by very simple means. Then, again, as Dr. Silvester states, there is no air in the lungs of infants. In reference to adults, he (Dr. Sharpey) believed himself right in saying that in many of the cases of recovery after submersion respiration commences spontaneously as soon as the patient reaches the air. If in such cases Dr. Marshall Hall's method is begun at once, it would be unfair to give the credit solely to it. May it not even be, as suggested by Sir Benjamin Brodie, that recovery often follows, not from the means used, but in spite of them? Dr. Sharpey considered that there were just grounds for reviving the old method of insufflation, which was, he thought, given up without sufficient reason. The objection to this plan was that it produced emphysema; but this, as he knew by experiment, was from forcing the air in too quickly, and in too large quantity. Dr. Sharpey then alluded to the various instruments which might be used in inflation. He recommended an elastic bottle of a capacity of about twenty inches. In reply to the objection that insufflation could not always be practised for want of apparatus, he would remark that there could be no reason why the inflation should not be carried on from mouth to mouth. He believed that emphysema, in reference to this class of cases, was a bugbear. Dr. Christian had proved that in unsuccessful cases, in which insufflation had

been tried, no emphysema had been found at the autopsy; and a friend had informed him (Dr. Sharpey) that emphysema was never found in the lungs of still-born children who had been unsuccessfully treated by insufflation at the Edinburgh Maternity Charity. The objection that the air would be deficient in oxygen was not practically valid, as the operator might by several deep inspirations quite change the air in his lungs, and then by emptying his chest quickly the air would not have time to undergo much change. There was an outcry against the warm bath founded on mere speculation. The Royal Humane Society had found it very efficient. The experiments of Edwards and Brown-Séquard were made on puppies and kittens, and were not, he thought, fairly to be brought into comparison with the use of the warm bath in cases of suspended animation.

[We here reprint, from "*Retrospect*," vol. xxxviii., the Silvester Method of resuscitating persons apparently drowned.]

"The *Silvester Method* is 'a simple imitation of natural deep inspiration, and is effected by means of the same muscles as are employed by nature in that process.' In deep inspiration, we lift the ribs and sternum by the pectoral and other muscles which pass between the chest and the shoulders; so, in the 'new method,' the ribs and sternum are lifted through the intervention of the muscles, *by steadily extending the arms up by the side of the patient's head.* In this way the cavity of the chest is enlarged, a tendency to a vacuum is produced, and a rush of air immediately takes place into the lungs.

Expiration is brought about by simple compression of the sides of the chest by the patient's arms. Thus 'the arms of the patient are to be used by the operator as handles to open and close the chest.'

The following rules are to be observed in inducing artificial respiration by this new method.

1. *Position.*—Place the patient on his back, with the shoulders raised and supported on a folded article of dress.

2. *To maintain a free entrance of air into the windpipe.*—Draw forward the tongue, and keep it projecting beyond the lips. By raising the lower jaw the teeth may be made to hold it in the proper position.

3. *To imitate the movements of respiration.*—Raise the patient's arms upwards by the sides of his head, and then extend them gently and steadily upwards and forwards for a few moments. (This action, by enlarging the capacity of the chest, induces respiration.)

Next turn down the arms, and press them gently and firmly for a few moments, against the sides of the chest. (Forced expiration is thus effected.)

Repeat these measures alternately, deliberately, and perseveringly, fifteen times in a minute." (p. 300.)

[Dr. Marshall Hall's Ready Method, as published by the National Lifeboat Institution will be found in "*Retrospect*," vol. xxxviii. p. 299.]—*Lancet, Feb.* 2, 1861, *p.* 111.

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## 139.—THE TURKISH BATH.

By Dr. R. H. Goolden, Physician to St. Thomas' Hospital.

[The following are extracts from a most able letter written to the authorities of St. Thomas' Hospital, by Dr. Goolden, in answer to a circular addressed to the Medical Staff, requesting their opinion upon the Turkish Bath. Speaking of the effects observed by him in a bath much resorted to by the lower orders, the writer observes:]

The most marked relief was found in cases of gout, rheumatism, periosteal nodes, and sciatica. Some cases of Bright's disease were more relieved by the sweating than I had ever seen; but they produced a most distressing effluvium in the bath, so that it was necessary to change the entire atmosphere of the hot room. Eczema was soon relieved; psoriasis much benefited, but required a longer and more frequent bath. Bronchitis was relieved at once in many cases; but it required several baths to cure those cases which

were of any long standing. Pleurisy, the only case I saw, received no benefit, and suffered very much distress. Several cases of phthisis said they felt relief; the cough was quieted, and the night sweats relieved, but I saw no permanent benefit. If any injury supervened, I was not likely to be made acquainted with it, as the patients would not return to the bath; but I should fear that where cavities are formed, hemorrhage or bleeding from the lungs would be very likely to result. My inference is drawn from a single case, to which I shall allude presently. I saw a number of children suffering from scarlet fever who were brought down daily. I will not pledge myself that the bath exerted any beneficial influence; but the cases, many of which were very severe, *all* recovered rapidly, and left no serious results. One case of typhoid fever was much relieved. This is what one might expect, because this form of fever is unknown in the tropics; but what struck me as most remarkable was the influence of the hot room in quieting the circulation in some cases of palpitation of the heart. Some cases that I saw were decidedly injured by the bath; but they were such as one would expect would be so injured. One was a case of diseased liver, who suffered pain in the right shoulder, and supposed that pain to be rheumatic; but, on the other hand, *I* have seen cases of severe dyspnœa and sluggish fever most satisfactorily relieved by it.

I have been speaking of cases concerning which I had no responsibility. I was allowed to watch, but had no right to interfere.

In my private practice I had prescribed these baths, and I have found them to afford a very great relief in some cases of chlorosis (green sickness), and in a form of chronic inflammation of the stomach common to young women. Common catarrh is cured at once; and the weekly use of the bath diminishes, or altogether suspends, liability to the attacks. Common catarrh includes what are called colds in the head, quinsy, sore-throats before suppuration, the common winter coughs, and some forms of diarrhœa. Pains in the muscles after excessive and unusual fatigue are removed; and those who suffer pains in the seats of old injuries upon slight changes of the weather, lose those pains entirely.

The influence on gout is very remarkable. During the acute attack the pain entirely leaves the patient when he has been some time in the hot chamber, and during the perspiration; but the pain usually returns as he cools down afterwards, though with considerably less intensity, and each bath perceptibly improves the patient. I find, however, that draughts of a solution of bicarbonate of potassa and nitre greatly facilitate the effect of the bath.

A near relation of mine, who had led a hard life of exposure to various climates and great fatigue in the Queen's service, and had retired on account of ill-health, suffered attacks of angina pectoris, became greatly emaciated, dropsical, and was seized in addition with an attack of bronchitis. His appetite entirely failed him; and Dr. Watson and I considered that he was rapidly sinking. He had had no regular sleep for several weeks, waking up in a state of alarm as soon as he dropped off to sleep; and he was then unable to walk across his room. In this state, with Dr. Watson's approval, I took him to the bath. (He had had baths both in Turkey and Russia). I had great difficulty in getting him in and out of the carriage, and in supporting him down the stairs. When he was in the hot chamber, he soon broke out into a profuse sweat, and said he had not felt so comfortable for a long time. He remained there above an hour, and I had great difficulty in persuading him to leave it, he felt so much relief. The dyspnœa (difficult breathing) was gone; the palpitation of the heart had subsided to a natural action; he rode home without inconvenience from Bell-street, Edgeware-road, to Chester-square, Pimlico; he enjoyed his food, and slept incessantly for nearly three days and nights. From that time he rallied; and although still subject to attacks of angina, is in the enjoyment of comparatively good health, and capable of attending to his private business.

A clergyman (a country rector) complained to me of rheumatic pains, and *susceptibility* to take cold on any slight exposure, which interfered with his *clerical* duties. I recommended the bath to be used twice a week. Evans

persuaded him to use it every day during his stay in London, and he persevered in its daily use, although all rheumatism was gone. It so happened that he had been a midshipman in early life, and had received a stab with a knife from a Chinaman in some former contest at Hong Kong. The blade wounded the lung, but the wound was healed, and he never afterwards suffered the slightest inconvenience from it. However, whilst he was taking these baths, he was seized suddenly with splitting of blood. The old wound in the lung re-opened, and he nearly died from the hemorrhage; but he recovered.

This is the only case in which I have seen any serious consequences follow the use of the bath. It is important, however, to record it, because it would indicate some danger in cases of advanced phthisis, where cavities have been formed. I have several times seen boils follow the employment of the bath, but they never caused more than a temporary inconvenience.

I have given a fair and not over-stated account of the results of the bath used as a remedy for disease—sadly defective, I confess; but I hope to complete it by observations made in this hospital when the governors will afford the opportunity. There are, however, some points to be touched upon with respect to its construction and use.

The hospital bath should consist of several chambers; a hot room (150°), a tepid room (100°), and an ante-room, with a supply of hot and cold water, so arranged that the bather can have a shower bath or douche at any temperature. There should also be a tap of cold water supplied to each room. The rooms should be warmed by a flue, not necessarily under the floor. In attending to this matter, much saving may be effected without lessening the efficiency. The floor should be of brick, and the walls may be of cement, brick, or Dutch tiles: and the rooms must be so constructed as to admit of perfect ventilation. The estimated cost of fuel is about 1s. 6d. a day.

It requires some care in giving the bath for the first time, because the skin does not always readily respond to the heat by perspiration, and the patient experiences a sensation of faintness. The faintness is, however, more apparent than real, for the pulse keeps up, and the distress subsides as the skin begins to act. Whenever this distress is felt, the patient should have some cold or warm water thrown over the skin, or even be allowed to go for a minute into the cold air. They *never* take cold.

The atmosphere in the hot room should *not* be perfectly dry, otherwise it causes much distress to some persons, and the perspiration is retarded; for the skin must be moist to perspire. The air cells of the lungs also require moisture for the respiratory action: otherwise the patient would be suffocated, even in pure oxygen, as the fish dies in the atmosphere. Too rapid an evaporation therefore is to be avoided, and likewise an atmosphere saturated with vapour at a high temperature is unfitted for respiration, because the vapour displaces too large an amount of air. Mahomet has introduced into his bath a small jet of steam, which is easily regulated, and determines the dew point at its proper degree.

If you hear some of the advocates of the bath talk about its action on the human organism, you will probably hear much questionable physiology. They tell you that the skin does the action of the lungs, and breathes. I believe this to be a fallacy; but of this I am certain, that the skin was designed by nature to be a great organ for excretion. In the purest perspiration there is a large amount of urea, the same material so largely eliminated by the kidneys. This is a fact I know from chemical analysis, and it explains the great relief produced by the bath (i. e., the air bath) in cases of Bright's disease, where the urea retained in the blood is producing poisonous effects; it also accounts for the strong and offensive smell. This same urea, resolved into carbonate of ammonia, produces the distressing smell of a crowded room, and the carbonic acid of that carbonate of ammonia has misled some of our physiologists to believe that it is the result of cuticular respiration.—*Lancet*, *Jan.* 26, 1861, *p.* 95.

## 140.—THE HOT-AIR BATH.

By Dr. B W. Richardson, M. A.

From the use of the heated air bath as a therapeutical agent to its use as a social enjoyment or luxury, is a wide step, and a step, which I for one, hold back from taking. It seems to be the misfortune of this remedy, that its administration is for a time attended by a sensation of great pleasure and satisfaction. This is not a peculiar feature of the heated air bath. In the palmy days of blood-letting, when every one was periodically bled, the argument in favour of the practice was, that it produced for a time a lightness and an agreeable feeling. I absolutely knew a man, who for years had been cupped once a month regularly, for no other reason than that the operation was followed by sensations of lightness and ease. Persons who are recovering from a long and depressing illness, or who take purgatives, experience the same agreeable feeling. In the respiration of nitrous oxide gas, and even in a certain stage of opium-narcotism, the same mental liberty is experienced. But what is the meaning of this sensation? Is it one of power? I believe not. I believe it is simply a transitional decline, either from a state of artificial exaltation to the natural life, or of decline from the natural life to a condition below it—conditions of unconsciousness and temporary or absolute oblivion; even in the passage to death itself, there is a transitional stage of pleasure, if physical local pain be not present to interfere. I take it, then, that this luxurious state, as extorted by the heated air bath, is one of depression; a state, perchance, valuable in disease, and in health harmless as a mere experiment, but ruinous when driven into an intemperate and constant habit.

I deny that this bath can ever take the place of exercise, or in any way supplant exercise. I urge that in all cases the time required for the bath in healthy men were better spent in digging, walking, rowing, the cricket-field, the skittle-ground, or the rifle-drill; and I regret to see even an attempt to introduce this luxury as a substitute for these occupations. I look back into history, and I am more fully preserved in this belief. *Turkish* and *Roman* are words of evil omen. I recall the histories of Turkey and Rome. I recall that in the periods of the rise of these nations a ruler lived in a cottage and tilled the ground, or a caliph clothed himself in an undressed skin, and washed not once a week. I recall that when these nations reached their glory they were more civilized, but yet not luxurious; and, lastly, I recall that when these great nations collapsed, and were beaten by inferior tribes, luxurious tastes had crept in and destroyed them, whereof this bath was the most prominent and enticing of all.

I predict that it will be the same here under the same principles. I predict that, whenever Englishmen give up the active occupations I have named, and in slippered pantaloon luxuriate daily in a bath to rid themselves of the products of exertion, then this country will have passed the zenith; then there will be no great hero to bid every man do his duty, no man to do the duty, and no England for which the duty should be done.—*British Medical Journal, Feb.* 2, 1861, *p.* 114.

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## 141.—THE LAMP-BATH.

By Dr. Charles Taylor, Nottingham.

Apropos to the discussion which has recently occupied a place in your columns on the Turkish baths, permit me to call the attention of your readers to a very simple method of exciting the emunctory function of the skin, in my opinion quite as effectual, and possessing advantages peculiarly its own. I allude to the lamp-bath, which is thus applied. The patient, *in puris naturalibus*, is seated on a common wooden chair with his feet upon a low stool, the body is then enveloped in two or three blankets, the head being excluded, and a large spirit-lamp placed under the seat; in a few minutes the skin be

comes moist, and in about a quarter of an hour the perspiration streams down the body and is collected in beads upon the face; this is kept up so long as may be considered necessary, the patient meantime drinking one or two tumblers of water, when the blankets are rapidly removed, and he is subjected to a douche of two pails of cold water, and dried with much friction, after which a smart walk in the open air may be taken. The process does not occupy more than three-quarters of an hour, and may be applied at any time in the invalid's bedroom. I believe the profuse perspiration thus excited, and which may be carried to any reasonable extent, effects all the good that can be accomplished by the *calidaria* of the Turkish baths, while its extreme simplicity and portability ensure for it advantages as a therapeutic agent that those institutions can never command; among the more important of which I would enumerate the fact that during the whole process the patient breathes cool air instead of that of an apartment heated to an extent necessary to produce such an effect, and indeed may, as I have seen in the summer time, be placidly seated regarding a beautiful landscape from an open window during its administration; thus the ill effects of respiring hot air experienced by many even in over-heated rooms, such as difficulty of breathing, violent palpitation, giddiness, faintness, &c., are all avoided; while the fact that it is used in the patient's own apartment, and may at first be moderated to any extent that may suit his fancy, materially lessens the risk of disagreeable nervous symptoms very likely to be experienced in invalids; who, moreover, are frequently unable or unfit to visit the public baths, or unwilling to expose their infirmities to the gaze of strangers, who, I may observe, *en passant*, would be little likely to patronize establishments where they were liable to meet as "companions of the bath" persons suffering from lepra, psoriasis, dropsies, influenza, &c.

I have seen the above process attended with the very best effects in some cases of obstinate skin disease, in rheumatism, œdematous swellings, catarrh, syphilis, albuminuria, torpor of the liver, &c.; and have more particularly noted its good effects in cases where individuals, from leading a sedentary and town life, had grown fat, pallid, flabby, and altogether out of condition. Such speedily lose fat, and gain muscle: the complexion clears; the colour heightens; the skin becomes smooth and polished. They experience a feeling of lightness and buoyancy to which they have long been strangers, and soon add to the appetite of a wolf the digestion of an ostrich. It is seldom omitted in the treatment of persons subjected to what is known as the water-cure, and has been employed for years in the hydropathic establishments to which used-up men about town notoriously resort, and with the best result, to recruit their injured health and shattered frames. By fixing a small saucepan, containing water, over the lamp, it is converted into a vapour-bath, medicated or not; and may be used for sulphureous and mercurial fumigations.

I must confess that I cannot see the force of the objections that I have heard urged against the employment of this simple remedy. We are told—1. That the patient is surrounded with an atmosphere deprived of its oxygen and loaded with carbonic acid, the product of the combustion of the spirit of the lamp; and that this, by interfering with the respiratory function of the skin, must have a most deleterious effect; 2. That the atmosphere surrounding the patient is stagnant, and does not circulate; and 3. That it is an advantage for the patient to breathe the heated air, as was done in the baths of the ancient Romans.

With regard to the first, it appears to me to be of very slight importance if the air were loaded with carbonic acid, as the respiratory function of the skin, while bathed in perspiration, which is coursing on all sides in little rivulets, in quantity sufficient to saturate the blanket or cushion on which the patient is seated, and drops from the footstool to the floor, must be reduced to nothing; but I cannot admit that the patient is surrounded by such an atmosphere, as the specific gravity of the carbonic acid would ensure its falling below the level of the chair on which the patient is seated. In answer to the second, I contend that the air surrounding the patient is not

stagnant, but circulates freely; cold air, according to a natural law, rapidly taking the place of the hot as fast as heated, which must necessarily be the case, unless the patient were hermetically sealed up, when the combustion of the lamp could not be supported. With regard to the last, I own that I am skeptical as to the benefit to be derived by a delicate invalid from inspiring air hot enough to cook a chop.—*British Med. Journal, Nov.* 24, 1860, *p.* 927.

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## 142.—ON A CASE OF PROPTOSIS, GOITRE, PALPITATION, &c.

By Dr. C. Handfield Jones and Dr. C. J. B. Williams.

[The following case was recently brought before the Royal Medical and Chirurgical Society in a paper read by Dr. Handfield Jones.]

C. B., a woman aged forty, had been on two occasions under his (Dr. Jones's) care, suffering from excessive prostration induced by over-work, proptosis, vomiting, cough, more or less palpitation, and thyroid enlargement. On the first occasion the throbbing and enlargement of the thyroid were so great that the question of ligaturing the arteries was entertained. The debility was so marked that her life was in considerable peril. Iodine internally and externally had no effect in diminishing the goitre; leeches gave very temporary relief; but the application of ice was very beneficial, reducing the volume of the gland and checking the throbbing of the arteries. In the first attack nitrate of silver was eminently serviceable in allaying the irritability of the gastric mucous membrane, and then tonics were administered with great advantage, so that she was comparatively restored to health. In the second attack, which occurred at the interval of about a year, the free administration of opium at first, and afterwards of strychnine, quickly restored her. Country air had always been of very marked benefit. The proptosis disappeared or greatly lessened as she improved in health. The view taken by the author was that the fundamental malady was debility, especially of the nervous system, which, by affecting various vaso-motor nerves, gave rise to the several symptoms. Thus effusion behind the globe would cause proptosis; hyperæmia and increased action of the cell elements of the thyroid would produce goitre; and paresis of the vagi would give rise to palpitation and vomiting. The benefit derived from tonic remedies strongly corroborated this view.

Dr. C. J. B. Williams said he knew of no class of cases more curious, or more peculiar in their prominent symptoms, than that referred to in the paper. There was none in which the effects of treatment were more striking. At first, these cases seemed to present all the signs of excessive arterial action, requiring depletion, sedatives, or other lowering treatment; but we now knew that they were only to be combated by powerful tonics and general support to the system. Their pathology had at one time puzzled him (Dr. Williams) much. Many cases had been presented to him under the garb of heart disease or consumption. The physiognomical character of the disease was too remarkable to be mistaken. The prominence of the eye was striking: the eyeballs projected beyond the brows, sometimes to such an extent that the eyelids did not close during sleep. The vessels on the forehead, face, head, and neck, were more prominent, throbbing, and larger than natural. This feature led him to regard the pathological character of the disease as consisting mainly in a kind of aneurismal varix of the thyroid. Enlargement of vessels would account for the prominence of the eyes, which were also, probably, in some cases rendered more prominent by effusion into the orbit. The whole cerebral circulation was in the same state of enlargement, producing excitement and other symptoms of disturbance of the brain. This state depending on diminution of nervous power, and not on increased power, explained the use of tonics in the disease. The action of tonics was remarkable. It might be necessary in some cases in the first instance to *allay* pain and excitement by the use of sedatives; but it was surprising how *well* the patient bore tonics when so much excitement prevailed. He had

found the best tonics to be those of a more astringent kind, such as the perchloride and the phosphate of iron, which acted almost as specifics in the disease. Formerly he had been in the habit of using the nitrate of silver, and other of the milder tonics; but little benefit ensued until the more decided course of action was persevered in. This, with the occasional use of narcotics at night, highly nutritious food, pure air, and a careful husbanding of the strength, soon effected a beneficial change. He related the case of a lady to whom he was called in the early stage of this affection (he might here remark that all the cases which he had seen, occurred in females). This patient was highly nervous, and suffered from great excitement and pain of the head; the eyeballs seemed starting from their sockets. Temporary relief was obtained by the exertion of pressure on the eye, and binding a handkerchief tightly round the head. There was enlargement of the arteries of the forehead, face, and neck. Was this a case for tonics? The answer was in the affirmative. Sulphate of iron with an excess of sulphuric acid was ordered. Under this, with a local application of Fleming's tincture of aconite, the patient perfectly recovered in a few days. He inquired if any member had examined, post mortem, many of these cases; and suggested that the arteries of the brain and the orbit should, when possible, be carefully scanned.—*Lancet, Dec.* 8, 1860, *p.* 562.

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143.—*Simple Mode of Sedative Hypodermic Inoculation.*—M. LAFARGUE has published in the Bulletin de Thérapeutique (Jan. 15 and Feb. 28, 1861) two articles on these inoculations, and advises the use of a little instrument composed of a hollow needle and plug. By means of the latter, a moistened powder, composed either of atropine or morphine and powdered belladonna, is forced into the areolar tissue, the latter taking up the substances very rapidly. In the absence of the regular hypodermic syringe, or the instrument just mentioned, M. Lafargue thinks that neuralgia may be relieved by using an ordinary lancet, as is done in vaccination. The powder should be placed in a watch-glass, and moistened with a drop of water. In this kind of paste the lancet is to be dipped, and from two or three to a dozen punctures be made over the course of the nerve. The operator should each time be careful to turn the lancet in the little wound made by the instrument. If this mode is found to answer, it will soon recommend itself by its very great simplicity.—*Lancet, March* 30, 1861, *p.* 330.

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## 144.—ON TRACHEOTOMY.

By C. F. MAUNDER, Esq., Assistant-Surgeon, London Hospital.

In the performance of tracheotomy, the introduction of a canula under circumstances requiring its use is usually deemed to be the most difficult step in the operation—a difficulty due to the constant and rapid elevation and depression of the trachea during respiration and deglutition. The difficulty is especially felt in children, in whom the windpipe is small and yielding. To overcome this obstacle I would beg to suggest a procedure which will facilitate the completion of the operation.

The trachea having been incised longitudinally, the operator should insert the point of a *double* hood through the incision into the windpipe, and hold the latter elevated and fixed; he should then slip up the clasp and allow the halves of the hook to separate by their own elasticity, and so to widen the slit in the trachea; this done, the canula may be passed into the tube with comparative ease, and the hook withdrawn.

Should any difficulty be experienced in the attempt to open the trachea after division of the softer tissues, the organ may be fixed in the adult by holding the cricoid cartilage firmly with the finger and thumb, or by a sharp hook inserted into the latter cartilage through the upper angle of the wound. In the child the sharp hook should be used.

Although a double canula be used and the inner one be removed at intervals, cleaned, and replaced, still mucus becomes inspissated, and, adhering to the extremity of the larger tube, offers a serious obstacle to respiration. This inconvenience may be remedied, while the inner tube is being cleaned, by the careful introduction of a small elastic catheter—well warmed, so as to soften it, and oiled—quite through the larger tube just into the trachea. By this means a passage is cleared and the obstruction is removed.

Again, although a double canula be used, mucus collects at the further extremity, and cannot always be dislodged either by cleaning the inner tube or by a catheter or feather; both must therefore be removed and cleaned, or be replaced by others. This necessity occurred to me in a case recently under my care; and in order to obviate the difficulty anticipated on re-introduction of the cleaned tubes, I first removed the inner canula, and then passed a small elastic catheter, well softened in hot water, through the outer tube into the trachea, and, maintaining it there, withdrew the tube over the catheter, and, as soon as it was cleaned, passed it back again along the catheter, still in the trachea, till the former occupied its original position; the catheter was then removed from the trachea.

The catheter served both as a tube by which respiration could be sustained, and also as a guide for the re-introduction of the canula, after the manner of Wakley's catheters.—*Lancet, March* 16, 1861, *p.* 262.

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## 145. A PECULIAR SYMPTOM PRODUCED BY THE USE OF TOBACCO.

By Dr. JONATHAN OSBORNE, Physician to Mercer's Hospital, Dublin.

[The following passage we extract from an able paper by Dr. Osborne, on the "Involuntary actions of Voluntary Muscles." Speaking on the subject of the effects of tobacco-smoking, he observes that there is one peculiar symptom which he has never seen described elsewhere.]

It is this: the smoker feels a pain, dull, and, although not acute, yet distressing, from the constant sense of oppression which attends it. Its seat is behind the breast bone, and rather tending to the left side. It is neither aggravated nor diminished by full inspiration, nor by any change of posture; comes on usually in the afternoon, but irrespectively of meals, and sometimes increases in severity during the night, so as to interfere with sleep, yet almost completely vanishes before morning, and this without any appreciable disturbance of the circulation, respiration, or digestion. My attention was first directed to it by a gentleman, at that time attending the clinic at Sir Patrick Dun's Hospital, who having contracted the habit of smoking, suffered much from this pain, which resisted every remedy till he gave up his pipe altogether. Within less than a month afterwards it ceased, and never returned. I have subsequently met several similar cases, and in some of these ordered only the most insignificant medicines, but always insisted on a total abstinence from smoking, and in every instance, as far as I have been able to ascertain, with uniform success. This affection, in all my cases, occurred before the middle age of life, did not appear to be produced by any one kind of tobacco more than another, attacking indifferently the votaries of Cavendish and Mundungus. From the vagueness of the seat of pain and its transitory character, it would appear to be a semi-paralytic, and consequently a neuralgic, affection of the central and anterior portions of the diaphragm.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 291.

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## 146.—ON CYSTIC DISEASE.

By Dr. MAURICE H. COLLIS, Surgeon to the Meath Hospital.

Of growths which are not cancer, but which equally with it have their origin in the lymph-cell, and are interstitial or infiltrating, there are almost

as many varieties as there are cases. After reading most of what has been written about them, and having seen a good many, I am satisfied that this whole class may, for practical purposes, be included in the following formula:—"*The nearer in form and power of development that the constituent cells of a tumour are to the healthy lymph-cell, the more innocent is the tumour: the further removed, the more destructive.*" To expand this a little, and make it intelligible, we find the healthy lymph-cell small, circular, slightly granular, with a little nucleus, and developing into a fibre. Our simplest tumours are composed of cells scarcely, if at all, to be distinguished from the above; and these white fibrous, or desmoid tumours, are the most innocent possible growths as a general rule. A stray exception may occur, now and then, to prove the rule. We then come to fibroid, fibro-nucleated, recurrent fibroid, fibro-plastic, fibrinous tumours, named according to the fancy of writers, who recognize alike their similarity to simple fibrous tumours, and their divergence from them. These are of variable malignancy; they are of as variable minute construction. Not only do their constituent cell-elements differ more or less in form from the primary lymph-cell, but they also differ in power of development. Some remain always as cells, and never develop into fibres; these are the most recurrent. Some make attempts at development, and hence the caudate cell of various form; some appear as nuclei, only without external cell-wall. Again, power of development into fibrous forms is quite different from active reproduction; generally it is not associated in the same cells. The recurrent tumours are masses of rapidly-produced cells, or nuclei, with no attempts at formation of fibre.

Of these I have seen many examples, none more remarkable than the case of a man from whose shoulder such tumours have been removed, at least twenty times, by Professors Rawdon Macnamara, senior and junior, and by other surgeons. My friend, the present Professor of Materia Medica to the College of Surgeons, knowing the interest I took in such subjects, kindly gave me an opportunity of seeing the tumour on one occasion. It was a mass of most elementary cells, low in organization, active in reproduction, and was the most recurrent tumour I ever heard of. There is some tendency to this form of growth in all secondary tumours, whether glandular or local relapses.

Cancer itself can be brought under the above law. It is no heterologous or parasitic formation. It is simply a monstrously abnormal plastic growth; its cells differ as widely as possible from the healthy type. In acute cases they are rapidly produced, making scarcely an attempt at development, and die off with rapidity; in schirrus they are formed more slowly, and in much smaller numbers, live longer, and make some attempt at caudation, but they are still farther removed in form from the typical cell of healthy tissues.

The more I think over the subject of morbid products, the more am I convinced that, in the above formula, we have the expression of a law that includes most of their phenomena. It will, slightly modified, apply not only to large classes of tumours, but also to tubercle and to pus. Tubercle is a lymph-cell, of low vitality, incapable of development into healthy fibre, dying after a short existence, and generally becoming a foreign body. Pus may be described in words almost identical—their material difference being one, probably, more of chemical constitution than of vital power—for both are possessed of almost a minimum vitality. Tumours are composed of cells whose vital force is greater than pus or tubercle; and this vital power is rather spent in reproduction than in development, as in the healthy cell. The ordinary plastic cell goes through certain phases, dies, and is removed; its place is taken by a new cell, developed, probably, from the nucleus of its predecessor. The abnormal cell fails to arrive at perfection, often becomes a monstrosity, and has a tendency not only to reproduce itself from its nucleus, but to generate in neighbouring lymph organisms similar to itself. Thus the constituent cell of the tumour has a certain independent vitality, similar to that of the entire tumour; or rather the converse is true—the tumour is composed of cells of independent vitality, and hence it possesses the same form of life with the cell. And, as the life of the cell is of a low

type, so is that of the tumour. I cannot but think that many of the able minds which have been engaged in studying cancer, in its minute anatomy, have failed to make an adequate impression upon practical surgery, because they have been led away to look on cancer as a thing quite different from any of the ordinary structures of the body, instead of a perverted form of a natural structure. And I am sure that their views, carried out to their legitimate conclusion, would lead us to despair of any remedy for cancer but the knife, or other agents of destruction. Whereas, if we regard the cancer-cell merely as a perverted lymph-cell, we shall never rest until remedies are found which will influence it to a more healthy type.

I am sure we neglect too much the auxiliary treatment of good tonics, especially mineral tonics, good air and abundant food, with proportionate exercise; and we should have slower tumours, and fewer relapses, if we compelled the attention of our patients to these matters.

Whether chemistry, combined with the microscope, will ever lead us to the knowledge of how best to effect our purpose, is more than I can say. The study will, at all events, lay open many valuable truths—it will amply repay any one who can give time to pursue it. The actions of many medicines, may I not say most, are involved in obscurity; and we prescribe them on grounds which our utmost efforts cannot rescue from the taint of empiricism. Microscopic chemistry will solve some, at least, of these problems. For example:—The diuretic power of the bitartrate of potash is explained when we see the force with which it dissolves the epithelial cells of the kidney; its value in certain conditions of the liver is similarly explained. Ether is not only useful for its stimulant properties (the result of its rapid combustion, as chemistry has taught us), but also for its expectorant action, which is due to its solvent power over the bronchial epithelium: for this knowledge we are indebted to the microscope; and many other valuable results will reward those who apply themselves to it.

A word or two with regard to local treatment, after operation for the removal of tumours. A rapid cure is of the highest importance: it is obtained with less inflammatory action, and is so far less favourable to the plastic exudations in which secondary growths will spring up. I do not believe that suppuration is at all beneficial as a preventive of relapses, nor can I conceive on what imaginable grounds such a theory could rest. To obtain union as rapidly as possible, it is not enough to secure all bleeding vessels at the time; we must also prevent the possibility of blood lodging under the flaps; and we must do this without exerting such pressure on the latter as will either provoke reaction or interfere with healthy circulation in them.

The mode of dressing I adopt is as follows:—Let us suppose an amputation of the breast. I insert a wire suture at every inch; having twisted them, I cut the wires off at about an inch from the twisted portion; when this is completed, and the edges of the wound lie nicely in contact, I lay along each side of it, and parallel with it, two large compresses of lint filled with soft cotton, taking care to get their edges under the ends of the wire: these compresses are for the purpose of making elastic pressure all along the sides of the wound, thus preventing any accumulation of blood under the flaps; and they also interpose between the skin and the cut ends of the wire sutures, and prevent chafing; over the track of the wound, a wet compress can be laid, and the whole secured by a few turns of a roller, applied with moderate tightness. If there has been any dissection of glands from the pouch of the axilla, a sponge rolled in lint, or a handful of cotton, can be similarly used, to prevent accumulation of blood; and when the arm is brought to the side, sufficient pressure is made. A little ingenuity will adapt this mode of dressing to many operations. I have used it in amputations of limbs, castration, the removal of all tumours, and for the dressing of scalp wounds (omitting the stitches, or inserting as few as possible), and I have found it most useful. One advantage it possesses is, that we can easily inspect the line of wound, and sponge it clean without disturbing it; blood in the wound is the great cause of delay and danger after all operations—it prevents adhesion, causes suppuration, and tends to produce phlebitis and

purulent absorption, or angeioleucitis; and, as this mode of dressing prevents its accumulation, it so far diminishes the risk of operating.—*Dublin Quarterly Journal, Nov.* 1860, *p.* 369.

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## 147.—ON THE MODE OF ACTION OF ALCOHOL IN THE TREATMENT OF DISEASE.

By Dr. Edward Smith, L.L.B., F.R.S, Assistant-Physician to the Hospital for Consumption, Brompton.

[The following is a summary of the late Dr. Todd's views on the subject of the therapeutic action of alcohol. His views are based upon the chemical theory that alcohol is transformed within the system and produces heat.]

1. Alcohol acts primarily on the nervous system, and, like other hydro-carbons, but in a greater degree, has great affinity for the nervous system.

2. It acts in two degrees; one beneficially, when it augments the generation of nervous power; and the other injuriously, by deteriorating, impairing, or destroying the nutrition of nerve-matter. In the first degree, and when it acts beneficially, there is no smell of alcohol in the breath; but in the latter, or when the dose is too large, the odour is perceptible.

3. There is no true secondary depression of the vital powers, except when the quantity is too large, and then it acts by deranging the digestive functions.

4. Alcohol does not, in any dose, cause inflammation of the lungs, heart, or liver. The brain symptoms do not show congestion or inflammation, but a poisoning of the nerve-cell and nerve-fibre.

5. When carefully taken, it upholds the calorifying process, strengthens the action of the heart, and reduces the frequency of the pulse. By upholding the calorifacient process, it prevents oxydation of the nervous and other tissues.

6. It is simply absorbed into the circulatory system; but oil (another hydro-carbon) has a more complicated digestion, and therefore alcohol acts more quickly and certainly.

7. The dose should be from two drachms to two ounces, and repeated as frequently as food would be given, and with the same intention—viz., to save the tissues from oxydation. The dose and the frequency should be precisely adhered to.

8. The general use of alcohols show an instinct in man for them, and in recovery from disease they uphold nervous force, and supply the most assimilable material for combustion. They calm the nervous system, and avert delirium.

9. It is very dangerous to withdraw the alcohol, but the signs which would sanction this are referable to deranged digestion—as flatus, eructations, sickness, and dry tongue and mouth; and also the smell in the breath, which implies that the alcohol is passing off unchanged. Coma and delirium from excess of alcohol rather indicate that more alcohol should be given.

[Dr. Smith proceeds to remark:]

Such is a summary of Dr. Todd's views, and they chiefly include the belief that alcohol increases nerve-nutrition, strengthens the heart's action, lessens the rapidity of the circulation, shields the tissues from oxydation, and produces heat; and the sign which measures the efficiency of it is the non-existence of the odour in the breath. As to the skin, he merely remarks that when alcohol is given to a patient who has a hot skin and rapid pulse, it may look like an illustration of the dogma, "Similia similibus curantur." In reference to all his inferences from the supposed transformation of alcohol in the system, we may remark that they are of no value if it be shown that this transformation does not occur, but that alcohol, after remaining in the system for a time, and disturbing its action, is ejected still as alcohol. I believe that the latter is almost, if not quite certain.

I see no ground whatever for the statement that hydrocarbons have an especial affinity for the nervous system, taking starch and oils as the representatives of this class, and Dr. Todd does not adduce any. Neither has it been in any way proved that alcohols improve the nutrition of the nerve-cell and fibre in any separate and special way, but only by improving the whole nutrition of the body. Indeed, the phenomena of nutrition lead our attention away from mere local actions as independent conditions, and teach us that it is a general and not a local act. All the phenomena attending upon the action of alcohols, when taken in quantities which may render their effects noticeable, evince great nervous disturbance; and I cannot but believe that, whilst the nervous system is principally affected, it is by the opposite of a healthy and nutrient action.

The statement as to the action of the heart is doubtless correct, as is also that upon the frequency of the pulse; but the latter action is indirect, and probably due to the former, whereby a larger quantity of blood is forwarded at each contraction of the heart, and the blood is more perfectly distributed to the whole body. A rapid pulse is commonly a small one, and the impulsion of a large quantity of blood at a time lessens the necessity for the quicker circulation of it.

It is singular that so acute an observer should have made so little reference to the effect of alcohol upon the skin, its power to accumulate water in the body, and to retard the elimination of urea; but in reference to the former I may remark that acetate of ammonia was a most frequent adjunct to his alcoholic treatment, and this might do much to counteract the action of alcohol upon the skin. Moreover, whilst he did not withhold alcohol when the skin was hot and dry, his cases were frequently such as had perspiring skins, and in such instances he remarked upon the diminution in the perspiration during the action of alcohols. It may also be here observed that Dr. Todd most commonly added chloric ether, and not unfrequently quinine, to his plan of treatment, and hence there were several actions induced, some co-ordinate and others opposed.

His statement in reference to the absence of alcoholic exhalation from the lungs, in cases of disease in which the alcohol is well borne, is worthy of further investigation; but on inquiry I do not find that this fact has been recognised by others. If alcohol pass off untransformed, it is evident that this cannot be true; but if it be true, it is a most valuable guide.

Upon the whole, I do not think that the arguments used by Dr. Todd are now sufficient to establish his theory of the action of alcohols, or to warrant his peculiar plan of administering them; but, on the other hand, I think that the practice which he pursued must rest only upon the ground of his personal authority.

It now remains to state the conditions and stages of disease to which alcohols in various forms, seem to be well fitted,—the action in health being accepted as a sufficient guide,—and it will not be difficult to show that the usual practice of the profession has been based upon truth, however little or much the mode of action may have been understood.

I think that it must be accepted that the *essential and direct actions of alcohols, as remedial agents, are, the increase of the force of the heart's action, the local stimulation of the stomach, and the diminution in the action of the skin;* whilst the most dependent actions are, the retention of urea and fæces, by, or with, retention of fluid, diminution of certain secretions, and derangement of the assimilative process.

In conditions in which the force of the circulation is too feeble, there must be defective nutrition, innervation, and oxydization of the products of alimentation; for a certain fulness of the bloodvessels is clearly necessary to such brain phenomena as consciousness, as may be inferred from fainting; to excretion, as may be seen by the increase of urine after digestion of fluids; to general innervation, by the sense of *malaise* which accompanies feeble, and of *bienaise* which is found with proper, heart's action; and a certain frequency and duration of exposure of the blood in respiration, is necessary to the proper interchange of gases, whilst a due amount of action of the heart,

as well as of inspiratory effort, is required for the free circulation of the blood through the lungs. Hence, with weakening of the force of the heart, the muscular and nervous forces are weakened, and all vital processes are inadequate to health.

Such a state is that of general debility, deficient innervation, or exhaustion from numerous causes, temporary debility from over-exertion or anxiety, convalescence from almost all diseases, and the state of exhaustion in fevers, with certain limitations.

If, in such conditions, alcohol be administered in such a manner as not to induce derangement of the system, it is easy to see how Dr. Todd's statement may be verified—viz., that it improves the nutrition of the nerve-structures, and does not produce inflammation; and, in addition, it may be shown that, by its power to increase the force of the circulation, it will prevent or remove the tendency to local congestion of organs, as the lungs and liver, and the results of such congestion—viz., effusion of fluid,—and will improve the whole nutritive and vital powers of the body. This is certainly in accord with universal practice, both in this and former ages, for whilst we may appeal to the great mass of the profession in reference to their experience now, we have on record, in the works of Aretæus, an admirable description of the effects of wine in such conditions in his day, which we will transcribe. In the treatment of synope, he remarks: "But if converted into syncope, and this also happens, (the powers of life being loosened, the patient being melted in sweat, and all the humours being determined outwardly, the strength and pneuma being also dissolved,) we must disregard the delirium, and be upon our guard lest the patient be resolved into vapour and humidity. Then the only support is wine, to nourish quickly by its substance, and to penetrate everywhere, even to the extremities; to add tone to tone; to rouse the torpid pneuma, warm that which is cold, brace what is relaxed, restrain those portions which are flowing outwards,—wine being sweet to the sense o smell, so as to impart pleasure; powerful to confirm the strength for life, and most excellent to soothe the mind in delirium. Wine, when drunk, accomplishes all these good purposes, for they become composed by the soothing of their minds, are spontaneously nourished to strength, and are inspired with pleasure."

But we require at least one other indication as our guide in the administration of alcohol—viz., the condition of the skin. The regulation of the heat of the body, as well as the due distribution of the volume of the blood to the central and peripheral organs, rests very much with the skin. We know well the great benefit which results in the last stage of fevers, in local inflammation, and in sun-stroke, from the free use of cold water to the skin, although that could cool the body only by contact and radiation; and of the application of warmer fluids in similar conditions, which, so far as they were of lower temperature than the skin, directly cooled the body, and so far as they approached or exceeded that of the skin, might tend to restore the function of evaporation to the skin, and thus indirectly cool the body. Both these plans are of well-known efficacy, and although apparently opposed, produce, or tend to produce, by different methods, the one result. Now although there may be deficiency in the heart's action, if the skin be hot and dry, it is manifest that great risk may follow the use of alcohols; and when, as in the sthenic stage of fevers and inflammation, the force of the circulation is increased, from the resistance to the current of blood offered by the capillaries in the great inactive external organ of the body—the skin,—it must tend directly to do injury. Hence, it was never the practice to give these substances in the active and hot stage of fever; but when the condition arrived in which the skin had lost something of its dryness, and become soft, and particularly when it perspired, alcohols and bark became the sheet-anchor of the practitioner. With a perspiring skin, wine and other alcohols could be given profusely, and I once gave six bottles of port wine to a female in forty-eight hours, in such a condition occurring in fever. These are the states in cholera and in diphtheria in which very large quantities have been administered, with the effect of saving life from imminent peril.

Such, also, were the views of the ancients, for Aretæus again writes: "But if the period be already come to a crisis, if there be dew on the clavicle and forehead, the extremities cold, the pulse very small and frequent, or if creeping and feeble in tone, the patient must take a little food, and partake of wine effectually." And again: "But if much sweat flow, the pulse come to a stop, the voice become sharp, and the breast lose its heat, we are to give as much wine as the patient can drink. For those who are cold, wine is the only hope of life."

Hence, a feeble pulse and an active skin are, and have always been regarded as, the clear indications of the requirement of alcohol, and as the skin loses its excess of action, the rapidity of the pulse will decline, and its fulness and force increase. This is the most perfectly exemplified in the case of many Europeans, even ladies, living in India, who find it really impossible to sustain innervation, without the use of some forms of alcohol.

I have before referred to Dr. Todd's practice of giving acetate of ammonia, and may remark, that in cases of fever, when the exhaustion had just begun, it was always the rule amongst practitioners to give ammonia, and not an acid, with the cinchona, with or without wine; and no doubt the true action of this was, to temper the effect of the bark and the alcohol upon the skin, so as to prevent a too rapid reduction of the action of the latter organ. It is, however, singular, that patients with hot and dry skin should have borne the exhibition of twelve or twenty ounces, and even much more, of brandy daily for weeks together, whatever may be the truth as to the mortality, or prolonged duration of the disease, which followed its use. We see, in health, that there is a remarkable variation in the toleration of the system to the action of alcohol, and in certain conditions of disease, we know that the toleration is much greater. If Dr. Todd's statement is correct as to the non-emission of the fumes of alcohol by the breath in certain cases in which large quantities have been given, it is quite open to question if, in such cases, the alcohol obtained its usual entrance into the circulation. This seems to be the explanation of the similar condition in cholera, where the vital power of absorption is so greatly reduced, and it is quite certain that the body has naturally a power to limit the introduction of food and water into the blood, but allows it to pass off by the bowel. This is, probably, the safety-valve provided for such emergencies.

It has fallen to my lot to see cases of subacute inflammation of the lungs, and of the deposit which has succeeded to the inflammation, treated on Dr. Todd's method with alcohol, whilst the action of the skin was deficient, and I certainly have been impressed with the belief, that the progress of such cases towards recovery was greatly retarded.

The action of alcohol in restraining the secretion of the saliva and of the mucous membranes did not appear to me, in such acute or subacute cases to be of great importance; for whatever might be the state of the tongue, if the skin were freely acting, and other conditions called for the use of alcohol, the alcohol was well borne.

It is impossible to pass over in silence the influence of alcohol in lessening the excretion of urea; for if it do so simply by causing the retention of fluid in the body, it may be most prejudicial in cases of fever, where the whole safety of the patient seems to depend upon the free elimination of the excessive amount of urea which then occurs. It is only when the amount of urea has natural fallen with the subsidence of the febrile condition that alcohol can be safely administered.

But in any condition in which the amount of urea is lessened by reason of interference with the digestion and transformation of food, it is allowed by all that alcohol is injurious. In this condition it is very likely that the diminution in the flow of saliva may play a very important part, for it must greatly lessen the transformation of starch, and the retention of the fæces with other co-ordinate actions will explain the temporary condition of the liver and the headache, which are their constant attendants.

The efficacy of alcohol in delirium and coma has been quoted as a prime

argument in support of recent views. When the delirium is not the result of blood or brain disease, but indicates exhaustion, as in delirium tremens, syncope, or after fright, it is easy to explain that alcohol, opium, or bark, by giving force to the heart, and lessening the action of the skin and the kidneys, is very likely to cause that condition of fulness of the bloodvessels of the brain which is necessary to perfect consciousness. In certain forms of coma the beneficial action of alcohol may be explained by its power to quicken the circulation. In both of these states we shall doubtless find that any remedy which acts in the manner now indicated will be equally beneficial with alcohol. Whilst there is so much diversity and apparent opposition in the remedies employed by various practitioners, there can be no doubt tha they have a unity in the mode of their action which it is most desirable to discover.

The local action of alcohols upon the stomach does not call for remark, since the direction of their action is evident and well understood. The statement that alcohol must impede digestion, because when a substance is placed in it, it becomes hard, can have no reference to the action of the moderate and diluted doses of alcohols which are usually employed in medicine.

The frequency and regularity with which alcohol should be given, when it is necessary to prevent exhaustion, have been well stated by Dr. Todd. As the primary action is temporary and passes away in half an hour, the call for it is quickly renewed, and in cases of imminent danger an interval of half an hour would be long enough.

As to the dose, I venture to recommend the use of small doses—say half an ounce of alcohol where spirits would be given; for we have shown that the frequent repetition of a small dose conduces both to a full and a sustained action. It may be well to remember that, as spirits contain from 40 to 70 per cent. of alcohol, sherry and port wines from 17 to 24 per cent., and good ales from 6 to 10 per cent., two ounces of spirits are equal in alcohol to about six or eight ounces of wine and twelve or sixteen ounces of ale.

In conclusion, I would invite attention to the use of the aromas of wines and the saccharine and nitrogenous elements of ales. It is clear that these have an action apart, and sometimes different, from the alcohol, and therefore are fitted for different conditions. The conservative influence of the aromas of wines seems especially to be fitted for the cases of excessive nervous action, in which to reduce the action would conduce to health; whilst in fever it has been remarked that the newer wines containing little aroma and more spirit than old wines, have a better action.

It is well known that a quantity of alcohol may be taken in wine, and particularly in good wine, in health, without affecting the sensorium, which could not be borne at all if drunk as spirits and water—a fact due partly to the greater length of time over which the drinking of the wine is extended, and in other part to the opposing effect of the aromas. The value of the aromas has been recognized in all ages, and Aretæus directs that "the wine is to be fragrant and not very astringent."

As sugar largely increases the evolution of carbonic acid, and gluten has a similar but less and less rapid action, and in the latter case at least the carbon could not have proceeded from the gluten, I am led to believe that beers have the power of directly promoting the assimilation of carbonaceous food. Whether this is due to the quality of a ferment which nitrogenous matters possess is not here important to discuss; but the combination of alcohol with these principles and with a bitter property gives the most admirable compound which the world has seen, and especially fitted for the cases in which there are general enervation, defective powers of assimilation, and active state of the skin, as in the lassitude and exhaustion of many in hot climates.

Hence there are certain cases in which alcohol is chiefly called for, and in which spirits of wine may be given quite as usefully as the ordinary compounds; others in which the aromas of fine old wines are of great

value; and a third class in which the addition of the nitrogenous digestive agent in ales should be given.

I think it would be to the advantage of our patients if, in cases in which we desire the effect of alcohol, and not the other ingredients found in alcohols—viz., increase in the action of the heart and decrease in the action of the skin—we administered spirits of wine instead of spirits, because by obtaining it during the first hours of distillation we may obtain nearly a pure alcohol, and by indicating the specific gravity we may at all times know the true amount of the agent which we would employ. Alcohol varies very greatly in flavour and degrees of purity, and it commonly happens that the kind which is used in the manufacture of spirits is that which passes off in the later hours of distillation, and contains much fusil oil and free acid, which are very injurious to health. Moreover, as there is a clear difference in the action of alcohol and fine old brandy, we have in alcohol an agent which will effect the two purposes above-mentioned, and will at the same time support the respiratory process.—*Lancet, March* 2, 1861, *p.* 212.

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## 149.—ON THE PATHOLOGICAL HISTOLOGY OF TUBERCULOSIS.

By Professor Virchow.

[The following is an extract from "Virchow's Cellular Pathology," a valuable work lately translated by Dr. Frank Chance. The Reviewer of this work in the Dublin Quarterly Journal says:

"Virchow's views with respect to the microscopical and pathological nature of tubercle, are deserving of especial notice; for who has not had reason to mourn from the ravages of this inexplicable disease? and where the physician, so proud of his own attainments, who has not felt the blush of shame mantling his cheek, as the slow and insidious step of this deadly affection stole silently, but slowly, on, and he was powerless in checking its onward progress? Before this unsparing and relentless foe, that mocks its intended victim, while its remorseless gripe is gradually tightening on the well-springs of life, how lowly we fall in our own estimation, wringing from us the sincere, but bitter confession that, in this case, all our art and all our knowledge stand forth like pigmy dwarfs against such a gigantic and invincible antagonist. The fate of the French medical student, who, made aware of this incurable affection under which he was labouring, dared to anticipate by more speedy means, the event which he knew must be his lot, though by a process slower, indeed, but equally as sure, has branded, in characters that never can be effaced, our utter impotency to deal with this destructive malady successfully in the present condition of our knowledge. We ought, therefore, to hail as it deserves, every attempt that may be made to give us a proper insight into this hitherto unfathomable secret, as it may thus conduct us perhaps to a more rational mode of encountering an enemy who has hitherto proved so totally unassailable in every respect."]

It may, at least according to what I consider to be the correct view of the matter, certainly be said, that the greatest part of whatever, in the course of tuberculosis does not appear in the form of granules, is an inspissated inflammatory product, and has at any rate no direct relation to tubercle. But by the side of these inflammatory products, or also independently of them, we find a peculiar structure [the knot, granule] which, if they are to be regarded as real tubercle, would no longer be included in their ordinary classification; and it is certainly an extremely characteristic circumstance that in France, where the terminology of Lebert has become the prevailing one, and the *corpuscules tuberculeux* are wont to be regarded as the necessary accompaniments of tuberculosis—bodies, concerning the tuberculous nature of which there can be no doubt, have quite recently been set down as *something* altogether peculiar, and which had hitherto remained undescribed. For one of the best, nay, perhaps the best, micrographer France possesses,

Robin, has, in his examination of cases of tubercular meningitis, deemed it impossible to regard the little granules in the arachnoid (pia mater) which everybody looks upon as tubercles, as being really tubercles, because the dogma now prevails in France that the tubercle consists of solid non-cellular corpuscles, and, in the tubercles of the cerebral membrane, cells in a state of perfect preservation are met with. To such curious aberrations does this track lead, that one ends by being unable to find a name for real tubercle, because so many accidental objects have been confounded with it, that what was sought for, or even what had been found and was already grasped, has, in consequence of the attention of observers being diverted by these objects, been allowed to slip out of one's hand again. I am of opinion that a tubercle is a granule, or a knot, and that this knot constitutes a new formation, and indeed one which, from the time of its earliest development, is necessarily of a cellular nature, and, generally just like all other new formations, has its origin in connective tissue, and which, when it has reached a certain degree of development, constitutes a minute knot within this tissue, that, when it is at the surface, projects in the form of a little protuberance, and consists throughout its whole mass of small uni- or multi-nuclear cells. What especially characterises this formation is the circumstance, that it is extremely rich in nuclei, so that when it is examined as it lies imbedded in the tissue which invests it, at the first glance there seems to be scarcely anything else than nuclei. But upon isolating the constituents of the mass, either very small cells provided with one nucleus are obtained—and these are often so small that the membrane closely invests the nucleus—or larger cells with a manifold division of the nuclei, so that from twelve to twenty-four or thirty are contained in one cell, in which case, however, the nuclei are always small, and have a homogeneous and somewhat shining appearance.

This structure, which in its development is comparatively most nearly related to pus, inasmuch as it has the smallest nuclei and relatively the smallest cells, is distinguished from all the more highly organised form of cancer, cancroid and sarcoma, by the circumstance, that these contain large, voluminous, nay often gigantic corpuscles with highly developed nuclei and nucleoli. Tubercle, on the contrary, is always a pitiful production, a new formation from its very outset miserable. From its very commencement it is, like other new-formations, not unfrequently pervaded by vessels, but when it enlarges, its many little cells throng so closely together, that the vessels gradually become completely impervious, and only the larger ones, which merely traverse the tubercle, remain intact. Generally fatty degeneration sets in very early in the centre of the knot (granule), where the oldest cells lie, but usually does not become complete. Then every trace of fluid disappears, the corpuscles begin to shrivel, the centre becomes yellow and opaque, and a yellowish spot is seen in the middle of the grey translucent granule. This is the commencement of the *cheesy metamorphosis* which subsequently characterizes the tubercle. This change advances from cell to cell farther and farther outwards, and it not unfrequently happens that the whole granule is gradually involved in it.

Now, the reason why I think that the name of tubercle must be specially retained for this formation, as being extremely characteristic of it, is this—that the tubercle-granule never attains any considerable size, and that a tuber never arises out of it. Those which are wont to be termed large tubercles, and attain the size of a walnut, or a Borsdorf apple, as for example in the brain—those are not simple tubercles. You will generally find the tubercles in the brain described as being solitary, but they are not simple bodies; every such mass (tuber) which is as large as an apple, or even not larger than a walnut, contains many thousands of tubercles; it is quite a nest of them, which enlarges, not by the growth of the original focus (granule), but rather by the continual formation and adjunction of new foci (granules) at its circumference. If we examine one of these perfectly yellowish white, dry, cheesy tubera, we find immediately surrounding it a soft, vascular layer which marks it off from the adjoining cerebral substance—a closely investing

areola of connective tissue and vessels. In this layer lie the small, young granules, now in greater, now in less, number. They establish themselves externally [to the previously existing ones], and the large tuber grows by the continual apposition of new granules (tubercles), of which every one singly becomes cheesy; the whole mass, therefore, cannot in its entirety be regarded as a simple tubercle. The tubercles themselves remain really minute, or as we are wont to say, *miliary*. Even when on the pleura, by the side of quite small granules, large yellow plates, looking as if they were deposited upon the surface, are met with; these too are not simple tubercles, but masses composed of a large aggregate of originally separate granules.

Here you see, form and nature are in reality inseparably connected. The form is produced by the growth of the tubercle from single cells of connective tissue, by the degenerative proliferation of single groups of connective-tissue corpuscles. Thus, without more ado, it appears at once in the shape of a granule. As soon as it has once attained a certain size, as soon as the generations of new corpuscles which develop themselves out of the old histological elements by a continual succession of divisions, at last lie so close to one another, as to cause a mutual arrest of development, gradually to induce the disappearance of the vessels of the tubercle, and thereby to cut off their own supplies, then they begin to break up, they die away, and nothing remains behind but débris—shrunken, disintegrated, cheesy material.

The cheesy transformation is the regular termination of tubercle; but, on the one hand, it is not the necessary one, inasmuch as there are rare cases, in which tubercles, in consequence of their undergoing a complete fatty metamorphosis, become capable of reabsorption; and, on the other hand, the same cheesy metamorphosis befalls other kinds of cellular new-formations; for pus may become cheesy, and likewise cancer and sarcoma. This metamorphosis, therefore, being common to more than one formation, cannot well be set down as a criterion for the diagnosis of any particular structure, such as tubercle; on the contrary, there are certain stages in its retrograde metamorphosis, where one cannot help confessing that it is not always possible to come to a decision. If a lung be laid before you with cheesy masses scattered through it, and you are asked if that be tubercle or no, you will frequently be unable to say with certainty what the individual masses originally were. There are periods in the course of development when that which is inflammatory and that which is tuberculous can with precision be distinguished from one another; but, at last, there comes a time, when both products become confounded, and when, if one does not know how the whole arose, no opinion can any longer be formed as to what its nature is. In the midst of cancerous masses also cheesy spots occur which look exactly like tubercles. I have demonstrated that it is by the gradual transformation of the elements of cancer that this cheesy matter is produced. But if we did not positively know from the history of their development that cancer-cells disintegrate step by step, and that no tubercles form in the middle of cancer, we should in many cases be altogether unable to arrive at any decision from merely examining the specimen.

If these difficulties be surmounted which lie in the external appearance of the formation, and lead the observer astray not only when he considers its grosser features, but also when he investigates its more intimate composition, there remains nothing else to assist us in coming to a right conclusion than the investigation of the type of development displayed by the individual new-formations during the stages of their actual development, not during those of their retrograde metamorphosis. The nature of tubercle cannot be studied after the period when it becomes cheesy, for from that time its history is identical with the history of pus which is become cheesy; an earlier period must be chosen when it is really engaged in proliferation. So in the case of other formations, that period must be studied which is comprised between their origin and their culminating point, and we must see with what *normal* physiological types they agree. Then it is, I think, certainly possible for us to arrive at a just conclusion with the aid of the simple principles

of histological classification, which I have already propounded to you. Heterologous tissues also have physiological types.—*Dublin Quarterly Journal, Feb.* 1861, *p.* 201.

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## 150.—ON BRIGHT'S DISEASE OF THE KIDNEY.

By Dr. S. J. Goodfellow, Physician to and Lecturer on Medicine at the Middlesex Hospital.

[Very different opinions are entertained by pathologists as to the nature of the changes which lead to the formation of the small, hard, contracted form of kidney. Some regard it as a simple degeneration, others as the result of a low and simple chronic process of inflammation. Dr. Goodfellow believes there is a combination of the two in most cases.]

In this form of kidney there is a great increase of the connective tissue, and not only an increase, but an alteration of the tissue. It is coarser, and is sometimes, I believe, more or less mixed with fibres of the yellow fibrous element. The normal connective tissue, or matrix of the kidney, is extremely delicate, and this, I suppose, has led some histologists to deny its existence altogether. It is very easily destroyed, and it is so extremely transparent that unless considerable care is taken in the management of the light in looking at specimens of healthy kidney tissue as transparent objects, and, therefore, by transmitted light, all appearance of fibres will be lost.

The atrophy and destruction, partial or complete, of the Malpighian bodies and convoluted tubes, which produce the contracted state of the organ, is, in a great measure, owing to this increase and alteration of the fibrous elements. Not altogether, however; for it is probable that, for the reasons assigned in my last lecture, there is, at the same time, and from the same cause, a degeneration and an actual wasting of these structures, independently of the mechanical effects of this increase of fibrous tissue. It is more than probable, as I there hinted, that the delicate anatomical elements composing the walls of the capillary blood-vessels, both Malpighian and secondary, and of the uriniferous tubes, are directly injured by the irritating influence of the alcohol, and other allied substances. This is not only the case with the anatomical elements of the kidney, but probably with those of the liver and other organs also, in which this condition is usually observed. This being probably the case, the question naturally arises, To what is this increase of the fibrous element due? Is it simple hypertrophy of the matrix from increase of normal nutrition, in consequence of the constant state of congestion and blood-delay, due to the influence of the several causes which I have named as instrumental in the production of this form of kidney? Nutritive hypertrophy it may be called. Or is it from fibrillation and organisation of an exudate, the result of a low and very chronic inflammatory process? Or, lastly, is it an intermediate process, partaking of the characters of both—development by cell-formation and fibrillation? The blastema for this growth of fibrous tissue we have seldom, if ever, an opportunity of seeing in the kidney, for, as I have said before, it is only after the tissue has been already formed, and the kidney has been considerably atrophied, and its secreting structures nearly gone, that death enables us to see the real condition of the organ. But, although we do not see it in the parenchyma of organs, we do on serous surfaces; and, supposing that it does not exactly resemble that, in consequence of the different size and arrangements of the capillary blood-vessels, and the greater intensity generally of the inflammation, and other circumstances, yet it may so nearly resemble it as to enable us to form a notion of the process by which a plastic fibrous exudate becomes converted into fibrous tissue. In the kidney it may not always result from a slow but continuous inflammatory process. It may be from frequent exacerbations, or from separate and frequent subacute attacks of inflammation, owing to the more intensified action of the different causes at different times, when the exuded fibrinous blastema may undergo those changes which we observe in similar exudates on serous surfaces. It may, at times, be partially absorbed, previously undergoing liquefaction by the more fluid portions of the

serum, or it may be developed into an organised fibrous structure, either by cell development and growth, or, which is more frequently the case, by conversion into fibrous tissue directly by the dissilience of the fibrillated blastema.

This increase having taken place. How is it likely to affect the true secreting structures? It produces it in two ways, both mechanical in their operation,—First, by pressure upon them, leading to absorption, and secondly, by pressure upon, and obliteration, partial or complete, of the blood-vessels, and in this way cutting off the supply of nutritive blood-plasma. This interception of blood-plasma may probably be induced in another way,—by the peculiar mode of action of the causes of this form of kidney disease, either from actual coagulation of the albumen and fibrine in the blood-vessels, or from the mechanical influence of the separated or precipitated fats in a non-saponifiable form.

But, as I said before, I do not think that the atrophy and destruction of the true gland structures are entirely due to these mechanical influences, certainly not, when the cause is alcohol. I repeat that the mere contact for any great length of time of an irritant with the delicate gland-tissues will lead, under certain conditions, to their degeneration and ultimate destruction. The mere contact, more or less continuous, of an irritant, such as alcohol, in the form of brandy, gin, and such ardent spirits, will produce something resembling a shock upon the tissues, alter or annihilate their inherent properties, impair or destroy the play of their normal chemical affinities, and consequently deprive them of their assimilating powers. They die, and cease to be renewed, from inanition, much in the same way as they lose the same properties of development, growth, and conversation, from general impairment of all the functions of the body in old age.

*On the Fatty Kidney, when occurring as an Independent Form.*—With regard to the process by which this independent form of Bright's disease is produced very little is positively known. It seems to me that the only difference between this independent form, and that which accompanies, and constitutes a part of, the other forms, consists in the fact, that as in these two forms—the large white, and the small and contracted—it is a conversion or metamorphosis of abnormal blastemata and subsequent tissue-formation into fat, so this is a conversion or metamorphosis of a blastema, degraded not by inflammation, or any palpable form of disease, but by some other condition which interferes with the nutrition of the blood, or with the proper development and growth, and healthy changes of the various proximate principles. Instead of the materials derived from digestion being converted into perfect assimilable protein principles, that process of conversion seems to be arrested, and they remain in the form of fatty matters, which, being the only, or at least, the most abundant produce of the digestive process, ultimately replaces the normal protein-tissue. But, besides this, there is strong evidence that, under certain conditions, protein-tissues may, by some process, even in the living body, be converted into fat. There is, probably, another source from whence fatty matters may be obtained. The effete matters, resulting from the waste of the tissues, may not be converted into such oxidisable states as are capable of being eliminated in the form of the perfect excreta of the body, and they may be metamorphosed into fat. The quantity of food daily taken, far too great for the wants of the system, will take away a great part of the oxygen, derived from respiration, that ought to, and would otherwise, have been exclusively appropriated to the oxidation of these effete matters; and if the supply of oxygen is still further diminished by insufficient exercise, or by confinement to an impure atmosphere, the tendency to this fatty production in the body will be proportionably increased. We should, consequently, expect to find this superabundance of fatty matters in the body, and the tendency to the replacement of protein principles by fat in elderly people, in whom the respiratory changes are deficient, who take but little exercise, and take more food than they can possibly convert and assimilate into the higher proximate principles and protein tissues. In drunkards this conversion probably is more nearly allied to a form of inflam-

mation, and owes a complex causation. As I have already stated, alcoholic drinks are a much more frequent cause of this fatty kidney. It is impossible that the separation of fatty principles, which was observed by MM. Lallemand, Perrin and Duroy, after the administration of alcoholic compounds, can take place in the blood without obstructing the circulation through the minute capillary blood vessels, interfering with the transudation of blood-plasma through the interstices of their walls, and leading to a superabundant admixture of these fatty principles with the plasma itself, even if it were otherwise normal in composition, which it very probably is not. How can it, indeed, remain in the normal state? This separated fat most probably comes chiefly, if not entirely, from the red blood-corpuscles, if it be true, as Lehmann asserts, that the fats of the blood are principally deposited in them. It is not surprising, then, that tissues, whether composed of fibres, molecules, or cells, or of all of them, should undergo changes, which ultimately end in fat. Old age, too much food, too little exercise, residence in an impure atmosphere, but above all, an intemperate use of ardent spirits, one, or more, or all, are the true causes of the fatty form of kidney when occurring as an independent form of Bright's disease, and also, in great part, when it is an engraftment upon the others.

*On the Amyloid, Lardaceous, or Waxy Kidney, when occurring as an Independent Form.*—Virchow was the first to discover the true nature of the metamorphosis that the tissues undergo in this condition of the organ. Those parts of the kidney, and of other organs which have undergone this metamorphosis seem to be converted into a substance analogous to its reactions with iodine and sulphuric acid to substances of the amylaceous group. On brushing over parts affected with this metamorphosis, they assume in a few minutes a deep red-brown colour. This seems to be distinctive, for it is very different from the colour produced by iodine on organs in any other condition; and when once seen, is represented by Dr. Harris not to be mistaken. It is not cellulose, for iodine does not produce the red-brown colour with this substance, and it offers less resistance to alkalies, and is convertible into sugar, which the amyloid substance is not. For the same reason it is not actual starch. The reactions with cholesterine with the same agents, for which it might otherwise have been mistaken, are essentially different. It is therefore not allied, probably, in any way to this substance. The process by which the presence of this amyloid substance may be detected, is very simple, and of easy application. "When a solution of iodine is brushed over a liver which has undergone this change, the affected parts in a few minutes assume a deep red-brown colour, very different, as before stated, from the colour produced by iodine on organs in any other condition. When to these parts, thus reddened by iodine, sulphuric acid is added, a change to a bluish-red or violet-red, or deep blue purple, or even to an indigo-black, speedily commences; in some cases this colour quickly passes into a deep reddish-brown. In the malpighian bodies, and arteries of the kidney, the bluish coloration is most marked, and in these the dilute acid is sufficient to produce it. In the liver the stronger acid is necessary, and the colour is observed with greater difficulty. Now, cholesterine, when treated with strong sulphuric acid and iodine, shows a very similar blue colour; but with iodine alone it is unchanged in colour. It is necessary, in order that the iodine shall produce its characteristic blue colour, that it undergo some amount of oxidation by the sulphuric acid." The following are Virchow's views as to the nature of the substance, its anatomical seat, and the character of the constitutional symptoms as quoted by Dr. Harris, from his work on "Cellular Pathology:"—

"Almost all parts of the body are capable of undergoing this process of degeneration. The affected parts become enlarged, somewhat indurated and anæmic; the cut surface is semi-transparent, but dull; the natural colour of the parts is lost, but the colour of the neighboring parts and vessels being seen through, gives them a yellowish or brownish tinge. The coats of the small arteries are the most frequent primary seat of this infiltration, and from them it spreads to the parenchyma of the organs; the walls of the

arteries become thickened, and their calibre reduced, and hence the anæmic conditions of the organs. The muscular fibres of the middle coat are the parts first affected. In the place of each muscular cell a compact homogeneous body is seen, in which, in the earlier stages, the centre of the nucleus appears as a hole; this afterwards disappears, so that a kind of spindle-shaped body remains, from which all trace of cell-structure has vanished, no distinction being left between cell-wall, contents, and nucleus. When the infiltration has reached this point, it commences to invade the parenchyma of the organs. In the liver, the cells in the immediate neighbourhood of the hepatic arteries are first affected; the liver-cells gradually become homogeneous; nucleus and cell-well gradually disappear; and at last nothing is left but an absolutely homogeneous shining body; the cells are thus converted into a kind of corpora amylacea. In the kidney, the vessels of the Malpighian bodies and the afferent arteries first undergo this change. In the earlier stages but little alteration is perceptible to the naked eye; the kidney appears merely indurated, and anæmic, and only when a solution of iodine has been applied to it, does the change it has undergone become apparent; then, throughout the cortex numerous fine red dots appear, corresponding in their size and position to the glomeruli, and fine red streaks running from them, indicate the afferent arteries.

"The disease is constitutional; one organ alone is rarely affected; the only spot, where as yet an independent development of this change has been remarked, was in the permanent cartilages. The organs thus affected cease to discharge their functions; the patients assume a cachectic appearance, and gradually waste away; dropsy frequently supervenes. Sometimes, too, the whole digestive tract is affected by this degeneration. During life this is rendered manifest by continued diarrhœa, and by diminished powers of absorption."—*Med. Times and Gazette, Dec.* 15, 1860, *p.* 573.

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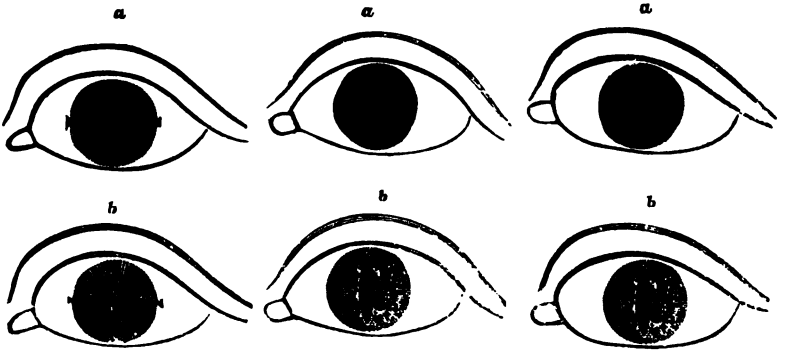
## 151.—MR. BOWMAN ON CONICAL CORNEA.

[Having been favored by the Editors of the Ophthalmic Hospital Reports with the wood-cuts illustrating Mr. Bowman's paper at page 258, but which were received after that article was printed off, we here append them. Mr. Bowman says:]

It is interesting to observe the movements of the pupils after these operations. The following diagrams exhibit the states of dilatation and contraction in the case of horizontal slit, of vertical slit, and of the balloon-shaped pupil from iriddesis downwards; *a*, dilatation; *b*, contraction.—*Ophthalmic Hospital Reports, Oct.* 1859, *p.* 166.



The second volume of the "Transactions of the Obstetrical Society" contains numerous valuable papers, some of which we here bring before the practitioners of midwifery. We received it too late to enable us to get the papers ready for insertion in their proper place.

## 152.—ON A SPECIAL POSITION IN LABOUR.

By Robert Hardy, Esq., Lecturer on Obstetrics to the Hull and East Riding School of Medicine.

[We have often found the following practice of Mr. Hardy's exceedingly useful. It was long ago suggested by Mr. Craven, of Hull, that in a case of lingering or impeded labour, the woman should alter her position, and *sit upright between two chairs* till the head was nearly born, the patient being then removed to bed. Mr. Hardy publishes a most interesting paper confirming this practice of Mr. Craven's, and illustrates it by several engravings in the Obstetrical Transactions, showing the various axes of the pelvic canal. Mr. Hardy explains his views as follows:]

The placing a parturient female in a sitting posture may be effected in two ways—1, by seating her between two chairs, suitably arranged; or 2, by adopting a simple and inexpensive apparatus made for the purpose. In private practice the chairs are most convenient, and answer admirably; whilst in obstetric institutions perforated seats on firm supports, and with side arms, are to be preferred.

The position on the chairs may be explained thus: Two firm chairs, of equal height, are secured to each other *in front*, by tape or other strong material; their *backs* are then separated from each other one and a half or two feet, according to the breadth of the patient's nates. The chairs, to secure them from moisture, as also to save the patient's thighs from painful pressure, are covered by folded napkins; and it is further desirable that they be placed on the *left* side of the bed, *facing the footpost*, in order that the patient, at the proper time, may be quickly removed on to the bed.

The woman's assent to the measure having been obtained, and the obstetric binder carefully secured round the whole of her abdomen, she is removed from the bed, and *seats herself well over or between the chairs*. When doing this the assurance is given her that no exposure of her person, while thus situated, can possibly occur. Standing behind the chairs, the accoucheur is to take care that the patient's *dress* is *free* and well drawn down; that her *nates* are projected a little *below* the plane of the chairs; that her *knees* are somewhat separated and pressed firmly against the framework of the bed, and her *feet* resting on a firm support.

Thus circumstanced, the woman is to bend forward and grasp firmly the footpost of the bed; should there be none, then the waist of an attendant, seated on the *side* of the bed, must be substituted for the same. The accoucheur, situated *behind his patient*, seats himself on a low chair or stool, or, if he prefer it, may kneel on a cushion placed on the floor. Thus circumstanced, his occasional examinations of the woman enable him accurately to note the progress of the labour, and at the proper time to have her removed to bed. For the sake of cleanliness, it is proper also to place an ewer beneath the nates, to receive the liquor amnii and contents of the rectum.

When the *perforated seat, with arms*, is the medium adopted, there is less necessity for the bedpost or friend in front, as the arms of the chair are giving firm support to those of the woman during the expulsive efforts.

Having premised thus much relative to the position of the patient, I must now remark that as it is *not* the design, so neither is it necessary or advisable to effect the delivery whilst she is seated *on* the chairs. I would lay great stress on this point, for *two* reasons. The first is, that, thus circumstanced, the perineum cannot be so efficiently guarded from lesion as in bed; and secondly, the infant, when born, cannot be so conveniently received by the attendant in this position of the mother.

The rule, therefore, is, when the difficulty in the labour has been overcome, which is known by the ossa parietalia beginning to advance from be-

hind the perineum, the woman is to be removed forthwith to the bed, and placed in the ordinary parturient position. Some slight cessation from pain sometimes follows this removal, but, as a general rule, the head passes in a few pains, and the delivery is, ere long, effected with perfect safety to both mother and infant.

In *all* cases it is to be understood, before the patient is placed on the chairs, that the presenting part (head or other organ) *is clearly within the pelvic inlet, and the os uteri dilated fully one half*: and further, that in every case a reasonable period has been allowed to the horizontal position, and its *failure* evidenced *ere* the measure is had recourse to.

*The contra-indications to the sedentary posture in labour* are one or more of the following:—1. *Systematic exhaustion*, or symptoms denoting its near proximity. 2. The existence of *inflammation* in any of the internal viscera, or parts more immediately affected by the labour. 3. *Serious uterine hemorrhage.* 4. *The previous advent of puerperal convulsions.* 5. *Prolapsus of a pulsating funis.* 6. *The presentation of parts at the os uteri requiring version.* And 7. *The presence of extreme pelvic obliquity*, wherever situated.

*The modus operandi of the sedentary posture and binder in labour*, in so greatly expediting that process, is not inaptly illustrated by *the act of defecation, as performed under circumstances of difficulty and distress*, as, for instance, in cases of *obstinate constipation or dysentery.* In these instances the greatest amount of expulsive muscular effort, voluntary and involuntary, is put forth, of which the system is capable, in order to contract the abdominal space and force downwards the irritating alvine contents, first *into*, and then *through* the pelvic cavity.

Seated on the chairs in the manner described, the same series of voluntary and involuntary muscular actions is induced and maintained as in difficult defecation, in addition to and in union with the powerful periodic and expulsive efforts of the *uterus.* The head and body are brought forward to an angle varying from thirty to forty degrees from the perpendicular, by which action the axes of the pelvic inlet and abdominal space are brought into perfect accord. *The hands, feet, and pelvis are becoming so many fixed points*, which are aiding the system in exerting itself to the very utmost in its expellent efforts. The result of all this is seen in the following important advantages, which are secured to the parturient female, viz.—1. Every available succour arising from *the force of gravitation* in the uterine ovum. 2. The putting forth, under highly favorable circumstances, of the most energetic *muscular efforts* of which the system is capable. 3. The bringing, as before named, the pelvic and abdominal axes into the same plane or line of agreement. And lastly, the great support and energy imparted to the *fundus uteri and abdominal muscles* by the *obstetric binder*, suitably applied.

Of the obstetric binder, not only when used in connection with the sitting posture, but as an independent aid in the treatment of *lingering and impeded labour*, I have formed a much higher estimate than is accorded to it by the profession generally. I am accustomed to make frequent use of it before the birth of the infant, where the chairs are *not* called into requisition. I find it greatly excites the laggard and strengthens the enfeebled uterine energies, thereby expediting the progress of the labour to a very useful extent. The good effects of this agent in cases which are simply *lingering* are most admirable. A binder *which embraces the whole abdominal parietes within its grasp*, pinned on firmly and smoothly, imparts wonderful support and energy to the *fundus uteri*, where it is most needed; and the labour, which before its application had remained in nearly *statu quo* for several hours, will frequently be terminated in half an hour, or at most an hour, after its application. In such cases it is important that the uterus should be kept well closed by the hand (through the abdominal walls) for a short period after the uterus has expelled its contents; then a couple of folded napkins should be placed over it, and the binder tightened and pinned securely.

*In impeded labours*, where the resisting powers to the delivery are not very serious, the application of the binder to the abdomen will frequently suffice for the delivery of the woman. In these cases, the laying hold of the

binder by the practitioner's left hand, and firm traction made during each expulsive effort, will most materially aid the delivery; the same precautions being taken after the birth to secure contraction of the uterus, the expulsion of the placenta, and the prevention of post-partum hemorrhage.—*Obstetrical Transactions, Vol. 2, p. 83.*

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## 153.—ON THE TREATMENT OF MENORRHAGIA.

By Dr. C. H. F. Routh, Physician to the Samaritan Free Hospital.

[Dr. Routh has published an interesting paper on this subject, but we cannot say that we approve of his treatment. It is necessary, however, that our professional brethren should judge for themselves, and we therefore give them the views of this physician, as his respectable position must have weight in our estimation of his practice. In one case of menorrhagia, the hemorrhage was stayed by the sponge-tent, but it returned. The uterine mucous membrane was then scraped off with Dr. Simpson's instrument, and tincture of iodine injected. Dr. Routh says:]

This patient I have seen since, and she continues well. The case is one of those obstinate cases of menorrhagia where no polypi exist, but where the blood is apparently secreted from a villous membrane. Remedies, in this case, at most did only temporary good, and the constant recurrence of the flooding was such as to justify alarm in relation to the ultimate issue of the case, if not speedily and permanently arrested.

[In another case of flooding, the womb being dilated and pulled down, a finger was introduced into the cavity and a small polypus removed by a curved gouge, and then tincture of iodine injected without scraping off the mucous membrane. Dr. Routh then proceeds to remark:]

The treatment by injection or by the uterine gouge in menorrhagia, singly, have been in vogue for several years, but neither have received that support from the British profession to which I think they are entitled.

This has arisen, I presume, because the modification of these plans which I now recommend, and which forms the original portion of this communication, was not observed. Again, I do not claim to be even the inventor or author of this modification. The injection of the uterus by astringent liquids, or the scraping of the uterine mucous membrane by the gouge, were, as separate operations, first introduced by my colleague, Dr. Savage, in the Samaritan Free Hospital. His uniform success led me to adopt it also, although I used stronger astringent solutions, and occasionally injected immediately after using the gouge. It is generally believed that to inject within the uterus any astringent injection is calculated to bring about peritonitis, and several cases of death from this cause, as a result of this treatment, have been quoted. It is unnecessary here to cite the authorities for this opinion. But as it expresses the opinion of modern accoucheurs, that of Dr. Simpson may be here given, who, in his late admirable lectures, published in the "Medical Times and Gazette," writes as follows:—"But, markyou, never think or dream of throwing liquids into the interior of the uterus by means of any injecting apparatus, for severe and fatal influences are very likely to ensue." And again: "The consequences of injecting fluids into the cavity of the womb are so often dangerous and deadly, that the practice has now been given up, I believe, by all accoucheurs." I feel very diffident in venturing to controvert so high an opinion. I admit these results, but think they depend not so much on the injection *per se* as upon the *modus operandi*. I think the common supposition, that in these cases, some of the fluid penetrates up through the Fallopian tubes into the peritoneal cavity, is not proved, except a force capable of rupturing the organ itself were used, because, precisely as in the case of the glottis, a contraction of the muscular structure would take place around the tube, and thus no entry of the fluid can occur

Indeed, Dr. Simpson himself seems to doubt this transmission upwards. When speaking of the fatal results due to injections, he adds—"Such a result *may*, perhaps, be caused by the fluid running through one or other

patent Fallopian tube, and escaping into the peritoneum; *more probably*, it may be due to laceration of the mucous membrane, and entrance of the fluid into one or more of the uterine veins." I had an opportunity, however, when in Vienna, of seeing this fact proved by an experiment. A woman, the moment after her death, was brought down into the dead house, the abdomen opened, and the uterus exposed before the irritability of the muscles had been destroyed. Dr. Ciari (the late professor of the Secret Obstetric Department, and one of the physicians to the division for diseases of women in the Allgemeine Krankenhaus) having placed the end of a powerful syringe within the uterus, and secured it there, attempted by main force to force up a quantity of fluid through the Fallopian tubes, and utterly failed; and yet, so far as one could judge from her antecedents, there was no reason to suppose that these were impervious. If the ordinary precautions are employed which are commonly taken when injections are given, no fluid could thus penetrate into the peritoneal cavity. Peritonitis, if it occur, would rather be due to *sympathy* of neighbouring parts or rupture of the uterine surface extending within the veins. But if *sympathy* was the common cause of peritonitis, why should not solids, liquified, as they must be, by the uterine discharges, not give rise to it also, whether when applied to a uterine mucous membrane or an ulcer of cervix? The absorption by a ruptured vessel appears thus more probable. We have an example of this kind of extension in those cases of metritis which occur after labour, and where the inflammation extends to the peritoneum, not through the Fallopian tube on its fimbriated extremity, but in the neighbourhood of the broad ligaments. Again, to set up this inflammation by sympathy, within the uterus, the injection must be more or less retained for a time, or very acrid in kind.

These facts being premised, it follows that, if any injected fluid *is not long retained within* the cavity of the uterus, no bad effect will follow. Experience, however, proves that if, through any untoward accident, it is so retained, then it will give rise to intense pain, and, as a result, inflammation may be expected to follow.

[It is very likely that Dr. Routh's opinion about the danger of iodine injections is true,—viz., that there is no particular danger if you first fairly dilate the os uteri so as to allow free egress to the solution, but that when you inject tincture of iodine through a narrow cervix uteri, it is retained in utero, and gives rise to mischief. In all kinds of injections, therefore, always see first that the os is properly dilated.]

It would appear that in these obstinate cases of menorrhagia there are two conditions of the uterine mucous membrane which require to be removed; one is the presence of polypi of various sizes and number, which grow from its surface; the other is a softened state of the mucous membrane, probably *inflammatory* in character, and which bleeds upon the slightest touch, in many cases upon the slightest movement of the patient.

I was lately enabled to notice this state of mucuous membrane in an old woman in whom menorrhagia was present, and in which there was complete prolapse of the uterus. Dilatation of the os enabled me very readily to see the mucous surface of the uterine cavity, which was covered with villi, varying in size, to the eye, about one-eighth to one-sixth of an inch long. On being touched, these bled profusely. The microscopical characters were those of erectile tissues surrounded by epithelium. It is easy to understand how, with such a structure, hypertrophy of one villus would produce a polypus.

The plan I adopt in these cases is to dilate the uterus by a sponge-tent, after the manner so beautifully described by Dr. Simpson in the Edinburgh Monthly Journal, (vol. for 1850, p. 7). This is consecutively done for three or four days, until at last the cavity is so large as readily to admit the index finger. I then draw down the uterus by means of a peculiarly constructed, corkscrew-shaped hook, which has, I think, a decided advantage over those in ordinary use, neither compressing the cervix, as in the case of tenacula, nor tearing the os, as is so commonly done with ordinary single or double hooks, a result very likely to take place when, by the very dilatation pre-

viously practised, the os is thin and easily torn through. Pulling down the uterus by this instrument, and pushing the index finger up into the uterine cavity, I am enabled to feel any irregularities or small polypi on the lining membrane. But in this way polypi may be felt quite at the superior part of the fundus uteri, which, owing to the length of vagina and uterus and the shortness of the finger, might have been overlooked. The finger being now removed, the scraper or gouge is introduced, and the whole mucous membrane removed. It is remarkable how, where the amount of hemorrhage has been so excessive that even the mere touch of the sound has almost justified alarm at the consequences, yet the moment the uterus has been denuded of its mucous membrane all hemorrhage ceases. This done, about half an ounce of tr. iod. co. or tr. ferr. sesq. is conveyed by means of an elastic-gum catheter to the superior portion of the uterine cavity, and allowed to flow down through a speculum in the vagina. The case is now left to itself; if pain occurs, an opium suppository is applied within the rectum. One application of the injection often suffices, but more lately I have repeated in very bad cases the injection, *i.e.*, from three to five or even six days, so long as the os continues patent enough to admit a goose-quill high up. After the second or third day, a reddish mucous discharge begins to flow, where tr. ferr. sesq. has been used, accompanied with large, thin, black flakes, which look very like scales of charcoal; this is, however, soon replaced by an ordinary leucorrhœa, and the cure is generally complete. The operation may sometimes fail in those cases where the hemorrhage is due to a softened, probably inflamed, villous membrane, if the whole diseased part of the mucous membrane, or perchance some polypus growing upon it, may not have been scraped away. The disease may, therefore, at no distant period, reappear. Now, I believe that in many of these cases the injection, if applied at once, corrects the error of the operator, by constringing and then setting up a healthy and absorptive action in the lining membrane of the uterus, and particularly in the diseased parts accidentally left behind. Indeed, this absorptive action, it might be said, in some measure was set up by the very sponge-tent itself. I agree upon this point with my colleague, Dr. Savage, that the mere and occasional dilatation by a sponge-tent suffices to cause the absorption and diminution of volume in the uterus, besides increasing the tendency of the uterus, after a time, to contract and expel its contents from the very irritation set up, so far favouring the egress of any fluid which may have been retained or even injected.

It has been asserted the scraping of the uterine mucous membrane may cause sterility. This is not even probable. On the other hand, sterility must certainly exist in cases of menorrhagia. Mucous membranes are readily reproduced, particularly that of the uterus, which is normally thrown off entire as decidua after labour, and again reproduced. Moreover, the catamenia are regularly established after the operation of scraping. This is *prima facie* evidence of possible conception.

The last objection made is that such cases may get well under other general treatment. Granted they do occasionally, but then it is only after many weeks' suffering, perhaps months, where local measures would have effected a cure in a week or ten days. It is equally certain some of these cases will not get well by any other means.

In conclusion, I do not mean to say that the method of cure now recommended will be successful on all occasions, and it may have to be repeated. Nor do I mean to say that in instances where polypi are larger than a small nut, and confined to the cervix, that it is to be practised. Here excision, by Dr. Locock's instrument or the knife, is the better plan, and in these cases it will be found that the corkscrew-hook before mentioned will often suffice to bring it down for excision with ease, or for destruction by potassa fusa. The last effectively prevents a recurrence. The only danger of this last plan is the fear of destroying too deeply, perhaps causing in the sequel obliteration of the os, a result I have seen follow the too free application of lunar caustic, although I am free to admit that, with so skilful an operator as Dr. Simpson, such mishaps could not occur. But we are not all Sir C.

Lococks or Dr. Simpsons, and the plan I propose is, I think, less dangerous. In cases where numerous and small polypi are found in the cavity of the uterus and at the fundus, I think the plan now proposed is to be preferred.

The uterus need not be scraped on every occasion. Injections, if preceded by a sponge-tent and repeated frequently, may obviate the necessity of the gouge.

[We have given Dr. Routh's opinion fairly. He is supported in this practice by many of the best practitioners, but nevertheless. we should decidedly object to all this scraping of the internal surface of the womb, because we think that it is unnecessary, as there are few, if any, cases of this description which will not yield to the ordinary methods of treatment, assisted by removal of the polypi, if these exist.]—*Obstetrical Transactions, Vol. 2. p. 117.*

## 154.—ON PELVIC ABSCESS AFTER LABOUR.

By Dr. George D. Gibb.

[We point out this short paper of Dr. Gibb's, because we suspect that such cases occur in private practice oftener than is suspected. A woman had been confined seven weeks, when a pain commenced in the right iliac groin, which became incessant, and was followed by a swelling above the pubes, accompanied by pain on pressure. Now in such a case, after confinement, always examine carefully all round the vagina for a fluctuating tumour, which will indicate where one of these pelvic abscesses following pelvic cellulitis will point, and where relief may be obtained. Dr. Gibb says:]

7th. On careful examination I detected a prominent fluctuating tumour, but hard and painful, occupying the lower part of the posterior wall of the vagina. Poultices were ordered to this, and an alterative and tonic mixture presented three times a day, each dose containing two grains of iodide of potassium with a little tincture of opium; to have, besides, ten grains of rhubarb twice a day.

11th. Bowels relieved two or three times. The abscess in the vagina had burst, but another had formed around the urethra, which is much more swollen, hard, and inflamed. Numerous small ulcers are present on each side of the labia, from the meatus downwards; pulse 92, a little stronger. Although these ulcers are not specific, I prescribed lotio nigra and small doses of mercurials and Dover's powders thrice a day.

It is well known that pelvic cellulitis is a serious affection occurring after delivery, and when followed by suppuration some anxiety is felt as to the course the matter may take. If the abscess should burst into one of the outlets of the body, like the uterus and vagina, or the bladder and rectum, there is always a fair prospect of recovery. Fortunately in the present instance, nearly eleven weeks after the first symptoms declared themselves, the abscess pointed at the lower and back part of the vagina, and spontaneously evacuated itself, with complete contraction of its walls. Some matter also found its way to the anterior wall around the urethra, which no doubt was the cause of much of the difficulty of micturation. A question arose in my mind, whether both of these abscesses originated from the same source, that is, the primary suppuration higher up in the right side of the pelvis, and it seemed to me that it must be so, from the fact of their forming pretty much about the same time. The quantity of matter discharged did not exceed several ounces, and to this I attribute the successful result of the case. The general health was well-supported throughout by liberal diet and wine, together with tonic and anodyne remedies. There was a curious feature in the case, which I have not mentioned, and that is, the patient could take nothing warm; all her food had to be swallowed cold.

The catamenia appeared seventeen weeks after confinement, and subsequently resumed their healthy condition. And what is of some importance in the history of this case, pregnancy again took place, and a second child was born nearly seventeen months after the birth of the first, without any inconvenience or bad symptom whatsoever, although the patient's mother was exceedingly anxious about her.—Obstetrical *Transactions, Vol. 2, p. 326.*

## 155.—ON ASSALINI'S FORCEPS.

By Dr. A. B. Granville, F.R.S., Vice-President of the Obstetrical Society of London.

[In a long but excellent paper "On certain Phenomena, Facts, and Calculations, respecting Propagation in Females of the Industrious Classes in the Metropolis," Dr. Granville recommends these forceps as follows:]

In an essay on the use of the forceps, with directions for the application of both the short and the long one, which I published in 1820, with a few cases, I strongly recommended, as preferable to all others, what is called Assalini's forceps, which I slightly modified. In my hands it has served both purposes. It is very handy, easier of application, and requires no cross-locking of the blades. It also gives a greater command over the fœtus to the operator, who is not puzzled with a right and a left blade. It is a curious fact that, up to the date of the publication here referred to, none of the latest writers on midwifery, subsequent to Smellie, had noticed the application of the forceps to the head, after the birth of the body of the child, or given any direction on the subject. Smellie was the first to employ the forceps under such circumstances, and evidently he considers himself to have been singular in that respect, as he says—"These two successful cases gave me great hope that the above method would be of great service." He attended two more cases afterwards. I followed his example, and gave in my essay a full detailed case, out of several, illustrative of the manner in which the application should be accomplished. In that essay I dissent from the ordinary received canons respecting the use of the forceps, which, until lately, had been accepted as infallible, namely, that the head of the child shall have rested on the perineum for six hours before we thought of applying the forceps, according to Denman, and that the forceps may be applied only when an ear of the child can be felt from the os externum. Both rules are bad as far as they were intended to be exclusive.—*Obstetrical Transactions, Vol. 2, p. 172.*

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## 156.—AN INTRA-UTERINE FIBRO-PLASTIC TUMOUR REMOVED BY ENUCLEATION.

By Dr. J. Hall Davis, Physician to the Royal Maternity Charity.

[In this case Dr. Davis detected a hard circumscribed body which seemed to be the uterus extending itself above the pubes.]

On now examining per vaginam, I found the uterus enlarged, the ordinary projecting vaginal portion of the cervix obliterated, owing to the development which the uterus had passed through for the accommodation of a growing tumour. The orifice of the uterus was open to the size of a half-crown, and within it I found a roundish tumour, smooth, and fibro-fleshy to the touch, if the expression may be used. I found this growth adherent to the anterior walling of the uterus, also to the right walling down to the margin of the orifice. Likewise to the left walling to within an inch of the edge of the uterine orifice. Posteriorly the tumour was quite free, as high as the fundus of the uterus, which the uterine sound informed me was distant from the uterine orifice between four and five inches, in other words, that was the depth of the cavity of the uterus. I could not pass the sound from behind round the tumour at the sides, it being there stopped by the lateral adhesions.

It occurred to me that, if I could get my finger through and within the reflection of mucous membrane passing from the cervix to the inferior surface of the tumour, I might succeed in gradually detaching or enucleating the tumour from below upwards. I proceeded first in front, and detached the tumour up to a line, beyond which I could carry the separation no further. I then detached the tumour laterally, and here a little higher than in front. My impression was that at least half, if not two-thirds, of the growth was now free and unattached. I now desisted, fearing to do mischief, the remaining attachment being very firm.

Very little hemorrhage attended or followed these proceedings. I washed out the uterus with tepid water. The patient passed a good night, without pain.

I next guided upon my index-finger a pair of vulsellum or Museaux's forceps; with which I got a firm purchase upon the lower part of the tumour, and gradually dragged down the mass, so that the most dependent part (indeed all below the ligature) which was now putrid, flaccid, and very compressible, passed into the vagina. Small portions broke away from time to time, upon which I got a firmer hold higher up. I cut off some portions, and proceeding with my traction, I at length brought the putrid part, which forms nearly two-thirds of the tumour, through the os externum. I now obtained a glimpse of the upper, undecomposed part of the growth. During this time firm pressure was being applied over the fundus of the uterus above the pubes.

The patient now complained much of the traction. I therefore examined and satisfied myself that I was not inverting the uterus. This organ had been brought altogether lower into the pelvis, and after the larger part of the growth had appeared externally the uterus had undergone some contraction of its muscular fibres and of its bulk. I proceeded with my traction, getting a fresh hold higher up, and now had the yet extensive attachment of the tumour under my command, when, by a combination of traction and twisting of the tumour close to its adhesion, I succeeded at length in brushing it away, as it were, cleanly from its connection to the uterus, and so delivered the patient of her unnatural offspring.

I now washed out the uterus again with the same lotion as before; scarcely any blood came away. Some port wine and water were given during and after the operation, which, owing to the slowness of proceedings that I judged advisable, occupied nearly half-an-hour. I gave no opiates by the mouth, as they always disagree with the patient when so taken. I had a laudanum poultice of linseed meal applied to the hypogastric and sacral regions, where she complained of pain.—*Obstetrical Transactions*, Vol. 2, *p.* 19.

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## NOTICES OF NEW WORKS LATELY PUBLISHED.

157.—*On Myalgia, being a Treatise on Painful and other Affections of the Muscular System.* By THOMAS INMAN, M.D. (Lond.). Physician to the Liverpool Royal Infirmary.—*London*, Churchill, 1860. pp. 307.

[We have already given so many papers by Dr. Inman on this subject in several of our volumes, that we need only say here, that this work places before the reader, in a connected form, all the views of this talented physician. It has given us considerable instruction with respect to the subject of myalgia, which is so liable to be mistaken for neuralgia, that we recommend all medical practitioners to make themselves masters of Dr. Inman's views.]

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158.—*On Asthma, its Pathology and Treatment.* By HENRY HYDE SALTER, M.D., F.R. S., Assistant-Physician to Charing-Cross Hospital.—*London*, Churchill, 1860. pp. 372.

[We must say of this (as of Dr. Inman's work on Myalgia) that we have already presented our readers with most of Dr. Salter's views on the subject of Asthma, reprinted from the pages of the weekly journals. The present volume embodies the author's views on this most intractable disease. It is the best work on the subject with which we are acquainted, is ably and pleasingly written, and contains the results of a vast amount of original observations, especially on the effects of remedies in this disease.]

159.—*On the Reparative Process in Human Tendons, after Subcutaneous Division for the Cure of Deformities.* By WILLIAM ADAMS, F.R.C.S., Surgeon to the Royal Orthopœdic and Great Northern Hospitals.—*London*, Churchill, 1860. pp. 174.

[This work is illustrated by numerous lithographic plates and woodcuts, and altogether forms a complete treatise on the subject, well worthy of general perusal and study. Mr. Adams' conclusions are, that:

1. Tendon is one of the few structures of the body which is reproduced.
2. When a tendon is divided and separated, the interval is filled up by new tendon, which re-unites the divided ends of the old tendon.
3. This is accomplished alike in man and animals.
4. The new tendon is liable to adhere to the neighbouring fibro-cellular tissue, and may thus spoil the cure, without great care.
5. The reparative process may be spoiled by the too early and too rapid mechanical extension afterwards.
6. It is not necessary to approximate the divided ends of the tendon soon after the operation, and then to *stretch* the new growth in order to make it long enough, thus gradually stretching it for a month or six weeks, like a bit of vulcanized india rubber; but, on the contrary, "the required length should be obtained during the time occupied in its formation, *i.e.*, in about two or three weeks." Therefore the object of gradual extension is, "to regulate the length of new tendon, by forcibly overcoming ligamentous resistance in some cases, and by preventing a too rapid and excessive separation of the extremities of the tendon in other cases."

These are the most important and practical conclusions; but Mr. Adams' work itself must be referred to for more information.]

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160.—*A Practical Treatise on Diseases of the Skin in Children.* From the French of CAILLAULT. By ROBERT HOWARTH BLAKE, M.R.C.S.—*London*, Churchill, 1861. pp. 277.

[The skin diseases occurring in children are very frequantly most obstinate in character, and the reputation of the medical attendant is much injured, if, month after month the little sufferer remain unrelieved, as we have frequently known to be the case. To those in large midwifery practice this little work would be of much service, as it is concise and practical. The following extract on the treatment of Syphilis will probably prove interesting to our readers:]

We have endeavoured to show, contrary to the opinions of many authors, that hereditary syphilis is rare, and that many syphilitic parents generate children not merely free from any syphilitic taint, but perfectly healthy and vigorous. What practitioner has not had patients who were undoubtedly syphilitic, and yet whose children enjoy robust health?

The existence of syphilis, however, being once positively established in a new-born infant, what is the most proper treatment to be adopted? and at what period should it be commenced? These are the first two questions which arise when a case of infantile syphilis occurs.

We have noticed the marked differences which distinguish adult from infantile syphilis; and, notwithstanding the numerous and fruitless attempts to make their stages correspond with each other, it has always been found impossible to arrive at any useful practical results founded on these false analogies. Thus, *à priori*, iodide of potassium would seem to be indicated when the infant exhibits symptoms more or less resembling the tertiary lesions of the adult, and still more so when the syphilitic child is born of parents suffering from the characteristic symptoms of that diathesis.

All these theories are valueless in the face of clinical observation. If we read attentively the numerous cases of recovery from infantile syphilis, and note the curative means which have been employed, we shall be convinced that in the majority of them the existence of syphilis is more than doubtful; and, further, that the variety of these lesions is so great that the changes

they undergo under the influence of therapeutic means will not warrant us in forming an opinion as to the superiority of any remedy with reference to any given symptom.

In accordance with our own observations we think that mercury should be prescribed in the form of proto-iodide; the bichloride requires too much dilution to admit of its administration to new-born children. (p. 40.)

[It was formerly a favourite plan to administer mercury to the infant through the medium of the milk of the mother, or of an animal. This practice is still followed in Paris, where several establishments exist in which various medicaments are introduced with more or less success into the milk of animals. The constitutional effects of mercury are very readily produced in infants by cutaneous friction, but M. Caillault prefers the use of mercurial baths, containing from fifty centigrammes to a gramme, (7—15 grains) of bichloride of mercury. No bad effects result from their use, and their favourable local action in these cases is sometimes quite surprising. M. Guillot, an authority quoted by Caillault,]

Prefers to all other remedies, a julep containing twenty-five milligrams of the protiodide of mercury, a tablespoonful of the liquid, previously shaken, to be given every quarter of an hour. If the medicine gives rise to vomiting, which is by no means unusual, the interval between the doses should be half an hour. If, notwithstanding this precaution, the infant should continue to vomit, or diarrhœa or colic should supervene, the administration of the medicine must be suspended. Contrary to the precepts contained in many books, M. Guillot advises great caution with regard to the doses of protiodide, administered internally, but he recommends us to increase the strength of the mercurial bath, which may be done with safety. (p. 43.)

The questions with regard to the duration of the mercurial treatment and of the most appropriate time for its commencement are not precisely stated by authors. The greater portion of them do not praise any preventive treatment. It seems to us that we should at least wait, before inflicting a mercurial treatment upon a new-born child, until the diagnosis is confirmed by a strict investigation, knowing how frequently healthy children issue from a tainted source. The duration of the treatment should not generally be in proportion to the persistence of the lesions. When the child regains its strength and flesh, when all the functions proceed regularly, it is not advisable to continue the anti-syphilitic agent until the cutaneous lesions disappear, for if administered beyond a certain time, it produces derangements of the bowels which require its discontinuance. The mercurial baths alone may be continued without any inconvenience, taking care to administer them at shorter or longer intervals, according to the exigencies of particular cases.* (p. 44.)

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161.—*A Manual of Minor Surgery and Bandaging.* By CHRISTOPHER HEATH, F.R.C.S., Demonstrator of Anatomy at the Westminster Hospital.—*London*, Churchill, 1861, pp. 208.

[As stated in the preface, the object of this work is to present, in a concise form, those minor points not contained in larger or more systematic works on surgery; yet, which details are inexpressibly useful in practice, and much contribute to success. The work is chiefly intended for the use of house-surgeons and dressers to hospitals. We extract a few of the most useful passages.]

*Stabs in the chest* may produce hemorrhage by wounding (rarely) an intercostal artery, or the lung. An intercostal artery may be twisted or tied, and seldom gives much trouble. Wound of the lung (generally shown by bloody

* The administration of the more powerful preparations of mercury, such as the iodide and the bichloride, is, we think, quite unnecessary in cases of infantile syphilis. The result of our own experience is in accordance with that of other [illegible] that [illegible] cases may be both speedily and safely cured by hyd. c. creta and [illegible] to the hygienic and dietetic means referred to in the [illegible] of infantile syphilis, potassium twice a day are quite sufficient.

expectoration) may be irremediably fatal from hemorrhage, the blood either pouring from the mouth or choking the lung, or more rarely filling the pleura and compressing the lung. The great object is the immediate arrest of the hemorrhage, by the inhalation of the vapour of turpentine, sprinkled on a handkerchief, the application of cold both to the surface of the chest and by swallowing ice, and lastly by venesection if necessary. Venesection for this purpose should be performed while the patient is supported in the erect posture, and the blood should be drawn from a large aperture, so as to induce a fainting condition as rapidly as possible, and care must be taken that the patient be not allowed to recover his heat and rapid circulation too soon, or the hemorrhage may recur. An early opportunity should be taken of administering one of those drugs which have an hæmostatic tendency (*e.g.*, plumbi acet., gr. j, in pil.; or acidi gallici, gr. v), in repeated doses. (p. 17.)

*Wounds of joints.*—Wounds of joints, if not obvious at first, are soon made evident by the escape of synovial fluid, which trickles out, and can easily be distinguished from blood by its light colour and tenacity. Closure of the wound is of the first importance, provided no foreign body is left in the synovial cavity. In the case of punctures or small incisions, collodion forms the best application, the same precautions being used as in cuts on the face. In the absence of collodion, white of egg is not a bad application; and plaister should be used to support the parts, and prevent the wound being dragged open. When the wound is so large as to require the application of stitches, care should be taken not to insert them through the synovial membrane, and collodion may be advantageously applied over them. Perfect rest and the application of cold are the best preservatives against inflammation of the joint; and a splint should therefore be applied to the limb, which must be kept, if possible, in an elevated position. The posture which is easiest, and which relaxes all the parts most, must be the best at first; but, should inflammation come on, care must be taken to place the limb in a position in which it may ultimately be useful, should the motion in the joint be lost or impaired. Irrigation with cold water is the readiest and most certain method of applying cold to a wounded joint. (p. 37.)

*Scalds of the glottis* require special notice and treatment. The patient is generally a child who has attempted to drink boiling water from the spout of the tea-kettle, and has consequently injured the mouth, fauces, and upper part of larynx, more or less severely. The symptoms of dyspnœa will vary according to the time which has elapsed since the accident, and the amount of damage done. If the little patient is evidently *in articulo* from want of breath, the house-surgeon had better perform tracheotomy at once; but if the symptoms are not of such extreme urgency, the operation should be deferred, for a time at least, in order to try the effects of treatment. The first thing is to place the patient in a warm and moist atmosphere, and this can be contrived by placing an ordinary surgical cradle over him, and under the bedclothes, or a piece of Macintosh cloth, and then bringing the steam of a kettle beneath the canopy thus formed. Care must of course be taken that the temperature be not raised above 80°, or the patient will be suffocated and parboiled. Relief may also be attempted by scarifying the back of the throat, epiglottis, &c., which has been known to be of great service in some cases.

As regards medicines, antimony and ipecacuanha appear to offer the best chance of success, and they may be most conveniently administered in the form of the wines. Large doses of either (and antimony by preference), according to the child's age, may be given, and frequently repeated, until the breathing is relieved. Vomiting is not to be wished for, and will seldom be produced. Mercury may be combined with the antimony, and to be of any service must be administered in heroic doses and frequently; but should the [illegible]hing become more embarrassed, the operation of tracheotomy must at [illegible] had recourse to. (p. 45.)

[illegible]*-and-Gum bandage* is applied in exactly the same way as the starch [illegible]e adhesive mixture is made by adding boiling water to equal [illegible]arabic and precipitated chalk; and this material has the advan-

tages over the starch, both of becoming firm sooner, and of having more strength, so that the addition of strips of pasteboard is rarely necessary. (p. 150.)

[The work also includes the minor operations of surgery, clearly and concisely given: the dressing of wounds, the diagnosis and treatment of fractures and dislocations (chiefly the details omitted necessarily in systematic works); and concludes with a few pharmacopœial formulæ and diet tables from the London hospitals.]

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162.—*Operative Surgery adapted to the Living or Dead Subject.*—By C. F. MAUNDER, F.R.C.S., Demonstrator of Operative Surgery, London Hospital.—*London*, Churchill, 1861.

[This useful little work is contained in two small volumes. It will chiefly prove of service to students practising operations upon the dead body. The following short extract on a method for perineal section, as practised by Mr. Cock, is new to us.]

When attempting to open the urethra in the perineum, the subject should be placed in the lithotomy position. The patient being so placed, the operator's left forefinger, well oiled, should be passed into the rectum to ascertain the exact position of the apex of the prostate, and be there maintained; with the right hand a double-edged scalpel incises the median line of the perineum anterior to the anus, the point of the knife being carried back, with the intention of opening the membranous urethra in the direction of the apex of the prostate, to which latter the finger in the rectum serves somewhat as a guide. As soon as the knife has entered the perineum, it should not be withdrawn till the requisite section of parts is completed.

This mode of treatment is very applicable to cases in which extravasation of urine has occurred behind a stricture. A ready means of escape is thus offered, both to the urine already poured out into the surrounding parts and also to that afterwards secreted. While the perineal wound is patent, the stricture may be treated by the usual method. (p. 156.)

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163—*Ready Rules for Operations in Surgery.* By ALLEN WEBB, M.D., F. R.C.S.L., Presidency Surgeon, Surgeon Superintendent to the Native Hospital, Calcutta, &c., &c.—*London*, Churchill, 1861.

[Most surgeons when about to operate are particular to see that all the instruments and assistants are properly arranged. Dr. Webb's book is the best of the kind we have seen, and will greatly help to facilitate the arrangements previous to an operation. It ought to be in the possession of every operating surgeon, and in every hospital. Two pages are devoted to each operation. The book being laid open, on the left page are the names of all the instruments required; and, printed in red letters, every thing that each assistant has to do, "the great aim being that the assistants should know what to do in concert with the operator, at the very time that the surgeon requires it done." On the right page are directions for the operator himself: and space enough is left on each page to add any further directions which he may think necessary.]

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164.—*The Composition of the Urine in Health and Disease.* By EDMUND A. PARKES, M.D.—*London*, Churchill, 1860, pp. 404.

[This is a most complete work on the urine; the whole subject is thoroughly and practically considered. The first part of the work treats of the urine in health, and the variations caused in its composition and amount by every conceivable physiological condition, and also by the use of medicinal agents. The second part treats of the urine in disease, the abnormal constituents and urinary sediments; then the state of the urine in every separate disease is treated of in a most able and concise manner. We give one or two brief abstracts on points worth remembering.

In all congestive diseases of the liver, the absorption of water from the stomach and intestines is so much lessened in amount that a copious or even natural flow of urine is at once a strong argument against the existence of congestion or cirrhosis of the liver.

On the subject of diabetes, Dr. Parkes observes, that it is not absolutely proved that all the starch ingested by diabetics appears as sugar in the urine, but this is probable. In two patients, examined carefully for two months, the quantity never exceeded the amount of starch, and almost all the starch was accounted for by the diabetic sugar. In one case abstinence from starchy food completely freed the urine from sugar. In the later stages of the disease sugar is not, however, formed entirely from the starchy ingesta, but also from albuminous foods.

We think this work well suited for a book of reference; and to medical men in extensive practice it would prove very useful, as the contents are so arranged, that, without much trouble, the state of the urine in any particular disease may be referred to; also the action or effect of any internal remedy upon the urinary secretion.]

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165.—*On Urine, Urinary Deposits and Calculi.* By LIONEL S. BEALE, M.B., F.R.S., Physician to King's College Hospital.—*London*, Churchill, 1860, pp. 424.

[Few books have afforded us more pleasure in perusal than this of Dr. Beale's. We give some extracts upon the most important parts, which will show the character of the work, and also we hope prove interesting to our readers. Speaking of the circumstances affecting the formation of urea, the author observes:]

In all probability, urea is formed in the organism by the oxidation of uric acid. The latter substance, if the oxidating processes in the body are active, becomes resolved in great measure into urea and carbonic acid; but if, on the other hand, these processes are less active than they should be, the uric acid in the urine is increased in quantity; a certain quantity of oxalic acid, and other substances of a lower degree of oxidation than urea, seem to be produced, instead of nearly the whole of the uric acid being resolved into this soluble substance. Wohler and Frerichs have shown that, if uric acid be taken at night, oxalate of lime is found in the morning urine: and Neubauer found that, when rabbits were made to take a considerable quantity of uric acid with their food, the urea in their urine increased from 1·34 to 4 grammes (from 20·67 to 61·72 grains). Large quantities of fluids cause an increase in the proportion of urea formed in the organism. A dilute state of the solids is favorable to their oxidation; and in certain conditions, where these changes are but imperfectly carried on—and in consequence uric acid accumulates in the blood, or at most is resolved into oxalic acid—the further oxidation is promoted by the administration of increased quantity of fluid, especially of fluids containing alkalies, which not only increase the activity of the changes, but effect the solution of the insoluble uric acid and urates. Hence the benefit of alkaline waters, baths, moderate exercise, and plenty of good air, in gout and other conditions in which much more uric acid is formed than can be, under ordinary circumstances, converted into urea.

The quantity of urea excreted is also increased by common salt. (Bischoff). It is probable that not only does chloride of sodium, so to say, filter through the different tissues, like other saline substances, and thus drive out certain materials which are contained in their interstices; but that it also facilitates the occurrence of chemical change in the body, and directly influences the quantity of urea formed. The importance of chloride of sodium to cell-growth and the development of different textures, and its value in nutrition, are well known. (On the Chlorides in Pneumonia, *Med.-Chir. Trans.*, vol. xxxv.) The beneficial effect of alkalies in different cases is generally acknowledged; and it is probable that this is in part to be explained

by the influence they have been proved to possess in promoting chemical change in the body, and especially in favouring the oxidation of albuminous substances. (p. 24.)

On the other hand, both the solids and fluids of the urine are diminished by alcohol; so also is the proportion of carbonic acid exhaled. Tea causes a diminution both in the quantity of urine and fæces, as the beautiful researches of Dr. Böcker have conclusively proved. (*Medico-Chirurgical Review*, vol. xiv.) Coffee exerts a similar effect, which seems to be due, not to the caffein, but the empyreumatic oil which it contains, according to Julius Lehmann. These substances, tea, coffee, and alcohol, in moderate quantity, affect the disintegration of tissue, and directly diminish the quantity of the excrementitious substances formed in the process. Supposing the food to be insufficient, the loss of weight which must necessarily take place in the body would be lessened; and they may therefore be regarded as advantageous, not only in economising the food, but in limiting to some extent the waste of the albuminous tissues. (p. 25.)

[The following extract is from a paragraph treating of increased acidity of the urine:]

A highly acid condition of the urine, persisting for a long period of time, may cause the precipitation of uric acid, and so lead to the formation of a calculus. Acid urine not unfrequently causes irritable bladder, and excites other morbid actions. In most cases, the salts of the vegetable acids (citrates, ascetates, tartrates) will be found more efficient in counteracting this acid state of the urine, than alkalies or their carbonates, and are less likely to interfere with the digestive process. There are, however, low conditions of the system in which the acid state of the urine, and a tendency to the deposition of uric acid in large quantity, are not relieved by alkalies; on the contrary, such cases are often much benefited by an opposite plan of treatment—tonics and the mineral acids before meals, a nourishing diet, with a moderate supply of simple stimulants with a little alkali, or with alkaline waters. Pepsine is often of great use in these cases. Many of them seem to be intimately connected with impaired digestive power. The acid state of the urine may then depend upon very different conditions of the system, and these must be carefully considered in each individual case before any plan of treatment is suggested. (p. 82.)

[There is a class of cases, in which the urine contains a positive daily excess of urea. The symptoms are as follows:]

The patient is weak, and grows thin, in spite of taking a considerable quantity of the most nutritious food. He feels languid, and indisposed to take active exercise. In some cases, digestion is impaired; in others, the patient eats well, experiences no pain or uneasiness after food, and perhaps has a good appetite. Sometimes there is lumbar pain. It would seem that much of the albuminous material in the blood, instead of being applied to the nutrition of the tissues, becomes too rapidly converted into urea, and is excreted. The waste of the tissues is not properly repaired, and the patient gets very thin. To refer these symptoms to the existence of a particular diathesis, appears to me no explanation of the nature of the case. The pathology of these remarkable cases has not yet been satisfactorily investigated. Mineral acids, rest, shower-baths, and good air, often do good; but some of these patients are not in the least benefited by remedies, and they continue for years very thin, passing large quantities of highly concentrated urine, while the appetite remains good, and they digest a considerable quantity of nitrogenous foods. I am now trying, in one of these cases, which has resisted the usual plans of treatment, the effect of pepsine, with diminished quantity of meat, and a larger amount of farinaceous food. The condition often lasts for some years, and then the patient's health improves, and he gets quite well. (p. 90.)

[The following will show Dr. Beal's opinion of what is usually called the oxalic acid diathesis.]

We shall see that oxalate is one of the commonest urinary deposits; that

it results from decomposition; that there is no reason for believing it to be indicative of any peculiar diathesis or habit of body. (p. 91.)

[The occurrence of pus in the urine must always prove of much interest in a clinical point of view. In one case it may be of no importance, in another it may show serious disease, and every medical man ought to be able to form some opinion whence the pus is derived and what it indicates. In the female, small quantities of pus may be derived from the mucous membrane of the vagina, and may be of little or no importance. Pus may come from any part of the genito-urinary mucous membrane, or from an abscess opening upon any part of its surface.]

It is often difficult to form an opinion as to the exact seat of formation of the pus; and it must be obvious that we ought never to come to a decision on such a point until we have accurately weighed all the evidence that a careful investigation of the case will afford. Microscopical examination will give us important help; but we must not rely upon this, or indeed upon any single mode of investigation. The question is an extensive one, and I shall only refer to one or two points connected with the evidence deduced from microscopical examination, as you are quite familiar with the nature of the other evidence to be obtained in such cases. You may often form some idea of the locality from which the pus has been derived by examining attentively the characters of any epithelial cells which may be mixed with it. (Lecture IX.) When pus is derived from the bladder, it generally contains crystals of triple phosphate, and granules or small spherules of earthy phosphate mixed with it; and the symptoms of the case will generally enable you to decide if the pus is formed in this viscus. Large quantities of pus may escape from the bladder for a number of years. I know of one gentleman who has passed pus in considerable quantity from the bladder during a period of twenty-five years. The suppuration of the bladder may be due to gonorrhœa, to gout, and to a state of mucous membrane which is termed catarrh of the bladder. When the pelvis of the kidney is dilated and sacculated (a form of pyelitis), the quantity of pus passed in the urine is often enormous; and this may last for years, until the kidney becomes a mere pus-forming cyst, which, in favourable cases, gradually contracts; the formation of pus ceases; the cyst slowly wastes; and the patient perfectly recovers; the work of the two kidneys being performed by the remaining one, which has gradually undergone an increase in size corresponding to the increased work it has been called upon to perform. When pus is derived from the pelvis of the kidney, the earthy phosphates are often completely absent. There is a chronic state of ulceration of the ureters and pelvis of the kidney and bladder, in which pus is formed in considerable quantity, leading to the most distressing symptoms. Pus may depend upon the existence of old stricture.

Abscesses form in the kidney as in other organs; and, after the abscess has burst, pus makes its way into the urine. The inflammation of the mucous membrane of the kidney often extends upwards from the bladder. The presence of a calculus in the kidney, in the ureter, or in the bladder, may set up inflammation which may go on to the formation of pus. A very small calculus will sometimes excite great irritation in the kidney, so that both pus and blood are voided in the urine. (p. 281.)

[The importance of increasing the quantity of fluid taken by persons who suffer from certain varieties of urinary deposits must suggest itself to every one, but, from its simplicity perhaps, it rarely receives that amount of attention in the treatment of affections which it deserves.]

It is doubtful if that abundant deposition of urate of soda which is from time to time met with in almost all parts of the body, in certain cases, would have occurred at all, if the fluids had been constantly maintained in a proper state of dilution; and when these crystals have been deposited, we endeavour to remove them, or prevent further deposition, by diluting the fluids of the body, and by endeavouring to increase the solubility of the urate. We are perhaps too apt, in many chronic cases, to put patients upon a plan of treatment for so short a time as a few days or weeks; and our patients are

often unreasonable enough to expect that remedies will remove in a week, matter which has been slowly accumulating perhaps for years. It is chronic cases of this kind which receive such real benefit from the comparatively prolonged course to which they are subjected in a German bath or hydropathic establishment; and it too often happens that, in endeavouring to perform quickly by remedies that which it is only possible to effect by giving large quantities of fluid during a considerable period of time, we disappoint ourselves and our patients; and perhaps in the end they attribute to some quack remedy or system, to which they have subsequently had recourse, a favourable result which is really due to the water they have drank and the rules to which they have been subjected, instead of to the nostrums they have swallowed.

In certain cases of gout, in chronic rheumatism, and in many cases where uric acid and urates are constantly deposited in the urine or in the tissues of the body, the most important of all things is to ensure the thorough washing out of the system. Exercise when it can be taken, hot baths, Turkish baths, &c., by promoting sweating, excite thirst; and thus more fluid is ingested, which is soon got rid of by various emunctories, carrying out with it insoluble substances, the fluid removed being soon replaced by a fresh quantity. In the frequent repetition of these processes from time to time, a vast quantity of fluid is made to pass through the body, with the most beneficial effect.

You would be surprised how very little fluid some persons take as a rule; and this fluid, small as it is, is often saturated with soluble substances. The fluid thus introduced is, in many persons who live well, barely sufficient to hold the various compounds in solution while undergoing chemical change. Many dislike to drink water, and not a few have a strong prejudice against it; and these are often the very individuals whom we find suffering from gout, rheumatic pains in the muscular and fibrous tissues, and various forms of urinary deposits. They will receive the greatest benefit from moderate sweating, and alkalies dissolved in a large quantity of water. You will seldom find difficulty in prevailing on patients to take seltzer, vichy, or other alkaline waters daily, although it would be useless to recommend them to take pure water. They can take them with their wine at dinner, the last thing at night, and perhaps the first thing in the morning. People who live well, or rather too well, will soon find out that they must continue this plan, and take now and then small doses of alkalies. It is quite superfluous for me to enter into the minute details applicable in individual cases; but I cannot too strongly recommend you to study very carefully the different symptoms in cases of this class; for I feel sure that much permanent relief may be afforded such patients by explaining to them the importance of constantly attending to simple rules based on the principles to which I have adverted. Although I may have been somewhat tedious, I feel sure that you will not think that I have dwelt longer on these points than their practical importance demands; and I am convinced that we shall practise our profession with greater advantage to our patients, and advance its interests more, by studying carefully the nature of the actual processes going on in disease, and considering how these processes are to be modified by simple means and a few remedies whose action is certain and well understood than by hunting for new specific medicines, or combining together a greater number of compounds, many of which are completely modified as soon as they enter the stomach, and are certainly destroyed long ere they reach the part of the organism where we desire that they should exert their specific influence. (p. 356.)

[It is our belief that scientific medicine will make more advance by the study of animal chemistry than by any other means, and we always view with pleasure such a work as this of Dr. Beale's, which is pleasant to read and full of instruction on subjects of practical importance.]

# A SYNOPSIS,

CONTAINING A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE PRECEDING PAGES: SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

## AFFECTIONS OF THE SYSTEM GENERALLY.

FEVERS.—All animal poisons have an *adhesive* attraction for surfaces, especially the walls of a room in which the case has occurred. Hence, in all fever hospitals, and in private houses, after a severe case of typhus has occurred there, it is well to have the walls lime-washed, and those of the hospitals every month or so. (Dr. S. J. Goodfellow, p. 70.)

GOUT.—*Pressure.*—In some cases the pain in the affected joint is really agonizing; yet, though the aggravation of pain is great on any sudden pressure however slight, gentle steady pressure relieves it. Enclose the foot in cotton wadding, so as to make the pressure more easily applied, and more even; then take an elastic bandage, and, beginning at the ankle, gradually apply it round the foot, and, last of all, very gently and evenly round the toe. The relief experienced is remarkable. (Dr. C. B. Garrett, p. 276.)

SCARLATINA.—In all cases of scarlatina, if a moderate but efficient daily action of the bowels is secured, and the action of the skin promoted, we do not run much chance of the supervention of scarlatinal albuminuria. If, however, there is the slightest appearance of the disease assuming a malignant or severe form, immediately give iron in the form of the tincture of the sesquichloride. Along with other tonic treatment it will be found to have a most beneficial action. (Dr. S. J. Goodfellow, p. 69.

SCROFULA.—*Arseniate of Soda.*—In cases where the diathesis has not as yet produced a cachectic condition, and where the local manifestations are superficial, and confined to the skin, the mucous membranes, and suppurated lymphatic glands, there is no tonic so efficacious and energetic as arseniate of soda. It is an admirable succedaneum of iron, quinine, and cod-liver oil. It ceases to act as a tonic if given in too large doses. The one-fourteenth of a grain may be given at first, according to the age, and increased gradually up to one-fourth of a grain. It may be given in infusion of cinchona as a vehicle. (Dr. Bouchut, p. 27.)

SCURVY.—The main risk in cases of scurvy, as seen in sailors lately landed, is from fatal syncope; hence in the treatment of these cases the recumbent posture must be strictly maintained, and lime-juice must be given to the extent of two or three ounces daily, qualified by a proper amount of sugar. Chlorate of potash will be found of great value in all cases in which the gums are very tender and spongy; and, where there is much irritability, or sleeplessness, opium may be given with advantage. (Dr. S. H. Ward, p. 26.)

## AFFECTIONS OF THE NERVOUS SYSTEM.

EPILEPSY.—The author generally commences the treatment of a case of epilepsy by the administration of belladonna in quarter-grain doses twice a day, in pill or mixture. It is a curious observation, that the usual effects of belladonna are rarely produced in cases of epilepsy, or any other convulsive disease (as shown previously in cases of chorea, if we remember right, by Dr. West). In those cases in which there appears to be a tendency to regular recurrence of the fits, give large doses of quinine; five, ten, and even

fifteen grains to be given at intervals, shortly before the occurrence of the fit. By this means the postponement of the fit is procured. In cases in which the aura epileptica arises from one of the limbs, a ligature should be worn constantly on the arm, and tightened immediately the aura is felt. When an epileptic patient complains frequently of either a pain or a sensation proceeding from some part of the body, the actual cautery should be applied locally to this part. The effect on the fits is in many cases most marked, their number being reduced considerably. (Dr. Brown-Séquard, p. 44.)

[The treatment of epilepsy by belladonna is insisted on chiefly by Trousseau, who continues the use of the remedy, if necessary, for three or four years. (For his mode of administering it, &c., see *Retrospect*, vol. xxxii. p. 54.) We have ourselves seen a patient, of whom Trousseau publicly asserted that he was confident of curing him by the prolonged use of belladonna; he was, however, uncured at the date when we saw him, and would probably remain so, as the case was one of rotary epilepsy.—EDS.]

NEURALGIA.—*Hypodermic Injection of Morphia.*—The effects of hypodermic use of morphia require to be so carefully watched, and the dose to be so nicely graduated, that, except where the case is very urgent or obstinate, it is much better to apply the morphia endermically (the cutis being first denuded by a blister). One death at least has been known to result from injecting morphia subcutaneously, though here proper care was probably not taken. (Dr. E. H. Sieveking, p. 39.)

An ointment composed of two grains of *veratria* to the ounce of lard will frequently relieve neuralgic pain, if well rubbed into the part. The part feels deadened and a slight rash appears. (Dr. E. H. Sieveking, p. 40.)

PARAPLEGIA.—Cases of paraplegia may be divided into two groups; 1st. Cases in which there is an increased amount of blood in the spinal cord or its membranes. There are always symptoms of irritation, convulsions, cramps, twitchings, erection of the penis, from irritation of motor tract; formication, itching, pricking and other pains, abnormal feelings of cold or heat, of tightness, pressure, &c., from irritation of the sensory tract; diminution of temperature of the paralyzed limbs, wasting of muscles, œdema, bed-sores, alkaline urine, &c., from irritation of the ganglionic fibres. 2nd. Cases unaccompanied by any of these symptoms of irritation. In these, there is a diminished amount of blood in the cord. The former are cases of inflammation or congestion; the latter are cases of white softening, or of reflex paralysis. In the former only those remedies must be used which diminish the amount of blood in the cord; in the latter those remedies which increase it. *Belladonna* causes contraction of the vessels of the cord, and diminution of the amount of blood; so do ergot of rye and mercury, but mercury has such a depressing influence that it is better not to employ it, except in cases of syphilitic paraplegia. Strychnine increases the amount of blood in the cord, and should be used only in cases in which there is no sign of irritation of the cord; ammonia, sulphate of quinine, iron and cod-liver oil, are also very useful in similar cases. Iodide of potassium is one of the most powerful agents in the absorption of effused fluids; it may be employed in either class of cases, but it is especially useful in cases of syphilitic paraplegia. (Dr. Brown-Séquard, p. 34.)

TETANUS—*Aconite.*—The surgeons of the Middlesex Hospital place their patients, from the first appearance of the tetanic symptoms, under the influence of aconite. At first, three minims of Fleming's tincture are given every three hours, and increased subsequently to five minims, the pulse being carefully watched. At the same time acetate of morphia is injected subcutaneously once or twice a day. The diet given is liberal, wine and beef-tea as much as can be taken. (Ed. of Lancet, p. 43.)

The *tobacco enema*, half-a-drachm to the half-pint of boiling water, is much praised by Dr. Wood, of Philadelphia. It must be repeated every two or three hours until its relaxing effects are produced. The nervous system is much more readily impressed by tobacco than by opium. (Ed. of Lancet, p. 43.)

[We were much struck lately in reading Abercrombie's work on "Diseases of the Stomach" by the statement that he has used the tobacco enema very frequently (in cases of ileus,) but has never seen any of the dangerous symptoms so often mentioned by authors, and the fear of which have almost excluded it from practice.—Ed. of *Retrospect.*]

WASTING PALSY.—*Galvanism.*—The important thing is, to pass the current through the substance of the affected muscles, and not along the nerves leading to them, nor into the spinal cord. When a muscle wastes owing to paralysis of its nerves, the cause is a lesion of the nutritive operations in the muscles. The two poles of the battery should consist of small brass balls, closely covered with sponges, which may be thoroughly wetted before use. The sponges should not be placed more than an inch-and-a-half apart, and be well pressed in. No sitting should be protracted for more than ten or fifteen minutes, and the affected muscles must be successively stimulated, the operator passing rapidly from muscle to muscle. (Dr. W. Roberts, p. 34.)

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## AFFECTIONS OF THE CIRCULATORY SYSTEM.

LIGATURE OF THE FEMORAL BELOW POUPART'S LIGAMENT.—The doctrine is usually held that the vicinity of a collateral branch, however small, to a ligature, vitiates the process of union, by preventing the effusion of agglutinative lymph. This, however, is not the case; an artery may be tied within a short distance of a large branch on its cardiac side, and if the constitutional health is good there will be no danger of hemorrhage from this cause. One great danger in tying the femoral artery is wounding the vein; this is much better avoided, if the artery be tied about half-an-inch or rather more below Poupart's ligament, where both artery and vein are on the same level, and side by side. In this situation the avoidance of the vein is perfectly easy; in the place usually selected it is sometimes impossible to avoid it. It is extremely easy to tie the vessel in this situation, as it lies so very superficially. The incision made must be about an inch and three-quarters in length, *across* the direction of the artery, at the distance of half-an-inch below Poupart's ligament, and exactly parallel to it. (Mr. G. H. Porter, p. 124.)

PALPITATION, WITH PROMINENCE OF EYEBALLS AND GOITRE.—There is a class of cases characterised by violent palpitation, with enlarged thyroid gland, and prominent eyeballs. These cases are rare, but once seen are never forgotten. There is cerebral excitement, and other symptoms of disturbance in the circulation of the brain. This disease is almost exclusively seen in females, and depends on diminution, and not increase of nervous power. The proper treatment, and one very successful, is the free use of powerful tonics, especially of the astringent kind, as perchloride and phosphate of iron, which act almost as specifics in the disease. Narcotics may be given at night with great advantage. Highly nutritious food, and pure air are required. Unless aware of the proper treatment for these cases the practitioner would be most likely to use quite an opposite plan of treatment, and aggravate the symptoms. (Dr. C. H. Jones, Dr. C. J. B. Williams, p. 284.)

PERICARDITIS.—In the treatment of rheumatic pericarditis it is well to consider the local treatment subordinate to that suitable to the primary disease. The best local treatment consists in the application of from four to six leeches, followed by fomentations. If the relief is not complete the application may be repeated. In obstinate cases, blisters may be used. Upon the whole, the disease is one susceptible to a great extent of cure, under mild palliative local remedies and fitting constitutional treatment. (Dr. W. T. Gairdner, p. 45.)

VARICOSE VEINS.—Using a blunt-pointed needle (but still sufficiently sharp to penetrate the skin), pass under the vein a piece of fine iron wire, and let it emerge at the further side of the vein; again introduce it, but so as to pass between the vein and the skin, and let it emerge at the hole at which it

was first introduced. Now twist the two ends together, and cut them off about an inch from the skin. The pressure at first must not be such as to cause ulceration of the coats of the vessel, this must only be induced by further twisting the wire, when by previous pressure the walls have adhered from plastic exudation. (Dr. R. J. Levis, p. 179.)

## AFFECTIONS OF THE RESPIRATORY SYSTEM.

ASPHYXIA FROM DROWNING.—*The Warm Bath.*—The use of the warm bath has been objected to by Edwards, Brown-Séquard, Marshall Hall, and others, but on insufficient grounds. Repeatedly patients take the first sob or inspiration on immersion in the bath. The first thing to be done is to wipe the mouth and nostrils, place the patient on his back with his shoulders raised and supported on a folded article of dress, draw forward the tongue, and inflate the lungs by Dr. Silvester's method, *i.e.*, by raising both arms up by the side of the head, and then extending them gently and steadily forwards for a few moments, so as to make the pectoral muscles draw the ribs outwards; then turn down the arms, and press them gently and firmly for a few moments against the sides of the chest; repeating these movements alternately, deliberately, and perseveringly, fifteen times in a minute. After this the warm bath must be used at once, placing the body in it up to the neck; raise the body in twenty seconds from the water, and dash cold water against the chest. Pass ammonia under the nose. Use again Dr. Silvester's method, or if this be ineffectual, the lungs must be forcibly inflated by an apparatus, or if one be not at hand, by the mouth of the surgeon. Remove the body from the bath and rub the surface with dry hot towels. This is the plan followed by the Royal Humane Society, Dr. Marshall Hall's Ready Method having been fully tried, and found to fail. (Dr. Christian, Dr. Sharpey, p. 477.)

ASTHMA.—The best way to administer *stramonium* is to collect the fumes in an inverted glass bowl, from this the patient can readily inhale. The attack is often so violent that the patient cannot smoke a stramonium cigar on account of the urgency of the dyspnœa. (Mr. T. L. Pridham, p. 51.)

PHTHISIS.—*Ozonized Oil.*—If cod-liver oil impregnated with ozone be given in full doses, a remarkable lowering in the rapidity of the pulse will be produced. This is a therapeutic fact of great importance. (Dr. E. Thompson, p. 48.)

TRACHEOTOMY.—The wound in the trachea having been made, frequently great difficulty is experienced in introducing a canula into the opening, especially in children. To effect this, insert the point of a *double* hook though the incision into the wind-pipe, and hold the latter elevated and fixed, then slip up the clasp and allow the halves of the hook to separate by their own elasticity, and so to widen the slit in the trachea; this done, the canula may be passed into the tube with comparative ease, and the hook withdrawn. (Mr. C. F. Maunder, p. 285.)

## AFFECTIONS OF THE DIGESTIVE SYSTEM.

HEMORRHOIDS.—*Ecraseur.*—It is found that, in a great number of cases operated upon by the écraseur, a large and progressively contracting cicatrix is formed, and, as a consequence, traumatic stricture of the rectum. (Lancet Correspondent, p. 135.)

HERNIA.—*Radical Cure.*—The following is one of the simplest modes of operation, requiring no complicated apparatus, and the results are perfectly satisfactory. All the apparatus required is an elongated body perforated at one end by a hole, a piece of strong thread, and a needle such as is used at post-mortem examinations. The string must be passed through the hole in the end of the body, and the needle, threaded with one end of the string, laid with its concavity resting on the forefinger of the left hand, is then passed up

along the cord, within the external ring; the needle must then be turned round, so as to bring its point upwards, and passed with an inclination to the left through the textures, and brought out on the surface of the abdomen; the other end of the thread must then be passed through the parietes in the same way, only that this time the inclination of the needle must be to the right; the two threads must now be pulled tight enough to draw the body up the canal. The piece of tube or wood introduced must be smeared with cantharides ointment, to irritate the skin, and favour the formation of adhesions. The two ends of the thread must now be tied together, and to prevent any chance of cutting through the skin too fast, a bit of elastic bougie may be placed below them. For three or four days, or a week, a compress may be placed over the groin, and retained in position by a bandage. (Professor Syme, p. 127.)

The hernia having been returned, and the patient placed in the recumbent position, grasp the sac by the finger and thumb, and slightly raise it; introduce a wire ligature under the skin, and guide it so as to include the greater part, if not the whole of the sac, care being taken not to include the cord. The curved and handled needle used may be so manipulated that the ligature is brought out not far from its point of insertion; by then withdrawing it, and again applying it to the other end, this end may be brought out exactly at the point of insertion, and the two ends may then be tied, if preferred, over a small metal plate. A compress and bandage must then be applied, and the recumbent position of course maintained. (Dr. J. Morton, p. 131.)

A small recent protrusion, which will resist every effort at reduction, can, by a force acting from within, be reduced with the greatest ease. In such cases make a vertical incision an inch or an inch and a-half above the neck of the sac, dividing the skin of the abdomen, and gradually diminishing in extent until the peritoneum is reached, which open so as to admit a blunt hook, or a pair of curved forceps, with which the intestine may be grasped and drawn inwards. Should this proceeding fail, the wound may be enlarged and the usual operation proceeded with. (Dr. J. Niven, p. 134.)

*Wutzer's Operation.*—There are several causes of failure after Wutzer's operation, which unless known and observed will bring the operation into discredit. It must only be performed in suitable cases, *i. e.*, in strong, otherwise healthy persons up to 40 or 45 years of age, who lead a life of active bodily exercise. If the ring be very large, and the canal much shortened, the operation is useless, except to render a truss effective. If the ring only admit one finger, and the canal be only an inch and a-half long, success is almost certain. In some cases the cylinder has never been within the ring at all; in others, the cylinder has never been properly adapted to the canal, or the pressure has been unequally applied. Sometimes the use of a truss is neglected after the operation, whereas the use of a truss for several months is absolutely essential to permanent success. (Mr. T. S. Wells, p. 128.)

"*The Triple Lever Truss.*"—This truss, which has been recently invented by Mr. Heather Bigg, effectually supports a hernia, having, instead of the objectionable circular metallic spring, a soft padded band. This pad contains three small levers, the pressure of which can be exactly regulated by a small button outside. It effects pressure in the same manner as the hand of the surgeon, in an upward, backward, and outward direction. (p. 134.)

Liver.—*Action of Mercury on.*—It appears now, from experiments carefully performed, and observations carefully made, that mercury is not an hepatic specific at all, and that it is more likely in fact to diminish the amount of bile secreted than to increase it. The root of the entire tissue of errors seems to result from the very evident green stools produced by the administration of calomel; this, however, is owing to subsulphide of mercury, which is of a bright green colour, and may be obtained from those stools by levigation. Calomel relieves some hepatic complaints, but it is by its purgative action, and this may be equally obtained from any other pur-

gative. Dr. Inman, indeed, calculates, from experiments by Kolliker and Scott (Beale's Archives of Medicine), that the chances are six to one that calomel will diminish the hepatic secretion. (Dr. Thudicum, p. 58.)

Prolapse of the Rectum.—In most cases, at first, simply the mucous membrane is protruded, but subsequently, if the case grow worse, the muscular coat likewise is protruded. In some cases laxity of the sphincter is the chief cause of the mischief. In the treatment of this affection, when the disease has become very extensive, and it is probable that the muscular coat is protruded as well, and particularly when associated likewise with hemorrhoidal tumours, any operation short of the ligature will be useless in removing the disorder. In the cases so frequently met with in old people, where the prolapse is very voluminous, and the sphincter relaxed, an excellent plan of treatment is, first of all, to apply strong nitric acid on one or more occasions; and, when this agent has had some decided effect, to remove with curved scissors narrow strips of skin and mucous membrane from around the verge of the anus, at right angles to the orifice. These two operations have never been used conjointly in the same case before. The best case for nitric acid, is where the parts are vascular and readily bleed. Where the mucous membrane is thickened it is not of so much use. (Mr. H. Smith, p. 136.)

Salivary Fistula.—Pass a probe, armed with a few threads of silk, from the external opening into the mouth; secure the threads *in situ* for a few days, until a channel is established for the saliva into the mouth. The edges of the external wound must next be pared, brought together and maintained so by two points of twisted suture, using very fine needles. The wound will heal in a few days. (Mr. R. G. H. Butcher, p. 140.)

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## AFFECTIONS OF THE BONES AND JOINTS, &c.

Amputation of the Knee-Joint.—In amputating at the knee-joint (the flap having been made from the calf), it is better in all cases to remove the cartilaginous surface of the femur, as unpleasant symptoms sometimes result from its being attacked by ulceration, and sometimes by death. The best way of removing the cartilage is, to lay the fine blade of a butcher's saw on the healthy osseous tissue, close to the cartilage, but not infringing upon it, and then cut the bone in a curved manner from before backwards. This form of the bone, free from irritating edges, is better in every way for adaptation to the soft parts. (Mr. R. G. H. Butcher, p. 101.)

Contraction of the Knee-Joint.—In what cases can forcible flexion and extension be used? When the contraction of the joint is owing to disease which has existed within the joint itself, we can do little more than endeavour to put the limb into a more favorable position. Where, however, the contraction is owing to thickening *outside* the synovial membrane, we may use forcible flexion and extension with much prospect of restoring the motions of the joint. When inflammation has existed within the joint, the synovial cavity is generally destroyed, and motion cannot be restored except to a slight degree, and the employment of much force is injurious. (Mr. H. Coote, Mr. Brodhurst, p. 113.)

Dislocations.—*Instrument for Reduction of.*—It is a great desideratum to reduce dislocations by some means by which the extending force can be suddenly slackened, so as to admit of the muscles asserting their influence to rectify the displacement. A contrivance by which this can be effected will be found described at p. 123. It was invented by a student at the Aberdeen Infirmary. (Dr. Keith.)

Excision of the Knee-Joint.—To be successful in this important operation observe the following directions. 1. Select the cases judiciously, the bones not being diseased far beyond their articular extremities. 2. The H incision should be preferred, the perpendicular strokes being placed well back, so as to allow all fluids and discharges to drain well away. All portions of diseased synovial membrane must be removed. 3. The patella must

be removed, whether diseased or not. 4. All bleeding vessels must be carefully secured. 5. While the patient is yet on the operating table, the limb should be placed in the horizontal position by gentle and steady extension. 6. Great caution must be exercised that the surfaces of the bones may be in contact throughout their extent. 7. The limb should not be disturbed for several days after the operation. 8. In cases where large abscesses form in the vicinity of the excised joint, Chassaignac's drainage tubes may be used with the best hopes of success. 9. Stimulants and sedatives must be freely administered, regulated of course by age, sex, temperament, and habits. (Mr. R. G. H. Butcher, p. 104.)

Immediately the operation is concluded the limb must be forced into the horizontal position, and put up before the patient is removed from the operating table. It must be put up in a solid case, and so retained immovably. When the knee has been contracted for a length of time, it is better to divide the hamstring muscles in preference to shortening the bone, to admit of coaptation, which is difficult in such cases. (Mr. R. G. H. Butcher, p. 102.)

KNEE-JOINT.—*A peculiar disease of.*—There is a peculiar disease of the knee-joint characterised by the following symptoms. There is acute tenderness of the skin, and great aggravation of pain on the least motion, even of the bed on which the patient lies. The joint is swollen, and the swelling not only extends around the joint but around the lower part of the thigh and upper part of the leg; it pits on presure and a sensation of crackling is felt beneath the finger. The skin is of a pale white glossy appearance. The disease is an inflammation originating in the areolar and fibrous tissues external to the joint, and, Mr. Hawkins believes, in the periosteum covering the lower end of the femur. If the disease is supposed to be gout or common synovial rheumatism, and is treated by colchicum and afterwards hydriodate of potash, the patient will suffer for months from the violence of the pain, and the joint may become disorganized. The best and only successful treatment is rapid mercurialization of the system and local depletion. This is most successful when the disease is early recognized, but even at a more advanced period it will often arrest further mischief. A lotion of cyanide of potassium (4 grains to the ounce), applied tepid, is a powerful local sedative application. (Dr. F. J. Lynch, p. 118.)

LATERAL CURVATURE OF THE SPINE.—It is a common mistake to suppose that, in incipient curvature, the muscles may by proper exercise rectify the impending deformity; whereas, muscular exercise can no more cure a curved spine than it can rectify a knock-knee. Muscular exercise is healthful and useful as an auxiliary, but spinal curvature can only be corrected by mechanical support, and direct pressure. This pressure must be unremitting, and maintained at the proper standard for months or years. The best apparatus is that known as Tamplin's; it is weighty and strong. The pressure is exerted by pads attached to steel rods, moved with cog wheels. Slighter cases may be cured by the use of stays with lateral supports, or side crutches. These apparatus may be procured at Ferguson's, close to St. Bartholomew's Hospital. (Mr. H. Coote, p. 121.)

OSSEOUS ANCHYLOSIS OF THE ELBOW IN A FAULTY POSITION.—Forcible flexion is out of the question where the union is bony. The best plan is to saw through the lower end of the humerus above the elbow, bend the arm, and a false joint or anchylosis in an improved position is obtained. (Mr. H. Lee, p. 123.)

SCROFULOUS DISEASE OF THE JOINTS.—Almost every case is curable, unless very extensive disorganization has taken place, if the following treatment can be followed. The patient must reside in the country, in a dry bracing atmosphere, with much exposure in the open air to the light of the sun. There must be a free circulation of the atmosphere around the patient's bed at night. Animal food must be taken daily, also cream and cod-liver oil, and some of the preparations of iron. A healthy condition of the skin must be maintained. These means, along with cheerfulness, mental occupation, encouragement, and suitable local treatment, lead even in most unpromising cases to the best results that can be desired. (Mr. W. Perrie, p. 112.)

## AFFECTIONS OF THE URINARY ORGANS.

ALBUMINURIA.—In all cases of acute albuminuria it is advisable to avoid giving elaterium and other drastic purgatives; all stimulating diuretics are contraindicated, the only diuretic admissible being the liquor ammoniæ acetatis. Slight venesection, and warm or vapour baths frequently repeated, are the remedies of most utility. After the acute stage has passed off, and all feverish symptoms have subsided, *tonic* treatment must be adopted, and perhaps no tonic equals the tincture of sesquichloride of iron. When the disease is produced by cold, depletion will be better borne than when produced by scarlatina. When from alcoholic poisoning, general depletion will rarely if ever be called for, but local depletion may be freely resorted to. (Dr. S. J. Goodfellow, p. 72.)

LITHOTOMY.—Mr. Wood, of King's College, uses a staff (combining the uses of a staff, and of a dilator of the prostate,) the curve of which divides into an anterior and posterior blade. The membranous portion of the urethra being opened by a lunated incision, commencing a little to the right of the raphé, the finger, passed between the blades, dilates the prostate, and enters the bladder. The posterior blade, by means of a lever in the handle, can be made to press downwards, so as to hold the bladder firmly, and prevent its being carried before the finger. A woodcut and a fully detailed account of this instrument are given at p. 141.

STRICTURE OF THE URETHRA.—*Potassa Fusa.*—The difference between the local effects of potassa fusa and lunar caustic is, that the lunar caustic gives rise to a hard slough, and to the effusion of lymph around it; while the potassa fusa effects a softening and dissolution of the thickened tissue, without producing any surrounding hardness. Hence the value of the latter in stricture of the urethra. (Mr. C. De Morgan, p. 149.)

An instrument has been invented in France, by means of which a stricture may be cut, dilated, and cauterised. No other instrument is required. It consists of a flat metallic tube, seven inches long, to the end of which there is attached a grooved director, forming a slight curve; along the channel of this steel rods are protruded. A woodcut and description will be found at p. 152.

SUGAR IN URINE.—The copper tests are, as is well known, by far the most delicate. The solution known as Barreswil's liquid, forms the most delicate and ready of these tests (a formula for its preparation may be found at p. 80); but for the use of those practitioners who only require it occasionally, the following will do almost as well, and is very easily prepared:—Take five grains of sulphate of copper, ten grains of tartrate of potash, and dissolve in two drachms of liquor potassæ (previously made stronger by the addition of a little extra solid caustic potash—half a drachm will do), by adding this the liquid will keep in an efficient state for a considerable length of time. By the fermentation test, it is impossible to analyse the amount of sugar quantitatively, but by the copper solution this may be effected easily and accurately. (Dr. F. W. Pavy, p. 79.)

URÆMIC NARCOTISM.—*Bloodletting.*—In the treatment of uræmic narcotism coming on suddenly, if the patient is not debilitated by previous disease, nor overloaded with fat, bleed him freely, and at once. The congestion of the kidney and other organs will be removed, besides removing a portion of the poison. The relief is sometimes so instantaneous, that a patient who has lain some time completely unconscious, will be able shortly to converse and transact business. (Dr. Richardson, p. 76.)

URINARY ABSCESS.—Supposing a urinary abscess to point in the perinæum, after laying it freely open, do not interfere further. Theoretically, the usual practice of passing a catheter into the bladder seems to be the most appropriate, but practically it will be found that the presence of an instrument is positively mischievous. (Mr. Le Gros Clark, p. 146.)

## AFFECTIONS OF THE SKIN, &c.

ABSCESSES, SINUSES, AND ULCERS.—*Effect of Rest.*—There are some cases of abscess and sinus which will not heal, do what you will, unless the parts be kept in a state of perfect rest, and this rest is impossible owing to the constant motion of adjacent muscles, or fascia into which muscles are inserted. In some of these cases the muscle or fascia requires division. In abscess under the scalp, the occipito-frontalis may be kept quiet by strapping the head with long strips of plaster. Similar cases of abscess under the platysma myoides occur frequently, the result of strumous glands; these may be cured by precisely similar treatment, *i. e.*, when they do not heal, as they often do not, from constant action of the platysma myoides. Small facial abscesses may be cured by applying collodion externally, keeping the aperture in the skin open at the same time; the collodion causes pressure and complete rest to the parts. Many ulcers are exquisitely painful, and irritable. This pain will usually be found to exist, especially in one particular point—here a nerve fibril is probably exposed; by dividing a small portion of tissue between this painful spot and the spinal column and close to the spot, the nerve fibrils will be divided, the pain cease, and the ulcer heal. Any granulations, particularly sensitive, may be cut out with a pair of scissors. (Mr. J. Hilton, p. 173.)

*How to Open Deep Abscesses.*—If you plunge the knife into a deep abscess you may cause very dangerous, if not in some cases, fatal hemorrhage. If you wait until the abscess comes nearer to the surface, the patient may die in the meantime. The best plan is to divide the skin and subjacent fascia with a lancet, and then push a grooved probe into the swelling; when the probe has penetrated the abscess, a little pus will show itself in the groove. Now, forcibly push some blunt instrument, as a pair of dressing forceps, along the groove, into the abscess; by withdrawing the forceps with the blades rather open, a lacerated track or canal is left communicating with the abscess. This will not readily heal. For many years Mr. Hilton has not opened a deep abscess in any other way. (Mr. J. Hilton, p. 172.)

ALOPECIA AREATA.—(Syn. *Tinea Decalvans.*)—This disease depends upon the presence of the parasite called *Microsporon Audouini.* It produces patches of baldness, the skin being smooth and very white. First, carefully extract all the hairs within a couple of lines of the patch to prevent the disease spreading, and if any downy hairs are present on the patch they must also be removed. A lotion of bichloride of mercury is one of the best parasiticides which can be used. The growth of the hair may be promoted by the use of gentle vesicants, after the cause of the disease has been removed. (Dr. T. McCall Anderson, p. 167.)

ECZEMA.—Most, if not all, internal remedies are useless in this disease. Arsenic is the only remedy which exerts any influence, and this is only seen in a few cases; it is not worth while trying it except in very obstinate cases. When the eczema arises from the friction of two cutaneous surfaces, the best application is a mixture of starch and oxide of zinc (starch ℥ i, zinc ℥ iij). In acute eczema, or in chronic, when there is but slight infiltration, and the disease prevails over only a limited extent, the following are good applications; oxide of zinc, ℥ j to ℥ j of lard; sulphate or acetate of zinc or alum, ℥ j to lbj of water; red or white precipitate, 6 to 12 grains to ℥ ij of lard. When there is considerable infiltration the best application is the sapo viridis, or green soap (a kind of caustic soap used in Germany). This must be thoroughly rubbed in, and then the surface washed and cold applications be laid on until next rubbing. In many chronic and obstinate cases the only application which suffices, is a solution of caustic potash (℥ j to ℥ ij of water.) One or two rubbings with this will generally suffice. A mixture of equal parts of tar and cod-liver oil is an excellent application in many cases, but is not applicable in the early stages of the disease. Cod-liver oil alone sometimes acts well when the skin is dry and scaly. (Professor Hebra, p. 165.)

FAVUS.—There is an intimate connexion between the strumous habit and the development of favus, hence, in the treatment of this disorder (which is almost confined to the poor and destitute), the adoption of all means calculated to improve the general health is indispensable. Regular exercise, good diet, and the use of cod-liver oil are therefor required. After the scales have been removed, the local application of cod-liver oil, as recommended by Bennett, for the purpose of destroying the favus, by excluding it from the air, is attended with very satisfactory results. (Dr. W. Pirrie, jun., p. 161.)

INGROWING TOE-NAIL.—Drop a little hot melted tallow upon the part, and the pain will be at once relieved. (Dr. Clarkson, p. 179.)

PARASITIC DISEASE OF THE SKIN.—It is almost absolutely necessary for the cure of these affections to remove the hairs by a pair of blunt-pointed forceps, since the parasite is contained abundantly amongst the longitudinal fibres of the hairs, and also that applications may reach the bottoms of the hair sacs. One of the best and most generally used parasiticide applications is a solution of bichloride of mercury (two grains to the ounce of water). Sulphur ointment is more suitable for animal parasites. The mercurial lotion should be rubbed in directly each depilation is completed. In cases of *herpes circinatus* (ringworm of the body), the application of a parasiticide is sufficient, and when the patches cover different parts of the body at the same time, it is best treated by sulphur or mercurial vapour baths. Herpes circinatus, herpes or tinea tonsurans, and sycosis, are only varieties of the same disease. In the two latter depilation is required. Whatever parasiticide is used should be applied immediately the hairs are removed, as the hair sacs are then open. (Dr. T. McCall Anderson, p. 154.)

PRURIGO.—This disease occurs only in poor people, who pay little attention to the state of their skin. Any measures which will soften the skin, will remove the papules and itchiness. The best treatment is warm baths, with plenty of soap, and, after each bath, rubbing into the skin an ointment composed of equal parts of cod-liver oil and tar. The itching almost invariably disappears after the third application. (Prof. Hebra, p. 165.)

SCROFULOUS SCARS.—If unsightly puckered scars in the neck be destroyed by potassa fusa, we find on the falling off of the eschar, that a smooth even surface is substituted for the previous seamed and corded one. In the course of time, this new tissue regains so natural an appearance as to escape notice. (Mr. Hoffman, p. 151.)

SINUSES.—In cases of subcutaneous sinus, pass a probe along the canal for the distance of an inch, or an inch and a-half, cut down upon its point so as to destroy the continuity of the canal at this spot. Then introduce the probe at the point of incision, and passing it another inch, again cut open the canal, repeating the process until the whole length of the canal is so treated. Pressure must be carefully applied for five or six days, when adhesive inflammation will have obliterated the fistulous track. (Mr. W. Jessop, p. 178.)

[See also "*Abscesses. Sinuses, and Ulcers.*"]

TINEA FAVOSA.—*Carbonate of Copper.*—Carefully clean the head by means of unctuous cataplasms, and, when the crusts have been completely detached, shave it. Then apply an ointment consisting of 2½ drachms of carbonate of copper to 15 ounces of purified lard. This ointment cures porrigo with much rapidity. (M. Huet, p. 165.)

URTICARIA.—Apply locally a mixture of equal parts of liq. plumbi and glycerine. It relieves the painful itching caused by the disease very remarkably. (Dr. Frazer, p. 168.)

WOUNDS.—*Disinfectant.*—A mixture of powdered chlorate of potash and glycerine in the proportion of 2½ drachms of the former to 3 ounces of the latter, possesses a remarkable disinfectant power, when applied to suppurating unhealthy wounds. It forms a very good application to sores of every description. (M. Martinet, p. 277.)

## AFFECTIONS OF THE EYE AND EAR.

ARTIFICIAL PUPIL.—*Iriddesis.*—The operation of iriddesis is performed in the following manner. 1. Chloroform is given, the patient being placed in a convenient position. 2. The wire speculum is applied so as to freely expose the eye. 3. A puncture is made, by means of a moderately broad needle, at the junction of the cornea and sclerotic. 4. A loop of fine floss silk, moistened, is laid on the eye over the opening in the cornea. 5. The iris is seized almost midway between the ciliary and pupillary margins by the forceps canula, introduced into the anterior chamber through the puncture in the cornea, and drawn out through the corneal puncture to the requisite extent. 6. The loop of silk is tightened so as to strangulate the portion of iris which has been drawn out. The ends are then cut off, and the operation is completed. The ends must not be cut off too short, or the knot may be slightly drawn within the wound. This operation is applicable to nearly every variety of case in which an artificial pupil is required. It enables the operator to regulate with accuracy the exact size and position of the pupil he proposes to make, and it secures stability to the artificial pupil, so that it will continue of the size and form it had when first made. The operation is particularly applicable in cases of *central leucoma*, corresponding in size and position to the natural pupil; also in cases of *opaque capsule, with adherent pupil*, in which it is desirable to enlarge the pupil slightly in one direction, so as to expose a clear part of the lens. (Mr. G. Critchett, p. 192.)

CATARACT.—The German oculists have now almost completely abandoned reclination, as, even in successful cases, it is found that after a few years blindness returns, owing to choroiditis arising in the situation in which the lens had been placed. The results which are obtained by the ordinary extraction by a flap formed upwards for the hard cataract of old people, and by the linear excision for the soft cataract of young persons, are so satisfactory, that other modes of operation are scarcely necessary. (Correspondent of Medical Times and Gazette, p. 181.)

*Congenital Cataract.—Linear Extraction.*—Mr. Bowman's method of operating is as follows: Completely dilate the pupil by means of a strong solution of atropine, and, by repeated small strokes of a needle, introduced through the cornea, break up the lens thoroughly in every direction, taking care not to wound the posterior capsule, or enter the vitreous chamber. The lens by this operation becomes thoroughly softened and separated from the capsule. After four or five days, make a linear opening in the margin of the cornea, and with a small spoon or scoop gradually remove the lenticular matter, leaving a clear pupil. Atropine must be freely employed in all the stages of the operation. In cases where time is no object, a more perfect result may be obtained, and inflammation may be more completely avoided, by the older method of solution. (Mr. W. Bowman, Mr. G. Critchett, p. 196.)

CONICAL CORNEA.—There are two indications in the treatment of cases of conical cornea, first, to check the softening process; and second, to diminish the intra-ocular pressure. The former we can only pursue by measures of a tonic kind, the latter Von Graefe has lately proposed to effect by the operation of iridectomy, as employed in cases of glaucoma. The effects of conical cornea may, however, be remedied partially by converting the pupil into a narrow slit. (It is well known that an eye thus affected will see much better through a diaphragm perforated by a small hole, or a narrow slit.) The cornea must be punctured with a broad needle, close to its margin, and a short blunt hook introduced, and the pupillary margin of the iris seized, drawn out, and tied by a loop of floss silk placed on the eye in readiness, exactly as in Mr. Critchett's operation of iriddesis (described fully in this volume). If this does not effect the desired end in about a week, the opposite side of the pupillary margin must be treated in the same manner. Diagrams showing the states of dilatation and contraction in the case of horizontal slit, of vertical slit, and of the balloon-shaped pupil from iriddesis downwards, will be found at page 300. (Mr. W. Bowman, page 199.)

GLAUCOMA.—*Iridectomy.*—The operation of iridectomy is of no benefit whatever in cases of chronic glaucoma, but, in the acute affection, it is of very great use. In the latter case we must remember that the disease is one which will not wait; if the proper time is not seized it passes by for ever. (Mr. Dixon, p. 184.)

Some surgeons consider that the smaller the quantity of iris removed the better. This is a mistake; such proceeding is rarely attended with striking success. The segment of the iris must be rather large, and it must be removed completely up to the ciliary margin of the iris. (Correspondent of Med. Times and Gazette, p. 181.)

*Sclæroto-Corneal Section.*—Puncture the sclerotic coat about one-eighth of an inch behind its junction with the cornea with a small cataract knife, cut forwards to about the same extent through the cornea, making altogether an incision about one-third of an inch long, and dividing the outer margin of the iris. A portion of the iris will prolapse, and may be removed; this is rather advantageous than otherwise. The same operation will be of much benefit in rapidly increasing conical cornea. (Mr. T. Nunneley, p. 189.)

IRIDECTOMY.—*New Operation for.*—Mr. Greenway, of Plymouth, has invented an instrument by means of which an aperture can be punched in the iris, so as, if necessary, to form an artificial pupil in the natural position, and of a circular form. The instrument has the appearance of a syringe, "but, instead of being furnished with an ordinary nozzle, a canula is screwed on to the end of the cylinder, or body, and, there being no piston, the rod, which would otherwise be the piston-rod, is continuous with the blade which lies within the canula. This rod passes through an air-tight fitting at the upper part of the cylinder, and has a spring action. The canula is plano-convex transversely, its bore being one-twelfth of an inch by one-fifth; the plane surface presents near the extremity a circular aperture, about one-eighth of an inch in diameter, for the admission of a portion of the iris; that small portion of the tube beyond this aperture is filled with lead, which serves, not only as a plug to completely obstruct the extremity, but as a point of resistance for the blade. On the convex surface is a small mark which indicates the position of the aperture on the plane surface. The blade is kept in close contact with the floor or plane surface of the canula by means of a spring, which is interposed between it and the roof or concavity of the canula. Communicating with the upper part of the cylinder is a metal tube, about an inch in length, on which is fixed an India-rubber tube, which is furnished with a mouth-piece at its free end. This may be termed the suction-tube." By means of suction through this tube the iris is seized, and pressure being made on the head of the rod, a circular portion is immediately excised. The instrument, of which an engraving is given, may be procured of Weiss; it is a very ingenious contrivance. (Mr. H. Greenway, p. 182.)

INJURY TO THE EYE BY MOLTEN METAL.—It seems from a case related, that molten metal may be suddenly splashed into the eye, and still very little injury result, the sudden conversion of water into steam protecting the eyes from injury. (Mr. Le Gros Clark, p. 208.)

RESTORATION OF THE PUNCTUM BY A NEW OPERATION.—In cases where either punctum is completely lost, and its restoration is desired, a much more certain plan than any previously followed, is, to pass one of Bowman's canaliculus styles from the pervious punctum into the sac, and out along the lower canaliculus, using a knife to free the point of the style on emerging, or not, as necessary. This proceeding is easier than would be imagined from the acute angle the upper canaliculus makes with the lower at the sac but the whole course lies within soft parts. The only difficulty arises from spasmodic action of the orbicularis and tensor tarsi, which however may be readily overcome by chloroform. The style used must be slightly bent at the extremity. (Mr. J. F. Streatfeild, p. 206.)

*TRICHIASIS.*—To completely eradicate an inverted eye-lash, or bundle of *them*, plunge the point of a needle or fine knife, dipped in caustic potash allowed to deliquesce, into the tarsal margin along the course of the hair

hairs. to the depth of the eighth of an inch. On the second or third day remove the lashes so treated with the forceps; they are pulled out without any difficulty and are blackened at the roots. The bulbs of ciliæ are completely destroyed, never to be reproduced. No inflammation of any moment follows the operation, unless a whole row of ciliæ be removed at a time. (Dr. J. Williams, p. 205.)

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## MIDWIFERY, AND THE DISEASES OF WOMEN, &c.

AMENORRHŒA.—*Neuralgia.*—Pains under the left breast, in the intercostal spaces, or in the temples, are very frequent and distressing symptoms occuring in cases of amenorrhœa. They are most quickly relieved by anodynes, but generally only temporarily. The use of small doses of arsenic (two drops of Fowler's solution thrice a day) will be of more ultimate benefit, although not so immediately effectual. When there is any indication of a rheumatic taint of constitution, very satisfactory results will be obtained by the administration of thirty or forty drops of the tincture of *actea racemosa*, or black snake-root (an American plant), three or four times a day, in a little water. The most rapid relief can be given by the subcutaneous injection of morphia. In *chronic and obstinate neuralgic headache* a very effectual remedy in my hands has been from half a grain to a grain of sulphate or phosphate of nickel. (Prof. Simpson, p. 257.)

APPLICATION OF THE FORCEPS.—The aphorisms of Denman so frequently taught in our medical schools and followed by practitioners, that the forceps must never be applied until the head has rested on the perineum for six hours, or except an ear of the fœtus can be felt, are both bad. There are many cases where such delay would be highly injurious, and it is unnecessary to feel the ear of the fœtus. (Dr. A. B. Granville, p. 307.)

ARTIFICIAL DILATATION OF THE OS UTERI.—When the os is rigid and contracted in the first stage of labour, and turned towards the rectum, instead of downwards and forwards, first influence the patient gently with chloroform in the following manner: Pour a little chloroform into a tea-cup and hold the edge of the cup to the lower lip of the patient, bringing the open vessel a little over the mouth, but not so as to prevent the free ingress of air both into the mouth and nose; the nurse can manage this whilst you watch the effects. Now introduce the forefinger into the rigid os uteri, and, while the patient is under the influence of the chloroform, gently dilate the part by very quietly, during each pain, pulling the anterior lip towards the pubes. You may keep the patient gently influenced by the chloroform till the os has lost all its rigidity and has become dilated. Now leave off the chloroform till the tough perineum has to be dilated, when you may use it a little again. You will thus save hours of misery and do good to your patient. If the chloroform act too much, compound spirit of ammonia either taken or breathed is an almost instantaneous antidote. (Dr. Braithwaite, p. 210.

CÆSAREAN SECTION.—If the operation be done with resolution and rapidity, no great alarm need result if we find the placenta attached to the anterior part of the uterus. A sudden gush of blood will follow the incision into it, but the operation must be nevertheless proceeded with, and the placenta be extracted first, as rapidly as is consistent with doing it well. (Dr. J. Edmonds, p. 219.)

CANCER OF THE BREAST.—Sulphate of zinc is one of the best tonics which can be given, sometimes the pain is alleviated, and frequent improvement of the general health follows its prolonged use. (Mr. H. George, p. 260.)

CATAMENIAL ACNE.—In some persons a crop of acne appears if the proper menstrual flow is at all checked. In many, this eruption disappears if the flow be restored, but in some it still remains persistent. If, in these cases, you apply the oil or butter of antimony with a brush, very thinly, and neutralize it quickly with the bicarbonate of soda, the eruption will frequently disappear very speedily; and thus used, the preparation does not cause

any pain, nor does it exercise any caustic action. Citrine and mercurial ointments are also very good applications. If one part of the ordinary tincture of iodine be mixed with two parts of the milder aquæ ammoniæ, and allowed to stand forty or fifty hours, the mixture, which is at first brown, becomes quite clear and colourless. As it possesses all the activity of the tincture of iodine, and is yet colourless, it forms one of the best preparations for external application. It may be used with much advantage in cases of acne. (Prof. Simpson, p. 258.)

Chronic Mammary Abscess.—These cases are sometimes very difficult to cure. It has been recommended to pass a seton across them in a perpendicular direction; but milder means may be employed with success. Apply Scott's ointment to the breast, spread on lint, then place tightly over this strips of plaster an inch and a half in breadth, and bandage the whole carefully. (Mr. W. Coulson, p. 255.)

Deficient Involution of the Uterus after Labour.—*Bromide of Potassium.*—Sometimes the uterus does not undergo the normal retrograde metamorphosis after labour, and remains large and heavy, the fibre cells being loaded with fatty particles. The uterus is felt large, soft, and heavy, and there is a feeling of weight and discomfort in the pelvis. In many cases, the abstraction of a small amount of blood from the vaginal portion of the uterus, or the perineum, is called for, and is more especially beneficial in those cases where there lingers any degree of congestion or of inflammatory action in the uterus. The most efficacious of absorbent remedies are the iodide and bromide of potassium. The latter is preferable, however, as it does not produce that marasmus so often caused by prolonged use of the former. The bromide, moreover, acts as a special sedative on the reproductive organs. The dose should be about six grains, three times a day, and it may be continued with iron or other tonics, if anæmia or atony co-exist. Good diet and other hygienic means must be employed. (Prof. Simpson, p. 224.)

Fibrous Tumour of Uterus.—Intra-uterine fibrous tumours may be destroyed by gouging out a portion of their tissue. The vitality of the growth is so low, that the remaining portion gradually disintegrates, and comes away as discharge. As a preliminary step, it is better to incise the os and cervix uteri. The best instruments to use are those of Mr. Harper, for which see wood-cuts and description at p. 245. (Mr. I. B. Brown, p. 242.)

Inflation of the Lungs of Infants.—*New Instrument.*—Dr. Wilson, of Glasgow, has invented an instrument for the purpose of inflating the lungs of infants born in an asphyxiated state. It consists of a small india-rubber ball, to which is attached a slightly-curved german-silver tube about six inches long, and having two openings a short distance from the point, and a larger one almost an inch from the attachment of the tube to the ball. The tube is introduced into the larynx, and the larger opening being closed by the finger, the ball is compressed and air forced into the larynx; the finger must be removed during the expansion of the ball that fresh air may enter by the larger aperture. The insufflation of the lungs must be gently and slowly performed, and the chest may be slightly compressed after each inflation. If there is much lividity, allow a drachm or two of blood to escape from the divided vessels of the cord. The instrument may be obtained from Mr. Hilliard, surgical instrument maker, Glasgow. (Dr. J. G. Wilson, p. 261.)

Ovarian Tumours.—After tapping, apply constant and regular pressure, this much retards the refilling of the cyst. This treatment is only applicable in the monocystic forms of the disease. (Mr. I. B. Brown, p. 247.)

Ovariotomy.—If you think ovariotomy advisable in a case, do not delay it, for after the abdomen has been disturbed by repeated tappings, and the constitution drained by the constant refilling of the cyst, the mortality is very much increased. (Mr. I. B. Brown, p. 248.)

*When should* the clamp be used, and when the ligature, in securing the *pedicle?* This depends entirely upon the length of the pedicle; if it is *pretty long* the clamp is much preferable. If short, we are obliged to use

the ligature, letting the stump of the pedicle return within the abdomen; when this is done the mortality is much greater. (Mr. I. B. Brown, p. 247.)

Sixteen per cent. of the fatal cases die from bleeding from the pedicle. A most excellent plan, and one which renders the use of the clamp unnecessary, is to cut through the bottom portion of the tumour, instead of the pedicle, leaving a portion about the size of a small band, attached; this must be retained outside the abdomen. All risk of serious hemorrhage is avoided. (Dr. Tanner, p. 249.)

The metallic sutures or hare-lip pins, used to close the abdominal wound, must be passed through the peritoneal edges, as well as through the integuments and muscle; otherwise the peritoneum will retract, and a raw surface of considerable breadth will be left exposed to the general peritoneal cavity. (Mr. T. S. Wells, p. 250.)

Besides the precaution of fastening the pedicle externally, and making the smallest external incision compatible with the passage of the tumour, other essentials for success are:—1. To pass silver pins so deeply through the abdominal parietes so as to include the peritoneum. Union by the first intention of the deep parts of the wound is thus secured, and the accidental entrance of any pus into the abdominal cavity is prevented. 2. Nutriment and stimulants must be very freely administered per rectum, when the patient is unwilling or unable to swallow them; they are capable of absorption into the system to a most remarkable extent. (Dr. L. Roberts, p. 250.)

Placenta.—*Removal of, after Labour.*—It is usual to wait for a pain or to be able to feel the insertion of the cord into the placenta, before attempting to remove it. But pains may mislead, and may arise from other causes than contractions of the uterus, and the feeling for the insertion of the cord is a very questionable proof that the placenta has become detached from the uterus. The great objection, however, is, that frequent and painful examinations are sometimes, and indeed often, necessary. The cord after the birth of the child will be found in a state of flaccidity; in a short time, however, it again becomes turgid with blood, which state again passes into one of flaccidity; and this occurs so regularly that this flaccid state following the turgidity constitutes a sign of detachment, which may almost invariably be relied on. (Mr. J. Clay, p. 223.)

Polypus of the Rectum in Children.—Our attention is again called to this affection by a case occurring at Guy's Hospital. The polypus had been overlooked completely by several medical men, who evidently had not read the able article written on the subject by Mr. Bryant last year (*Retrospect*, vol. xli., p. 255.) This affection is most likely to be mistaken for piles, or simple prolapsus of the rectum. There is always hemorrhage from the bowel. The polypus may be broken off by traction, or removed by ligature. (Mr. T. Bryant, p. 263.)

Position in Labour.—In cases of lingering and protracted labour, it is often a good plan to let the woman sit upright upon two chairs fastened to each other *in front* by tape or other strong material, and the *backs* separated one and a-half or two feet, as may be necessary. The patient during a pain can take hold of the bed-post, and the accoucheur can from time to time ascertain the progress of the case. The patient must be removed to bed when the head is nearly born. In all cases in which this proceeding is adopted, the head should be within the pelvic inlet, and the os uteri dilated fully one half; and further, the failure of the pains in the horizontal position must be first evidenced. (Mr. R. Hardy, p. 301.)

Pregnancy.—*Operations during.*—If it becomes necessary to perform any operation on a pregnant woman, do not hesitate to perform it; but if it can be delayed till the labour is over, defer it. Some of the most serious operations, as tracheotomy, operations on varicose veins, and removal of condylomata about the anus and vagina, have been performed without any untoward result whatever. (Mr. Shaw, p. 241.)

Premature Labour.—*Induction of.*—Pass the tip of the forefinger of the left hand within the os uteri, and slightly dilate it, pulling the neck down a little, and passing the finger round a little inside. This process, if not ac-

complished easily at first, may be done more successfully upon a second attempt. Upon each occasion some pains will ensue, and ultimately labour will come on, when an elastic tube may be passed up between the walls of the uterus and the membranes, and a little cold water injected. If any resistance is experienced in introducing it, the tube must be withdrawn and reinserted in another direction. Labour will now proceed naturally. (Mr. H. James, p. 217.)

Prolapsus Ani of Children.—Those surgeons who like to hazard such a mode of treatment, may follow the plan pursued lately in a case of this nature by M. Foucher. This gentleman injected just external to the anus, ten drops of a very weak solution of sulphate of strychnia (twenty centigrammes to twenty grammes of water,) using a Wood's subcutaneous injection syringe. "The cure was immediate." The principle, of course, is the direct action of the salt upon the fibres of the sphincter. (M. Foucher, p. 263.)

Prurigo Pudendum.—In cases of follicular inflammation of the labia, in eczema, and prurigo pudendum, carefully rub over the diseased portions of skin or mucous membrane a piece of cotton-wool soaked in a solution of nitrate of silver. This may be continued two or three minutes at a time, and be repeated every day or two. (Dr. J. Tilt, p. 254.)

Puerperal Insanity.—*Its connexion with Albuminuria.*—It is a curious fact, unknown by the majority of the profession, that in a large proportion of cases of puerperal insanity, albuminuria precedes and attends the first access of the disease. It, however, generally passes off with extreme rapidity, often in two or three days. Hence it has been so rarely found, on examination, that no connexion between albuminuria and puerperal insanity has hitherto been recognised. When the insanity recurs in the form of successive attacks, each attack will be found connected with a fresh appearance of albumen. If the doctrine of Frerichs be right, that the urea retained, as it always is in cases of albuminuria, is not injurious *per se*, but only from its decomposition into carbonate of ammonia, we have some explanation to the phenomenon of one affection (albuminuria with, of course, uræmia) causing two other diverse and separate affections—puerperal convulsions and puerperal insanity; the former being caused when carbonate of ammonia results from the change in the urea, and puerperal insanity when some other unknown alkaloid product results. (Mr. Calvert has lately shown that various unknown alkaloids are formed during animal decomposition.) (Prof. Simpson, p. 235.)

Puerperal Mania.—If, in the onset of a case of puerperal mania, we can induce sleep, the probability is that the patient will awake up tolerably well, or at any rate, the case becomes much more hopeful. The best remedy is opium, but it must be given in doses of two or three grains. If the patient cannot, or will not swallow, you may succeed in introducing the drug in the form of a suppository into the rectum. One or two grains of morphia are required when the drug is administered in this form. In some cases sleep may be procured by means of ether or chloroform, and the patient thus anæthetised has continued to sleep on and has awaked up quite well. (Prof. Simpson, p. 233.)

Retroversion of the Gravid Uterus.—In reducing a retroverted gravid uterus it is very important not to attempt to push directly upward at first, as the promontory of the sacrum is in the way. The womb must be placed as much as possible in the right oblique diameter of the pelvis, by drawing the os and cervix towards the left acetabulum, and moving the fundus as much as possible towards the right sacro-iliac syncondrosis. It is also well before commencing much manipulation, to introduce compressed sponge into the vagina, to dilate that canal for the more ready introduction of the hand, and to exercise gentle and steady pressure on the uterus. (Dr. T. Skinner, p. 213.)

Sore Nipples.—A mixture of equal parts of brandy and glycerine forms an excellent application. (Dr. W. Frazer, p. 260.)

Suppression of the Secretion of Milk.—Moisten a flannel with sat-

urated solution of camphor in glycerine, and apply it over the breast. This will often check the secretion of milk very quickly. (Dr. Harriss, p. 260.)

Vaginitis.—Inject a solution of nitrate of silver (two scruples to the ounce.) This acts equally well whether the vaginitis be the result of uterine catarrh, or occurs spontaneously. It is most conveniently done if a small glass speculum be inserted first; as the speculum is withdrawn the solution follows it and may be received in a cup. (Dr. E. J. Tilt, p. 254.)

Vesico-Vaginal Fistula.—Do not operate in these cases when the woman is suckling. Also avoid operating on a ruptured perineum at this period. The patient is more liable to pyæmia at such times. (Mr. I. B. Brown, p. 240.)

Mr. Buxton Hilliard, surgical instrument maker to the Glasgow Royal Infirmary, has invented a set of instruments which we think will prove extremely useful in the tedious operation for the cure of vesico-vaginal fistula. The speculum is one which, preserving the form of the vaginal, expands so as probably to give a better view of the parts than any speculum previously in use. The edges of the fistula are seized by an instrument called a "fistula clamp," which is so contrived that the edge of the fistula, when seized by it, is elevated above the surrounding parts, and only requires shaving off with a slightly curved knife. When Bozeman's knives are used, there is often much uncertainty whether the whole of the edges have been effectually pared or not; when this instrument is employed there can be no doubt about it. The tubular needle invented by Mr. Price is the best instrument for passing the metallic sutures through the edges of the wound. The ends of the ligature are brought together and twisted by means of another instrument, and an oval metallic plate applied, differing from Bozeman's in having nipples attached to it, instead of loose pellets being employed. Much trouble is thus saved, otherwise caused in passing each pellet separately. These instruments will be better understood by referring to the original article and the engravings at page 238.

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## MISCELLANEA.

Chlorodyne.—Mr. Davenport's secret remedy, chlorodyne, sometimes produces alarming symptoms, such as complete prostration, distressing cerebral fulness; and forty minims produce complete unconsciousness. It has not greater power over neuralgic complaints than morphia, hydrocyanic acid, or chloroform. (Dr. B. Washbourne, p. 275.)

Chloroform.—*Cause of Death from.*—The cause of death, in some cases, is the position of the patient during the exhibition of the chloroform. In the usual prone position with the face upwards, the tongue falls back, closes the epiglottis, and causes asphyxia, as soon as the insensibility is complete. The lateral or sitting postures are the best during the exhibition of anæsthetics. (Dr. J. Petrie, p. 275.)

Hypodermic Inoculation.—A simple mode of introducing a substance hypodermically is to puncture the part quickly two or three dozen times, the lancet being dipped in the substance used, as morphia, made into a thin paste. (M. Lafargue, p. 285.)

New Anæsthetic.—*Turpentine.*—Sprinkle some turpentine on a handkerchief, and let the patient respire the vapour freely. It will be found to allay pain and produce a gentle sleep, and anæsthesia, not followed by any unpleasant symptoms. It was used with good effect in a case of neuralgia in the course of the supraorbital nerve, and in one or two slight but painful operations. (Mr. J. Wilmshurst, p. 274.)

Nitrate of Ammonia as a Tonic.—In some cases of debility and cachexia the nitrate of ammonia is found to act as a tonic. It is supposed to act by introducing an increased amount of oxygen into the system. It may be given in doses of gr. xv., three times a day. (Dr. W. Coles, p. 273.)

Poisoning by Strychnia.—In poisoning by strychnia one principal cause of death is the diminution of temperature. The patient must be kept near a good fire, and well covered with blankets. The nausea produced by the administration of emetics is favourable to the recovery of the patient. Copious draughts of warm water must be given, so that the patient may vomit fully and freely. The action maintains and developes animal heat. The violence of the paroxysms may be much mitigated by chloroform, not however given so as to produce complete insensibility. (Dr. J. Part, p. 267.)

The Lamp Bath.—The following is a very simple and most effectual method of exciting the functions of the skin. Let the patient, in *puris naturalibus*, be seated on a common wooden chair with his feet upon a low stool, the body then enveloped in two or three blankets, the head being excluded, and a large spirit-lamp placed under the seat. In about a quarter of an hour the perspiration streams down the skin. After a time the blankets must be removed, and the patient subjected to a douche of two pailsful of cold water, and then dried with much friction. After which a smart walk may be taken. (Dr. C. Taylor, p. 282.)

# A COMMENTARY ON MIDWIFERY,

AND THE

## DISEASES OF WOMEN AND CHILDREN, FOR THE LAST HALF YEAR.

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The Diseases of Women have given rise, of late, to perhaps more attention than has been devoted to any other special subject, and have drawn fo th papers of the most interesting nature. We have thought it useful, therefore, to make the following Special Report, with such comments as struck us at the time of collecting the information. This report we hope to continue from time to time.

In the management of a Rigid Os Uteri, in the early stages of labour, the greatest care and skill are necessary. We are tempted to dwell on this subject for a few lines on account of the paper of Dr. Charles Arnott, who has mentioned some excellent points, but has omitted others. It often happens that you are called to a case of labour when the pains are severe, but the os is quite undilated, and pointing downwards and *backwards*, while at the same time the head of the child is felt to be pressing forcibly on the anterior portion of the womb, just above the os uteri; in short, the womb is doing its work well enough, but the os uteri is in a wrong direction, and is, as it were, opposing its own dilatation, causing the womb to expend its powers in stretching the anterior parietes, instead of dilating the os. Dr. Arnott practises what most experienced men now do—he carefully draws the os uteri forwards so as to be more downwards and forwards, and so as to be under the influence of the uterine pains; at the same time carefully dilating the os. So far we can agree with him; but when he talks of *notching* the os uteri with a guarded probe-pointed bistoury, although in a *rare minority* of cases, we must be cautious how we approve of such a practice. Certainly Nature herself does sometimes play some odd freaks with the os uteri in such cases, as we have recorded a little further on in the present review; but the cases in which this operation is supposed to be required, must be so rare that we should disapprove of it entirely. When the smallest laceration occurs spontaneously, in such cases the *slit* will almost certainly extend considerably upwards: so long as it remained low enough to include only the parietes of the womb, it would do comparatively little harm, but if it included some of the pelvic fasciæ or peritoneum, or so injured the cellular tissue around it as to cause suppuration within any of these fasciæ, it would entail endless misery on the patient. We would therefore avoid all notching or slitting up of the os uteri in labour, and trust rather to *chloroform* in relaxing these parts in the first stages of labour; maintaining that in chloroform we have an infallible remedy, the employment of which allows the parts to be dilated either by the finger of the practitioner or the pains of the womb. In our notice of Dr. Arnott's paper we have alluded to the way in which we recommend the chloroform to be used (see page 275.)

To the excellent papers of Dr. Tyler Smith and Dr. Skinner on Retroversion of the Womb, we would next direct the attention of our readers. We differ on some points from those high authorities. In our remarks at page 277, we have expressed the opinion that most writers have erred a little on this subject, and frequently called that retroversion of the womb which is nothing more than the gravid uterus in a slight state of prolapsion, and remaining too long in the lower region of the pelvis during the first months of pregnancy, the os pointing *downwards* and *forwards*, and with the

enlarged posterior portion of the fundus still in the hollow of the sacrum. Where the womb is more or less displaced downwards from previous gestations, or from weakness of the parts, how can the fundus be anywhere else in the early months of pregnancy than in the hollow of the sacrum, and more or less pressing on the rectum? This is, as it were, its natural position under the circumstances, and ought not to be called Retroversion or Retroflexion. Neither ought we to be meddling too much with such a case, seeing that the fundus is not prevented from rising above the brim of the pelvis, by being wedged up against the promontory of the sacrum, but simply because it was originally so low in the pelvis that it has not yet, in the usual time, at its usual rate of increase, arrived at this point; but it will, if let alone, soon do so, and meantime, if it be slightly prevented from *clearing the promontory*, it can easily be pushed over this obstruction without all the manipulations so dwelt upon by writers. Take the dried pelvis of a female, and examine its axes in the upright, prone, and supine positions: place this pelvis on the table in its prone position, resting on the promontory of the sacrum and the pubes; now place a good large pear, with its stalk, or a small round soda-water bottle with its neck downwards, and its fundus just *under* the promontory of the sacrum, or rather in the prone position in which you have just placed the dried pelvis, just above the promontory; keeping the neck of the bottle in its position, gently press its fundus with the finger, and observe what a very slight force is required to send it over the promontory, and forwards through the brim of the pelvis, so as to rest on the symphysis pubis. Even so it is easy to reduce the (so called) retroverted womb, by placing the woman *prone* on the edge of the bed, with her legs and thighs on the bed and her head on the floor, but supported by her elbows. As to this being indelicate, such a feeling is not to be considered when both mother and child are in danger. In this position we shall be able with the greatest ease to reduce any retroverted womb, without introducing the [illegible] into rectum or vagina, as is recommended by some of our best writers. In such a [illegible] danger to the woman till the womb arrives at the promontory of the sacrum, it may be arrested during the [illegible] of gestation. [illegible] out of impaction by [illegible] vagina with [illegible] pessaries, and allow [illegible] expand? [illegible] where this kind [illegible] can extend. [illegible] position on the [illegible] ed, as we [illegible] finger, or [illegible] portioned [illegible] ly: the [illegible] will cause

[illegible] different in [illegible] rating the [illegible] when the [illegible] ve in this, [illegible] womb, about [illegible] to an elastic [illegible] good, but [illegible] membranes [illegible], and the [illegible] will [illegible], and are so [illegible] fresh recommendations are [illegible] tubs of gestation, to get [illegible] without such a male cath- [illegible] and accomplish

[illegible] of the cases called Spurious [illegible] that no child can be felt by the [illegible] ask the strongest assertions that she [illegible] warn the medical attendant to value [illegible] finger or ear, and even his ear may deceive

him. Dr. Priestley truly states that, where the distension of the abdomen can be relaxed by chloroform, (as may often be done in these cases of abdominal swelling owing to the muscles of the abdomen) we have a good means of distinguishing them from pregnancy. But sometimes we find that the organic muscles of the intestinal canal, and even in some measure, we have no doubt, the womb itself, are capable of a kind of spontaneous distension, which would not be so amenable to the effect of chloroform. Chloroform, therefore, may deceive us. If the child be of any size, that is, after the fourth month, when the fundus uteri can be felt by the ends of the fingers appearing above the pubes, we can seldom be deceived by proceeding as follows. The general error committed is, in examining the woman not in the prone but in the lateral or supine position. Now, what we want to do, is to bring the fundus uteri as forwards as possible. Therefore place the woman in the prone position, more or less, either standing at the edge of the bed, but leaning with her face and elbows on the bed, or in bed on her knees and elbows with face prone. Introduce the forefinger so as to press on the anterior portion of the cervix uteri, and with the fingers of the other hand compress the fundus downwards. You have now the uterus between your two hands, and if there be a womb with anything in it, you will have no difficulty in distinguishing it. Of course this is only *one* of many other ways of distinguishing the true from spurious pregnancy; but where a woman declares positively that she feels the child, when you cannot feel one in this way, be guarded in your diagnosis, for fear of subsequent ridicule.

The common Cranial Perforator, generally used, is a formidable military-looking instrument, although efficacious enough for its purpose. We want, however, a less ugly looking instrument, and we think that recommended by Dr. Thomas, of New York, will prove to be so. The wood-engraving in article 96 will show the reader what kind of an instrument this is.

The case related by Dr. James Edmunds, at page 219, is instructive. It shows us, First, that in a case of cancerous disease of the os and cervix uteri, where delivery is almost impossible without either destroying the child or slitting up the womb itself, the Cæsarian Section may be safely performed; and Secondly, that even when the placenta is situated in front, and immediately under the incision intended to open the womb, the danger of hemorrhage may be averted by rapidly cutting through the placenta and dragging it out. In the present instance this proceeding stopped the hemorrhage which had commenced violently, and moreover, is an additional proof of the value of Dr. Simpson's practice of separating the placenta in some cases of uterine hemorrhage. We have, however, often been surprised to read of successful cases of delivery where *slitting-up* of the os uteri was obliged to be adopted, and therefore, in such a case as the above, although we admire the judgment and skill of Dr. Edmunds, we must say that, unless some very formidable impediment presented itself, we should have preferred slitting-up the os and cervix and delivering either by embryotomy or turning. In such a case turning would be the most proper proceeding because the most easy, and comparatively safe to both mother and child. No doubt, however, this case was most successful and creditable.

Mr. Clay, of Birmingham, points out an indication by which we can judge of the Separation of the Placenta after the birth of the child, without feeling for the insertion of the cord. When we have ligatured the cord in two places and divided it between the ligatures as usual, so as to separate the child, if we may now examine the maternal portion above the ligature we shall find it flaccid; but if again examined in two or three minutes it will have refilled with blood, and will give a distinct feeling of fluctuation, like a tube filled with fluid; in short, we feel that the vessels have been again filled, although just before they were empty—this shows that the placenta has not yet been separated. But as soon as it does separate, this feeling of fluctuation ceases and the cord remains flaccid. We have not proved this assertion by sufficient experience, and it is probable the symptoms will in some cases be modified; but if proved to be trustworthy it will be a great addition to our means of accurately judging of this circumstance.

Dr. J. Y. Simpson publishes some excellent remarks on those cases of Enlarged Womb which we meet with after parturition and miscarriages. When the womb remains too much enlarged after labour or miscarriage, and its substance does not undergo the usual process of absorption it is now called *Sub-involution*; when it becomes *too much absorbed* or reduced in size, as it does in some cases, it is called *Super-involution*. It is very interesting to watch the absorption of the muscular fibres of the womb. This is not accomplished by the absorption of the muscles primarily, but the muscle is supposed first to undergo fatty degeneration, and is *melted down*, and absorbed as fat. This fatty metamorphosis commences a few days after delivery, and may be seen as a series of glistening particles in the fibres. The womb may be hypertrophied from many causes as well as from pregnancy; for example, from miscarriages, and from any other cause which *sends* or *detains* too great a quantity of blood in the organ, and which blood is *used up* in its nutrition. Thus a most common source of inconvenience in women is an enlarged womb which is using up more blood than is needed for its normal size and functions. From some such simple beginning we have found all kinds of trouble, such as prolapsions, retroversions, *so-called* (but which are often nothing more than heavy wombs falling back on the rectum on account of their occupying the inferior axes of the pelvis, instead of the upper and middle axes), hemorrhoidal congestions, &c. &c. In these cases Dr. Simpson places confidence in the prolonged exhibition of the bromide of potassium, given in doses of 6, 8, or 10 grains three times a day. He thinks we may continue this medicine for a much longer time than the iodide of potassium, and without any inconvenience. This medicine no doubt is valuable in its place, but what we want in these cases of enlarged womb, is *to diminish the quantity of blood* which has been going to the organ for months and years, and has been regularly *used up*, more or less, in the construction of the hypertrophy, the same as in the arm of a blacksmith from the exercise of his business. We may use iodine and apply leeches to the womb or anus as we like, but we maintain that all remedies are inferior to that of placing the woman, for a considerable portion of each day, on a couch with pillows or a hair bolster under her pelvis, so as to elevate it and thereby to encourage the veins to empty themselves as much as possible, the same as in hemorrhoids. This must be continued for weeks and months when we have tumours and other enlargements to contend with, for it is just as likely to be the quantity of blood *detained* in the veins of the womb as the quantity *sent* by the arteries, which causes these inconveniences. We think, therefore, that in all these cases sufficient emphasis has not been laid on *position*. It is a general principle in surgery that veins are best emptied when they can be raised above the level of the heart, as in varicosities of the leg. Now if you place a skeleton horizontally on the table in the supine position, it will rest on the end of the sacrum and on the vertebral column, but the vessels in the hollow of the sacrum are below the vena cava and below the vessels of the thighs; in fact, they represent a kind of hollow incurvation in which the blood from the legs is, as it were, enticed to linger before it is forced up the sides of the pelvis to the cava inferior. In order to remedy this encouragement to congestion, we must *elevate the pelvis* several inches above the vertebral column when the patient is *supine*: but the case is quite reversed when the patient is *prone*. Hence, in many pelvic diseases, we prefer to place the patient as much as possible in the prone position, or, when supine, with the pelvis considerably elevated. We think these suggestions will improve the treatment both of the sub-involution and super-involution cases, so well described by Dr. Simpson. The super-involution cases are just the reverse of the others: here the womb is absorbed too much. This kind of case, however, is comparatively rare.

It is sometimes difficult to account for the sudden way in which women die after labour. This difficulty is attempted to be explained by calling it Puerperal Fever. For our own part, we consider that it is often nothing more than the *inoculation* of the woman by the hand of the practitioner. Hence we have long held the opinion, in which we are supported by some of

sible separated from the practice of Surgery. It is wrong in any one going from some bad surgical case or *post-mortem* examination, in which his fingers have been bedaubed by some highly objectionable matter, immediately to the bedside of a woman, to assist her in labour. We have it authenticated by numerous instances, that when such a thing is done, the greatest danger to the woman will result, and frequently death itself. You may say that in such a case, when the parts are bloody and covered with secretion, that absorption of poisonous matter will not occur—but we know that it does occur. Observe the common process of vaccination: You make a small incision or a scratch in the arm, sometimes it bleeds a good deal, but nevertheless you insert the vaccine matter, which in fact is well mixed with the blood. But what occurs? The vaccine poison is carefully selected by the absorbents from the blood, and a genuine vesicle is formed, notwithstanding the bleeding; the hemorrhage in short makes no difference. In some measure the inoculation of a woman by the finger of the surgeon who has just arrived from some bad surgical case is similar. Puerperal fever is the result, and the woman dies. We maintain, therefore, that obstetric practitioners ought as much as possible to avoid surgical cases. These remarks are confirmed by the late discoveries of Dr. B. W. Richardson, of London, respecting clots of blood and fibrinous deposits in the veins and right side of the heart, which we think will account for many of those rapidly fatal cases called puerperal fever, and which we have not been able satisfactorily to explain. We have hitherto been satisfied by saying "the woman was struck down by a poison." Let us see if we can throw some light on these cases. Take the one related by Mr. Walford. This case indeed occurred a considerable period after labour, but it only illustrates what in other cases occurs earlier. The lining of the uterus was vascular; "some of the uterine veins were blocked up with fibrinous clots, and this state could be traced into the body of the uterus: a coagulum existed in the right uterine vein close to its entrance into the vena cava." "The right side of the heart was filled with clotty blood, and a fibrinous clot extended from the auricle into the ventricle, and in the right ventricle was a layer of fibrin of a rose colour. The tricuspid valve was a little thickened, and one of the larger divisions of the pulmonary artery was completely blocked." In this case we think it easy to trace the mischief from the womb itself. Imagine one of these fibrinous clots in one of the large uterine veins which possess no valves, and which communicate so rapidly with the vena cava; it becomes loosened and detached, and must almost of necessity be washed along by the venous current into the cava and right side of the heart. It is now almost generally acknowledged that these coagula are very liable to follow uterine phlebitis: and, in a woman under these circumstances, the whole blood is poisoned and much more liable to the separation of its fibrin. We are supported in these opinions by the first authorities, and we again urge that the practice of surgery and midwifery ought to be as *much as possible* separated from each other. This we know cannot be done completely, but "to be forewarned is to be forearmed. The discoveries of Dr. Richardson will throw more light on this subject, and enable us to diagnose as the cause of death some sudden and violent interruption to the action of the heart, in consequence of fibrinous clots washed into it from some other region, in some cases probably caused by a poison acting on the veins, as in the uterus; and in other cases, independent of the specific and prostrating influence of poison thus applied. (See Mr. Crisp's paper, p. 49.)

In the treatment of Puerperal Mania, we have frequently great control over the case by attention to the following things. Instead of bleeding and using other depressing agents, when the pulse is strong at the commencement of the case, we can reduce both its power and frequency by judiciously giving the tincture of aconite, or the tincture of veratrum viride. Both these medicines have a powerful effect in reducing the circulation, and must be carefully watched: with care they will reduce the pulse beautifully, but must not be continued longer than necessary. Dr. Simpson calls our attention to these medicines, and also to his practice of giving opiates by the rec-

our best writers, that the practice of Midwifery ought to be as much as postum. He says you will often find it impossible to give an opiate by the mouth, but you can always introduce a suppository of two grains of opium, or one grain of morphia, into the rectum. He relates a case in which he introduced two grains of morphia. Dr. Simpson has drawn our attention frequently to articles of the Materia Medica which are new to us. There seem to be many powerful remedies in the American Materia Medica which we have not used much up to the present time, but which are gradually obtaining notoriety in Great Britain. For example, we have not used the tincture of black snake-root, the *Actea* or *Cimicifuga racemosa*. When the patient has been a good deal pulled down, and is mentally depressed, with a loss of energy and memory, we often give her good diet, wine, tonics, &c., and perhaps an opiate at night. In these cases the tincture of actea will be useful. The Americans use it in acute rheumatic fever, which is often thus cut short by the eighth day. Dr. Voris, of Rochelle, New York, thinks that it is quite an antidote to rheumatism. A strong tincture is made of the root, and doses of 30 or 40 drops are given every few hours. It is also used in chorea and other nervous disorders, but Dr. Simpson has used it with good effect in puerperal hypochondriasis and depression, in doses of fifty drops three times a day. The account which he gives of its effects in one of his cases is highly interesting.

It is now well understood that Puerperal Convulsions are constantly preceded by Albuminuria; so is also Puerperal Mania, but perhaps not so commonly as in the case of convulsions. Dr. Simpson remarks that, in puerperal insanity, if we examine the urine before the attack, we shall probably find albumen. Soon after the attack of insanity the albumen often disappears. "The fire of disease goes on burning in these cases after the lighted match is merely applied, and the strange morbid clockwork runs on, as it were, after the key that wound it up is withdrawn." It would be a useful rule, therefore, in all suspicious cases of pregnancy to examine early for albumen. How many catastrophes might we not thus avert, and what useful warnings might we not give both to mothers and friends. It is singular that these cases more frequently happen in first pregnancies. When the nervous system is thus poisoned by the decomposition of urea, whether in the form of carbonate of ammonia or not, it is like a Leyden jar highly charged with electricity, and ready to go off on the first opportunity; especially on receiving any sudden shock, from the officious kindness of friends, such as evil, or even good, news. Hence, when we know that in any given case albumen is in the urine, we can offer timely warning to our patient and her friends. This practice is too much neglected by accoucheurs, perhaps because it has not been thought of.

Much has lately been written on Vesico-Vaginal Fistulæ, and various instruments have been devised for the performance of this difficult and tedious operation, but we have seen none to surpass in simplicity, and apparently in effectiveness, those figured at pages 238–240, invented by Mr. Hilliard, instrument maker, Glasgow. This gentleman seems to have witnessed several of these operations, and observed the defective instruments used. The reader will be interested with his account. We are warned by Mr. Baker Brown not to operate for vesico-vaginal fistula when the woman is suckling. He relates a case which was apparently unsuccessful from this circumstance.

When a patient applies to you in a state of anæmia from repeated floodings, you naturally examine in expectation of finding a polypus. In many cases, however, there is not a polypus, so called, with a peduncle attaching it to the uterus, but a regular Fibrous Growth, with a broad attachment to the womb. In such a case it is necessary to *incise the os uteri*, so as to get at the tumour, and then to devitalise it, so to speak, by doing it considerable injury; the tumour then sloughs away to a considerable degree and preserves the woman from her repeated hemorrhages. Even simply incising the os uteri will often stop the hemorrhage without doing anything else, we suppose somewhat on the principle that, in cases of placenta previa, to separate this

body by means of the finger from the movements and dilatations of the os, the severe flooding of such cases is often arrested. Mr. Baker Brown not only incises or slits the os uteri, but *gouges* pieces out of the tumour or breaks it up with scissors. It will, afterwards, be likely to come away in lumps with offensive discharges. We suspect that in some of the unsuccessful cases the peritoneum may have been wounded, or the discharges may not have had free vent downwards, so that purulent accumulation may have occurred. It will be found that you may even wound the peritoneum, if the orifice be at the most dependent part, so that accumulations may find an easy exit. In one of Mr. Brown's cases the womb was retroverted, and the tumour could not be reached from the os; he therefore made an incision in the posterior wall of the womb, which was very thin, and carefully carried the incision up to the os and towards the fundus, when the fibrous mass could be seen and felt. It was then freely *gouged* three times, at intervals of a few days. The result is not reported, but was expected to be favourable. At page 245 the reader will find Mr. Harper's instrument represented, for what he calls *coring* these tumours. It seems to us that the great danger in these operations will be in implicating the peritoneum. You may see a womb injured a good deal at its os and cervix, but as soon as the injury reaches the anterior or posterior prolongation of the peritoneum, it becomes highly dangerous. These cases show that we may do a good deal towards getting rid of tumours of the womb by opening into them from the outside of the womb, instead of from the inside, *provided we keep low down and avoid the peritoneum*. A case which illustrates in some measure these remarks is published by Dr. Herbert Barker, in which the os and cervix uteri were torn off by the child's head, and came away entire, the child escaping from the uterine cavity without passing through the os uteri at all. The os even remained undilated, and the woman did well.

Another way of removing Fibrous Growths is by what is called the process of *Enucleation*. But in these cases you can get at the substance from *within* the womb; in those related by Mr. Brown you can only do it from the *outside*. Dr. Davis gives us a very good instance of enucleation and removal of a fibrous tumour of the womb. He succeeded first in detaching or enucleating the tumour from below upwards, so as to separate a good portion from the surface of the womb. By this process he seems to have made it resemble a pedunculated polypus, and then proceeded to detach it more completely by dragging down the mass so as to get it partly into the vagina. He carefully examined that he was not at the same time inverting the uterus, which, by the by, we should not be so afraid of doing in such a case, as the displacement would not be difficult to rectify. By a combination of traction and twisting of the tumour close to its adhesion he succeeded in removing it. These cases show what ill-usage the womb will bear without injury to the woman; and this remark is exemplified in other cases mentioned in the Obstetrical Transactions, where the womb was severely used without any ill consequences resulting. But the apparent violence in these cases of fibrous tumours and polypi is necessary, and, in the hands of an experienced man it may be done safely. Tumours in the womb, such as polypi, may be dragged down carefully, so that the finger, or the scissors or scalpel, may be used with safety.

An interesting discussion took place at one of the meetings of the Obstetrical Society in London, in February last, respecting Ovariotomy. Considerable difference of opinion seems to exist amongst those who have practised this operation in London. The points of difference may, at first sight, seem trivial, but in fact they are very important: and we think that these apparently little things are not sufficiently noticed. Dr. Tyler Smith operates by making a small abdominal incision, tapping the cyst, and bringing it through the wound. The peduncle is well secured by a ligature, and this ligature is *allowed to fall into the pelvis*. Dr. Smith states that the only objection to thus allowing the ligature to fall into the pelvis is the length of time before it comes away. In the paper as reported in the Lancet, we found no mention made respecting the *kind* of ligature; we should consider that to

return a common ligature into the pelvis, where it will remain for a considerable period, causing inflammation of a more or less suppurative character, to be objectionable. If the metallic ligature could be used it would be less objectionable. The common ligature causes *suppurative*, the wire ligature causes *adhesive* inflammation. And when we know that the peduncle of these cysts is often composed of a substance of a very low degree of vitality, we can account for the long delay of the ligature in coming away, and can thus perceive its danger. It will sometimes be 20 or 30 days in coming away. It seems to us, therefore, the best to bring the peduncle *as near as convenient* to the external wound and then secure it. Mr. Wells strongly objects to the ligature being returned into the pelvis, and secures the peduncle as near to the abdominal wound as possible, objecting to Mr. Brown's use of the clamp, except as a temporary measure, while he secures the peduncle by ligature. Mr. Brown, on the other hand, prefers to secure the peduncle at the abdominal orifice by his *clamp*, and when this is too heavy, he uses a pair of common *carpenter's callipers*. But only when he can secure the peduncle *easily* in this situation; when it is too short to be thus easily secured he uses the ligature, like other operators. We must confess that this seems to us an excellent proceeding. If you can easily secure the peduncle and cause its adhesion to the abdominal parietes, it seems best to do so; but in cases of difficulty and length of peduncle you can readily have recourse to the ligature. Injecting the cysts with iodine is useless when the cysts are compound, and confidence is nearly lost in the iodine even when there is only one cyst. Those cases succeed best which have not been previously tapped repeatedly; repeated tappings disturb the parts, and injure the constitutional powers. It is interesting amidst all these operative proceedings to watch Nature in her own way of doing things. The surgeon taps the cyst from *above*, but Nature does not always do this; she often taps the cyst from *below*. We have read of several instances in which the cyst burst downwards, and a case is related in this volume in which the cyst burst into the rectum. After repeated tappings, and twice injecting with iodine without relief, the patient was sitting on the close-stool and felt "something hard pressing into the rectum." This was followed by a *snap* and gush of fluid; twenty-seven pints of thick fluid escaped per anum. Now, when we remember how easily we can feel the enlarged ovaria in the female through the rectum, we can suppose that in this case a fluctuating tumour might have been felt, which might have warranted its puncture. A large portion of the rectum, the lower parts of the womb and the bladder are, we all know, uncovered by peritoneum in the pelvis, and curious enough, the more they are enlarged the larger is the portion below uncovered by peritoneum. This would almost seem to invite the operating surgeon to look for a vent for the fluid *downwards* instead of above. We would recommend surgeons to study more carefully the pelvic fasciæ, and especially those points where the pelvic viscera may be punctured without wounding the peritoneum. Speaking, however, of the peritoneum, Dr. Simpson says in one of his lectures, partly published in this volume, that a probe may be pushed through the fundus uteri and peritoneum without danger. His words are, "although the sound may doubtless be passed into the peritoneal cavity with impunity, yet it is not an experiment one would willingly and wittingly repeat." We mention this simply to show that wounds of the peritoneum are not necessarily fatal, but must in some measure depend on whether the wound be made in such a position that all foreign matter may easily escape, and not collect in its cavity. If we could find a secure road into these ovarian cysts, from the rectum or vagina, we cannot but think that it would be an improvement on the present way of treating these cases. We remember a very interesting paper by Mr. Bainbrigge, of Liverpool, in *Retrospect*, vol. xv., p. 302, in which this able surgeon alludes to various modes of curing these ovarian cysts, and amongst others, he mentions how the cyst or cysts burst spontaneously through different parts, some through the abdominal walls at the umbilicus, some through an opening into the vagina, one into the rectum and vagina, one into the bladder, and one into the intestinal canal. He refers the reader

to these several cases in the different journals, and states that many more such might be found in which Nature herself was the surgeon. Even in our present volume, at p. 252, a case is related by Mr. Fox, of Weymouth, in which the ovarian cyst evidently burst into the bladder, and thus was spontaneously cured. Now that it is discovered that the fine exploring needle, or fine trochar and canula can be passed on their journey of discovery into most important parts, without injury, we may safely predicate that the time will come when these and other obstinate pelvic diseases will be subjected to operations from below rather than from above. The operation of ovariotomy seems to become more frequent and more successful. Its success depends on attention to many little things; at least these things appear trivial, but they are in fact most important. For example, these cases do better when the external wound is closed by passing *metallic sutures* or *hare-lip pins through the peritoneal edges of the wound.* One important property of the peritoneum, like other serous membranes, is to unite rapidly by adhesive inflammation, and we have often wondered why hospital surgeons did not take advantage of this in healing wounds of the intestines. Instead of carelessly returning a wounded intestine into the abdomen, or even allowing a mucous structure to be in contact with a serous structure, or, perhaps, in contact with cellular tissue or fat; if they would carefully unite by fine metallic sutures two wounded serous membranes, they would probably adhere rapidly. Dr. Tyler Smith says that in a case of ovariotomy, where the peritoneal surfaces had been carefully approximated by removing the metallic suture through the adjacent softer tissue, the serous membrane united, and arrested the suppurative inflammation which took place in the whole length of the wound in the abdominal walls. Had the peritoneum not thus arrested the advance of the suppuration, death must have ensued; whereas the patient recovered. Mr. Wells says that the metallic sutures may be passed through the peritoneal edges with safety, and adhesion will take place in three days, when they may be removed. This is a simple principle of surgery, but not hitherto sufficiently noticed nor carried out in treating wounded viscera covered by serous membrane.

For the Resuscitation of Still-born Infants our instruments are not good. A catheter, male or female, or any other such contrivance introduced into the trachea, or the mouth of the practitioner applied to that of the child, with other practices, have been resorted to, to inflate the child's lungs. One of the simplest or readiest methods is what we occasionally adopt, viz., covering the child's mouth with a fold or two of clean linen, pressing the larynx on the œsophagus, so as to prevent the air passing down it, and then applying the mouth to the mouth of the child and gently blowing down the trachea, taking care to inflate very gently, for fear of rupturing the tender air-cells. Then compress the chest so as to force out the air. The simple fact of forcing air *out* is even more important than forcing our own breath *in*, as it creates a kind of vacuum, which will be filled by the natural expansion of the parietes of the chest drawing fresh air in, even without any further effort of the practitioner. This operation, however, we think may be improved by adopting the suggestion of Dr. J. G. Wilson, of Glasgow, who uses a vulcanised india-rubber ball with a silver tube attached to it The silver tube is introduced into the trachea like a catheter, and the ball is then compressed with the palm of the hand, air being thus forced into the trachea. A small hole in the tube, within reach of the thumb, can be closed and opened at pleasure, so as to supply fresh air. Of course, you may supply any quantity of air, with this little instrument; it may therefore be useful to resuscitate drowned persons, and ought to be possessed by every medical practitioner.

The usual Obstetric Position in labour is not always the best in a tedious case where there is obstruction, or where the pains fail in power. Although the womb may be called an organic and involuntary muscle, yet it is influenced by the voluntary nerves, and may be assisted, like the rectum in defæcation, by the muscles of volition. This is often best done in the upright position. We call the attention of the profession to a paper by Mr. Hardy, of Hull,

in the Obstetrical Transactions, which we give at page 301. He shows that in difficult and tedious cases it is very useful to apply a suitable bandage to the abdomen, and place the woman between two chairs, a little separated, in the upright position; thus one side of the woman will be on one chair, and the other side on the other chair, he rthighs partly supporting her as well. In this position she can often *bear down* with more force, and the weight of the child may likewise assist a little by the simple power of gravitation. This reminds us that many years ago we perused a paper respecting the Chinese mode of delivery, which was by the woman sitting on the broad edge of a kind of tub, thus leaving the perineum and other parts perfectly at liberty to be stretched, and which position did actually facilitate the labour. Indeed we can imagine that, in cases where the delivery had to be accomplished without any assistance from the medical attendant, this position would be the best. It would, however, leave the woman unassisted in cases of flooding, and where there was any difficulty with the placenta.

The treatment of Menorrhagia depends on so many things, that it is often difficult; but the result is so seldom dangerous that we cannot agree with the extreme measures which are sometimes adopted to relieve it,—such as first dilating the os and cervix uteri, and then scraping off the internal membrane and injecting tincture of iodine. When the flooding is caused by one or more polypi in the womb, it is proper to dilate and get rid of them; but this can be done with comparative ease by dilating the parts and using the finger, or a pair of very small, but long curved scissors. These scissors, however, should be *curved at a right angle* (so to speak), and only for about one-eighth of an inch; the curve should indeed represent more than a semicircle, so that when introduced they will not catch hold of any contiguous structure, and when brought out again their points can be protected by the finger, or even by placing a small bit of cork upon them, or, if preferred, the points can be blunted. A large polypus can thus be slowly but surely divided by scissors of this description. Remember that when you introduce curved scissors of the usual description into a narrow os uteri, and then attempt to cut through the thick peduncle of a polypus, you don't know exactly in what direction you are cutting, but with a pair of scissors such as we describe, and with the forefinger of the left hand on the peduncle, you may cut away with safety; you know that you are cutting *towards* your finger, and not into the parieties of the womb itself. When the flooding is not owing to these polypi, or when you have extracted them, we need not *scrape off* the mucous membrane of the womb to cure the hemorrhage. If you like, you can *wipe out* the organ with weak nitrate of silver solution; but we seldom fail in checking the discharge, if *very severe*, by the common plug, made with a long bit of rag, well greased and fitted to the os uteri, and following this up with our various styptics, such as tannin, gallic acid, and the tinctura pinûs laricis, in half-drachm doses, every hour or two.

When a woman does not recover well after labour, and where some obscure or severe pain exists in the pelvis, look out for an abscess in consequence of Pelvic Cellulitis; examine both rectum and vagina carefully. The pus will frequently make its way on the front or back of the vagina, and even burst into the vagina itself. To the practised finger it will not be difficult, in this case, to find out the seat of mischief, or rather, where nature wishes for relief. If you are doubtful in such a case, you need not be afraid of using one of the finest exploring needles, as recommended by Dr. Simpson. This fine needle may be pushed into much more vital parts than the parietes of the vagina without incurring danger. A confirmation of these remarks will be found in Dr. Gibb's paper on pelvic cellulitis.

We must point attention to the recommendation of Assalini's Forceps, by Dr. Granville, and slightly modified by him, which serves the purposes of both the long and the short forceps. It is handy, easy of application, and requires no *cross-locking* of the blades. These are certainly great advantages in some cases, but not in others.

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## 1. LATERAL CURVATURE OF THE SPINE

Sample movement for lateral curvature to the right—expanding contracted (left) side, unbending spine, and pressure on projecting (right) shoulder.

Sample movement for lateral curvature to the right—contracting the expanding (right) side, unbending spine, and pressure on projecting (right) shoulder.

Is caused by *unequal action* of the spinal muscles, generally (but not always) accompanied by muscular weakness. Sound sense and experience prove that supporters, by preventing muscular action, increase the weakness and aggravate the disorder; while gymnastics, acting on all muscles alike, can, at most, only benefit the general health, but cannot correct relative disproportions of muscular strength. A CURE would consist in such *regulated* action of the muscles as, in accordance with the anatomy of the body and peculiarity of the deformity, would expand the contracted muscles on the shrunken side, and contract the expanded muscles on the projecting side, and, by introducing a series of muscular actions *opposite* that which produced the deformity, would thus re-establish a uniform and harmonious action of antagonist muscles, when the deformity would disappear. (See cuts.)

## 2. PARALYSIS

Is produced by a suspension of the nervous stimulus in the muscles by some cause affecting the nervous centres. The shock may have passed off, or the clot in the brain may have become absorbed, and the paralysis may still, wholly or in part, remain, because it requires a special effort to re-establish the connection of brain and muscles. In ordinary exercise, the *un*affected muscles perform the most of the action, while the paralyzed ones perform the least.

This process should be reversed, and the paralyzed muscles made to act while the unaffected parts are at rest. The nerves must be re-educated to perform their functions, by sustained, gentle, well-directed, and repeated efforts of the will on the affected muscles, till the latent power is developed to be an efficient one.

Sample movement for paralysis,—concentrating the WILL on the extensors of the leg, while the rest of the body is at rest.

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